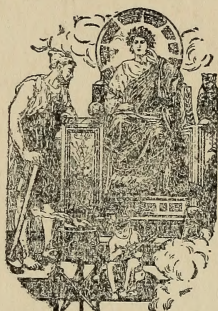


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AND
WILLIAM FRANCIS, F.L.S.

"Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster
vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit. lib. i. cap. l. Not.*

VOL. X.—SIXTH SERIES.
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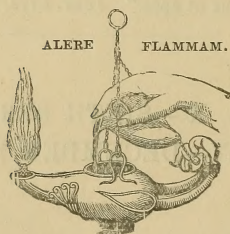
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“Meditationis est perscrutari occulta; contemplationis est admirari perspicua Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem.”—*Hugo de S. Victore.*

—“Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condant,
Quid toties diros cogat flagrare cometas,
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu.”

J. B. Pinelli ad Mazonium.



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THE

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AND

JOURNAL OF SCIENCE.

[SIXTH SERIES.]

JULY 1905.

- I. *Osmotic Experiments on Mixtures of Alcohol and Water*,
By P. S. BARLOW, B.Sc. (Vict.), St. John's College,
Cambridge*.

THE experimental work of this paper was undertaken by way of examining a curious and unexpected result obtained by S. U. Pickering† in an osmotic experiment with propyl alcohol. The most recent estimate of the importance of this experiment as bearing on the theory of the action of the membrane in osmotic phenomena is given by W. C. D. Whetham‡.

The following is a quotation taken from a letter written by Pickering to 'Nature,' dealing with the hydrate theory of solution§. It serves here the double purpose of describing his experiment and giving his own estimate of its value in supporting the hydrate theory. He writes:—"On the other (the hydrate) side we have two experiments, which would seem to be conclusive, but which the dissociationists have hitherto thought fit to ignore.

"Osmotic pressure, they hold, is due to the quasi-gaseous

* Communicated by Professor J. J. Thomson, F.R.S.

† *Ber. Deut. Chem. Ges.* xxiv. p. 3639 (1891), and "Solution," Art. ii., Watt's Dict. of Chem.

‡ Whetham, 'Theory of Solution,' pp. 96, 97.

§ *Nature*, lv. p. 223.

pressure of the solvent and dissolved substance acting on a diaphragm, which, being permeable to the solvent only, renders the pressure of the dissolved substance inoperative, and hence causes the total operative pressure of the solution to be that of only the solvent present in it. Now, I have shown that if we take a solution such as that of propyl alcohol in water, and place it in a semipermeable vessel surrounded by water, the latter will pass through towards the solution, *ergo*, the vessel is permeable to water but impermeable to the alcohol; but if the same vessel with its same contents is surrounded by propyl alcohol, it is the alcohol that passes through towards the solution, *ergo*, the vessel is permeable to the alcohol, but not to the water The obvious conclusion to draw from this experiment is, that it is the solution, and not either of the substances separately, to which the membrane is impermeable, and this is just what we should anticipate on the hydrate theory, the molecules of hydrates being necessarily larger than those of their constituents." The experiment is thus accepted as a 'crucial' one.

The experimental work, of which a short account is given in this paper, shows that the observation of an increase in the osmotic pressure after the cell is placed in the alcohol is correct, but that it is not due to an inflow of alcohol. Had more time been given to the experiment, it would have been found that this increase is not permanent as when the cell is in water. The permanent result is an outflow from the cell into the alcohol. And further: had the cell containing the solution been placed by Pickering again in water, he would most likely have found a decrease in pressure; and obviously the outflow causing it would have been the reverse of the anticipation based on the hydrate theory as above quoted. The explanation of what are here called "temporary" rise and fall (or inflow and outflow) is given after some account of the experiments.

The Cells.

It is not intended to give here a full account of the preparation of the cells. This was by far the most tedious part of the work. That some cells should prove useful and others prove failures, after exactly the same treatment, seems only to be accounted for by the presence of flaws within the pot. The membrane used was that of copper ferrocyanide obtained by diffusion. Of the thirty-one cells used in the preparation, fifteen proved sound. Of the others, their failure

was generally due to a fault within the cell-wall, but not always; sometimes the leak was through the glass at the joint being strained or through the joint itself.

The cells were very hard and close-grained, having walls between .4 cm. and .5 cm. thick. Their internal length was about 6.5 cm., and their internal diameter 1.6 cm. A few cells of more porous clay and slightly thinner walls, kindly given by Mr. Whetham, were also used and proved very serviceable.

The cell was prepared by washing in dilute alkali and then in dilute hydrochloric acid. It was then washed and boiled in several changes of distilled water and allowed to become nearly dry. Close-fitting glass tubing was ground into the end of the cell by means of wet emery-powder, and was fixed by litharge and glycerine cement. This hardens to a mass apparently unaffected by alcohol. This cement is rather porous, and any flaw in it may make it impossible to obtain a sound membrane. There is always the same risk of failure due to want of uniformity in the cells themselves. The joint obtained by using sealing-wax is more suitable for formation of the membrane, but had to be abandoned on account of the ease with which it is attacked by alcohol. Of the cells with sealing-wax joints a much larger percentage proved sound.

The air in the pores of the cell was driven out by means of electric osmose and afterwards by placing under the water-pump and drawing freshly boiled distilled water, still slightly warm, through the cell-walls. Its own volume of water was drawn through several times. In the case of the more porous cells, drawing water through in this way seemed sufficient. The cell was then placed in copper-sulphate solution and was filled with potassium ferrocyanide solution. With the harder cells, ten to fourteen days in these solutions seemed necessary for the formation of a useful membrane. The cell was then fitted with a gauge and tested by a sugar solution of known strength.

In the gauge attachments there were two T-pieces arranged in H form. Below one vertical portion was the cell; below the other the gauge was fixed. In this way the cell and gauge could readily be washed out without detaching the gauge. The opening immediately above the cell was closed by thick-walled rubber tubing wired over a flange on the glass, and a screw-clip. The opening above the gauge was closed by fusing the drawn-out glass. The rubber tubing made it possible to add pressure by means of a pump, and to

relieve slowly any pressure set up during an experiment. The volume of the air enclosed within the cell was very small, so that any change in the volume of the cell's contents immediately moved the gauge-level. The bore of the gauge-tubing was small, in the largest having a volume of $\cdot 0358$ c.c. per cm. length.

The Experiments.

In the experiments with the alcohol solutions, no attempt was made to get the actual values of the osmotic pressures. In most cases, the solutions were too strong and generally the pressure was not allowed to reach values greater than those for which the cell had previously been tested, thus keeping the experiment within the known limits of the cell's strength. However, in more than one case, a cell left overnight got up a pressure large enough to burst the rubber tubing or force it over the flange, below which it was wired. The cells still remained sound. What pressure the best cells could bear before breaking down is not known; and for obvious reasons it has not been sought.

Experiments were performed with varying strengths of solutions, going up from 15 per cent. alcohol by steps of 5 per cent. to 70 per cent. Strengths below and above these limits were also used, all with the same result. This gradual increase of the strength of the solution was thought to be advisable, because it seemed not unlikely that when the alcohol was in great excess the sign of the osmotic current might change. In other words, the alcohol might become the solvent and the water the solute. This, however, was not found even when the water was present in very small quantity indeed.

In these experiments, where water was always present, the alcohols were not further rectified. They were supplied to the laboratory as pure and so used. The specific gravity (compared to water at 4° C.) of the ethyl alcohol was $\cdot 7948$ at 16° C.; that of the methyl alcohol, $\cdot 7940$ at 18° C.; that of the propyl alcohol, $\cdot 8274$ at $14^{\circ}\cdot 8$ C. This last was taken some time after the experiments were completed, and is therefore likely to be much higher than when first used.

On removing from one liquid to another, the cells were dried by folding in filter-paper.

TABLE I.—CELL β . Ethyl-alcohol solution. Strength for maximum contraction.

	Time.	Gauge Readings (mm. scale).	
Cell in water.	Aug. 13. 12.50	open 290.8 closed 291.5	
	Aug. 14. 10.0	428	Internal pressure [nearly 3 atmos.
Placed in alcohol.	10.20	open 289 closed 290.5	
	10.30	299	
	10.50	310.5	
	11.8	311	Max. of temporary [rise.
	11.20	308.5	
	11.45	300.5	
	12.25	283	
		open 288.5	} Fall in pressure.
Placed in water.	12.30	closed 288.5	
	12.36	287	
	12.45	283	} Falling.
	3.0	273	
	4.20	315	Steady rise.
Placed in alcohol without relieving pressure.	4.30	323	
	4.35	327	
	5.3	336	Max. of temporary [rise.
	5.36	334	
	8.10	285.5	
		open 288	} Permanent fall.

TABLE II.—CELL XXIV. Solution of Propyl Alcohol:
60 c.c. of alcohol in 100 c.c. of solution.

	Time.	Gauge Readings	
Cell in water.	Monday 12.35	closed 268	
	2.20	303	
	4.20	349.4	Steady normal rise.
Opened and placed in propyl alcohol.	4.28	closed 271.6	
	4.34	276.4	Rising.
	5.17	300	
	8.0	269	Falling.
	Tuesday A.M.	Beyond the bend of the gauge-tube	Showing consider- [able outflow.
Placed in water.	9.45	274.5	
	11.20	270.2	} Temporary fall.
	12.10	270	
	5.5	330	
	5.20	336.2	
		open 277	} Rapidly rising.

TABLE III.

CELL XXIV.—The cell was first soaked in methyl alcohol.

The solution consisted of 49 c.c. of methyl alcohol made up to 50 c.c. with water: *i. e.*, nearly 24 molecules of alcohol to 1 of water.

	Time.	Gauge Readings	
Cell in methylalcohol	Monday 5.0	open 126.4 closed 126.9	Immediate per- [manent fall.
	5.17	122.4	
	5.55	120	
	Tuesday 11.30	101.6	
		open 125 }	
Placed in water.			Temporary fall.
	4.32	126	
	4.38	124.8	
	5.0	123.9	
	5.46	126	
	6.22	130	Permanent [rise. normal
	Wednesday A.M.	250	

TABLE IV.

CELL XII.—In this case the solution was outside the cell and alcohol inside.

Solution of ethyl alcohol: 60 grs. alcohol in 100 c.c. solution.

	Time.	Gauge Readings.	
Cell standing in the solution.	Thursday 12.20	289	Shows permanent rise and inflow from the solution.
	4.35	292.6	
	Friday 10.0	305.5	
	Saturday 11.45	323	
	Monday 10.40	354	
	Tuesday 10.0	361	
	Wednesday 10.20	373	

After the above experiment, the alcohol was withdrawn and the cell was filled with water. This would give a weak solution of alcohol (but not very weak, owing to small capacity of cell) inside. During the next 24 hours there was a large decrease in the internal pressure, indicating a current from the weaker to the stronger solution.

The above tables of results are typical of those obtained from experiments with other solutions and with other cells. Considering the results in Table I., we find a rapid rise of pressure when the cell is surrounded by water. The normal rise of pressure is spoken of as "permanent" for the purposes of this paper.

When placed in alcohol, cell β showed an increase of pressure, the gauge rising through 20.5 mm. in 48 minutes. There was no further rise. The level then fell steadily. The above rise was "temporary": this fall is "permanent," that is, it would have continued until the liquids on both sides of the membrane had the same composition, or until further outflow was stopped by the difference between the internal and external pressures. That this outflow was permanent in this sense, and not temporary, was readily shown by repeatedly opening and closing the cell while it remained in the alcohol; there was always a fall of the gauge-reading. It was necessary to open the cell at intervals to avoid the mercury being sucked back beyond the bend of the gauge.

In this connexion the experiment of Table IV. is important. The arrangement in this case is the reverse of having the solution inside and the alcohol outside. The osmotic current, which in the latter was the permanent outflow, now becomes a steady inflow and may be allowed to show itself over a long interval of time. The rate of rise of pressure is much slower than in the case when pure water is outside, as would be expected. The results given in Table IV. are spread over a week. Obviously, if the current can only be from the single liquid to the mixture, as expressed in Pickering's account, then results like those of Table IV. would be impossible.

When the cell was placed in water after being removed from the alcohol, there was a temporary fall in all cases. In Table I. this fall continued for about three hours, and was succeeded by a steady increase of pressure. On being placed in alcohol again without relieving the internal pressure, there was a temporary rise as before; but this time the rise was less. The internal pressure opposed the temporary inflow.

The case in Table III. is slightly different. The cell was first soaked in the alcohol, and then the solution was added. The cell being in alcohol, there was no temporary rise; the permanent fall began at once. When placed in water, the temporary fall was that usually found.

All the experiments point to the conclusion that the water tends to cross the membrane in that direction which will promote its equal distribution on both sides.

This conclusion has been put to the following severe test.

A cell was soaked in methyl alcohol, the outer vessel being closed by a cork covered with melted wax. With changes of alcohol, both sides were obtained osmotically similar ; that is, the closed cell showed neither increase nor decrease of pressure. This being so, the cork of the outer vessel was fitted with a water manometer. The absorption of the water vapour by the alcohol caused a slow but steady increase of the internal pressure. In the case of ethyl alcohol exposed to the air of the laboratory, the same result was obtained. In these cases, it might have been thought on *a priori* grounds that the alcohol was in so very large excess, that it could have been regarded as dissolving the water. Experimentally, the water acted as the solvent (in the osmotic sense).

The explanation of the temporary effects is very simple. After the preparation of the cell in the ordinary way, the cell-wall is soaked with water. When the solution is within the cell, the membrane soon gets into what may be called the osmotic condition ; that is, the condition of having water at its outer surface, and solution at its inner surface. The normal inflow of water takes place.

When placed in alcohol, the outer face of the membrane remains in contact with water, until the alcohol diffuses through 2 or 3 mm. of the small pores of the pot. While this diffusion is going on, the water in contact with the membrane is passing into the cell under normal osmotic conditions. This gives the temporary rise. This inflow of the water between the outside alcohol and the membrane helps the alcohol to reach the membrane in a shorter time than would be required if the water within the pores were stationary. When the alcohol is in contact with the membrane, the fall in pressure begins. In support of this explanation there is the absence of any temporary rise in the experiment of Table III.

This explanation also accounts for the temporary fall when the cell is taken from the alcohol and is placed in water. Now there is a layer of alcohol in the pot between the membrane and the outside water. So long as this remains there is an outflow. When the water reaches the membrane, the permanent and normal rise of pressure begins.

If this way of accounting for these temporary effects be the true one, it follows that, if the cell remain in the water until the pressure set up is at its maximum value and be then placed in alcohol, there will be no temporary rise. The osmotic equilibrium will last so long as water is in contact with the outer face of the membrane ; and further, since there is now no inflow, the alcohol will have to diffuse through a layer of water whose thickness is not reduced by any passage

through the membrane as before. The interval of time between placing the cell in alcohol and the commencement of the fall of pressure will be greater than that in the ordinary experiments.

The results given in Tables V. and VI. show this very clearly.

TABLE V.

CELL XVI.—Solution of Ethyl Alcohol : 25 c.c. of alcohol in 100 c.c. solution.

Cell in water till the gauge-reading was constant (after 3 days).

	Time.	Gauge Readings.	
Cell in water. Cell dried and placed in alcohol without opening.	Friday 5.0	389	Maximum.
	5.2	389.2	The small rise due to handling the cell during the drying.
	5.7	389.1	
	5.17	389	
	For every 5 min.		
	until 6.12	389	
	7.55	389	
	9.0	388	
	Saturday 10.30	376	
	Monday 8.50	307	
Placed in water without opening.	11.15	296.8	
	12.30	290.3	
	5.15	268	Temporary fall.
	Tuesday 10.0	324	Permanent rise.
	open	221	

TABLE VI.

CELL XXIII.—Solution of Propyl Alcohol : 20 c.c. of alcohol in 100 c.c. of solution.

	Time.	Gauge Readings.	
Cell in water.	Tuesday 10.0	open 256	Pressure added by [pump.
		closed 382	
Placed in propyl alcohol.	1.15	419	
	Wednesday 10.0	465	
	3.20	465	Maximum.
	3.25	465	
	3.30	464.1	
	3.40	464.1	
	Every 10 min.		
	until 4.30	463.9	
	9.0	459	
	Thursday 9.30	353.6	
	10.0	347	Permanent fall.

When placed in water after the above there was a temporary fall of nearly 80 mm. succeeded by a large and steady rise.

In Table V. the time required for the permanent fall to set in is about four hours; in Table VI. probably less than four hours. In Table I. the fall began in a little more than half an hour; in Table II. the gauge had risen and fallen again below its starting point in less than four hours. In this way, therefore, the temporary effects seem satisfactorily accounted for, and the true explanation of Pickering's observations given.

These experiments have shown that what was claimed as a "crucial" experiment in support of the hydrate (as distinguished from the dissociation) theory of solution can no longer be claimed as such. Nevertheless it would not be a justifiable inference to conclude that therefore a hydrate theory of solution is untenable. A large amount of work has been done on the properties of solutions of various solvents—among other workers mention may be made of Fitzpatrick*, Lincoln†, Schlundt‡, H. C. Jones and Carroll§,—and all goes to show that the influence of the solvent is considerable and that some kind of complex of solvent and solute is formed. Some kind of "hydrate" theory seems necessary; but in this sense it must not be regarded as excluding dissociation.

It may justly be concluded that these experiments prove that the part played by the membrane in osmotic phenomena is not a sieve-like one. For in the experiments where the outside alcohol got its water by absorbing the aqueous vapour in the air above it, the ratio of the alcohol molecules to the water molecules must have been very large; hence it is very probable that each water molecule would be the centre of an aggregate of alcohol molecules, which is carried along with it. Everything really seems to be in favour of the pure alcohol getting through to produce a current from within outwards. For if we consider the alcohol within the cell, it has none of its molecules hampered by the aggregating influence of water molecules; and therefore on any theory accepting a quasi-gaseous pressure exerted by the liquids, there would be a larger number of effective alcohol molecules inside the cell. This would lead us to anticipate an outflow of alcohol from the cell; a decrease, instead of an increase,

* Phil. Mag. xxiv. p. 377 (Nov. 1887).

† *Journ. Phys. Chem.* iii. p. 457.

‡ *Ibid.* vi. p. 159.

§ Amer. Chem. Journ. xxxii. 6 Dec. 1904, p. 521.

of the pressure. In support of this there is the long-known fact that a copper ferrocyanide membrane is permeable to alcohol, though not in an osmotic sense*; and from experiments recently performed by the writer, there is evidence that, under mechanical pressure, alcohol gets through the membrane more easily than water. This removes the argument based on the larger molecular volume of alcohol, which might be put forward in favour of a sieve-like action. However, with all this in support of the alcohol passing outwards, we find an increase of the internal pressure; the water, though present in so small quantity, gets through the membrane.

Any form of hydrate theory assumes an attraction between the two kinds of molecules. This attraction is the result of what has been called "residual affinity" †, a conception since revived by Lodge ‡ and Traube §. Its origin is most likely "chemical," and its action must be one of the causes of which osmotic pressure is the effect; most probably it is *the* cause. In consequence of this attraction there is mutual potential energy between the solvent and the solution. This tends to reach a minimum, and in doing so under the restrictions of an osmotic experiment, an opposing pressure is set up which hinders dilution and the consequent reduction of potential energy of solution.

I. Traube || has very recently put forward the theory that difference between the surface-tensions of the solvent and solution is the cause of the osmotic current; and that the latter will be from the liquid of weaker surface-tension. In connexion with this there is the experiment at the end of Table IV., where the current was from the weaker solution: that is, from the one with the greater surface-tension. In the same way, many similar experiments both with copper-ferrocyanide membrane and animal-parchment membranes are not in agreement with this theory. The only method of finally testing such a theory would be to use a membrane equally permeable in an osmotic sense to both liquids of the mixtures. The existence of such a membrane seems highly improbable.

In any adequate theory of osmotic pressure, the part played by the membrane must be taken into account. This necessary part is that the membrane must absorb that liquid

* Tamman, *Ann. Phys. Chem.* [2] xxxiv. p. 309.

† Pickering, *Ber. Deut. Chem. Ges.* xxiv. p. 3629.

‡ 'Nature,' lxx. p. 176.

§ *Phil. Mag.* viii. (Aug. 1904) p. 162.

|| *Phil. Mag.* viii. (Dec. 1904) p. 708.

which, in going through, forms the osmotic current *. On this point the work of Nernst † and Flusin ‡ may be referred to. During the last year the writer has been working on the osmotic pressure of ethyl-alcohol solutions with different membranes. One experiment may be mentioned here (it is not put forward as original). Pure alcohol and pure water were separated by a gutta-percha tissue, the water being within the cell. A large pressure was set up, showing an inflow from the alcohol to the water. With a copper-ferrocyanide membrane the direction is reversed. The cause of the current is the same in each case, namely, the mutual potential energy of solution of the liquids; the direction of the current is conditioned by the membrane.

The experimental work of this paper was carried out at the Cavendish Laboratory. I wish to take this opportunity of thanking Prof. J. J. Thomson for his kindness during the progress of the work.

II. *The Effect of a Surface-Film in Total Reflexion.* By ROBERT A. HOUSTOUN, M.A., B.Sc., Glasgow University 1851 Exhibition Scholar §||.

IT is well known that Fresnel's Laws for determining the phase and amplitude of a reflected or refracted light-wave from those of the incident wave have not been found to give the exact experimental result, when investigated with the most accurate experimental means. There is a difference between the calculated and observed results, greater than the experimental error. It is assumed in deriving Fresnel's laws, that at the surface where reflexion takes place the index of refraction changes discontinuously in going from the one medium to the other. The deviation from Fresnel's laws has been satisfactorily explained on the assumption that there is a "surface-film" between the two media. This surface-film can be regarded as a transition-layer, in which the index of refraction changes continuously from the value it has in the

* Since this paper was written, a similar view has been arrived at by Prof. Kahlenburg (Phil. Mag. Feb. 1905, p. 228) on similar lines of work.

† *Zeit. Phys. Chem.* vi. p. 37 (1890).

‡ *Comptes Rendus*, cxxxi. p. 1308 (Dec. 31, 1900).

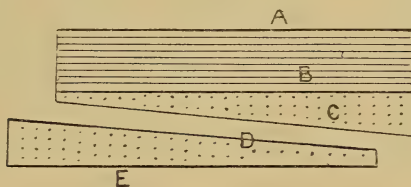
§ Communicated by Professor A. Gray, F.R.S.

|| "Wirkung einer Oberflächenschicht bei Total-reflexion." R. A. Houstoun, *Göttinger Nachrichten*, 1903, p. 352. "Ueber die Wirkung einer Oberflächenschicht bei Total-reflexion." R. A. Houstoun, *Phys. Zeitschrift*, vi. p. 208.

one medium to the value it has in the other ; or it can be regarded as a nearly homogeneous layer with a definite index of refraction, produced in polishing the surface. The formula for the deviation has been proved experimentally by Jamin* and Quincke† for the case of ordinary reflexion. This paper describes some observations made to prove the formula in the case of total reflexion, which was first given by Drude‡, though it follows naturally from the formula for the case of ordinary reflexion, which was found some years earlier. Fresnel's laws for the case of total reflexion were proved by Jamin§ and Quincke||, who worked with fresh surfaces for which the effect of the surface-film would be naturally very small.

The apparatus used was a spectroscope, the collimator of which was fitted with a polarizing nicol, and the telescope of which was fitted with a Soleil-Babinet compensator and an analysing nicol. A sodium-flame was the source of light. The incident light was polarized at 45° to the vertical ; after total reflexion it was once more made plane-polarized by the compensator, and then cut off by the crossed analyser.

The Soleil-Babinet compensator consists essentially of a quartz plate and two quartz wedges with equal angles. The



two wedges have their optical axes in the same direction, parallel or perpendicular to the edge of the wedge ; the wedges are parallel, but in opposite directions. The axis of the plate is perpendicular to the axes of the wedges. The relative phase-difference produced by reflexion &c. is compensated by moving the one quartz wedge relatively to the other wedge and the plate, which are fixed together.

The elementary theory disregards the reflected waves. It assumes also that the axes of the two wedges and the plate are

* Jamin, *Ann. de Phys. et Chim.* (3) vol. xxix. p. 263 (1850).

† Quincke, *Pogg. Ann.* vol. cxxviii. p. 355 (1866).

‡ P. Drude, *Wied. Ann.* vol. xliii. p. 146 (1891).

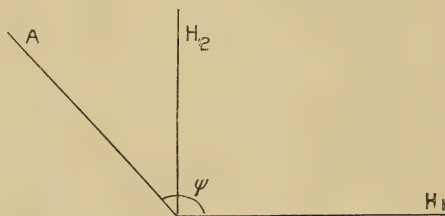
§ J. Jamin, *Ann. de Phys. et Chim.* (3) vol. xxx. p. 257 (1850).

|| G. Quincke, *Pogg. Ann.* vol. cxxvii. p. 217 (1866).

set exactly. This second cause of error was investigated theoretically, and does not seem to be an appreciable cause of error in the observations made. The first cause of error, the effect of inner reflexion, has been discussed by Prof. W. Voigt*. In the instrument used, the rays reflected on the surfaces C and D form separate images in the field of vision, and the intensity of the light reflected at B and again at A is too small to be taken into consideration. There remains only the light reflected on the surfaces E and A, the intensity of which is perhaps $\frac{1}{700}$ of the intensity of the light that comes direct through. As the distance between the surfaces A and E is great, and the light is not absolutely homogeneous, this error does not come into consideration.

In the elementary theory it is assumed that light is propagated in quartz in two waves, plane-polarized at right angles to one another. Prof. Voigt has, however, shown in a recent article†, that it is propagated in two elliptically polarized vibrations, the ellipses being long and thin, having their long axes perpendicular to one another, and being traversed in opposite directions. The effect of the ellipticity of the vibrations on observations made with the compensator was investigated, and a result in accordance with Prof. Voigt's theory obtained. The investigation is given here.

For simplicity we can consider the compensator as consisting of two plane parallel plates of quartz, with their axes H_1 and H_2 exactly at right angles to one another. Let us



suppose that the incident light consists of two vibrations, $E \sin T$ along H_1 , and $E' \sin T'$ along H_2 . To take account of the elliptic propagation in the quartz we must replace these two linear vibrations by four elliptic vibrations, a right-handed and a left-handed vibration having their long axes in

* W. Voigt, *Wied. Ann.* vol. xxii. p. 236 (1884).

† W. Voigt, *Gött. Nach.* 1903, p. 155.

each of the directions H_1 and H_2 ; *i. e.* we must write

$$E(\{1 - \kappa^2\} \sin T + \kappa^2 \sin T) + E'(\kappa \cos T' - \kappa \cos T') \text{ in the direction } H_1,$$

and

$$E(\kappa \cos T - \kappa \cos T) + E'((1 - \kappa^2) \sin T' + \kappa^2 \sin T') \text{ in the direction } H_2.$$

κ is very small. The underlined terms are right-handed or negative ellipses. Let the right-handed vibrations receive the phase-retardation r_1 in passing through the first plate, and the left-handed vibrations the phase-retardation l_1 . Then, if we disregard the κ^2 terms, after the first plate the two components are

$$E \sin (T - r_1) + E'(\kappa \cos (T' - l_1) - \kappa \cos (T' - r_1))$$

and

$$E(\kappa \cos (T - r_1) - \kappa \cos (T - l_1)) + E' \sin (T' - l_1).$$

These may be written

$$(E^2 + E'^2 \kappa^2)^{\frac{1}{2}} \sin \left(T - r_1 - \tan^{-1} \frac{\kappa E'}{E} \right) + E' \kappa \cos (T' - l_1), \quad (I.)$$

and

$$(E'^2 + E^2 \kappa^2)^{\frac{1}{2}} \sin \left(T' - l_1 - \tan^{-1} \frac{\kappa E}{E'} \right) + E \kappa \cos (T - r_1). \quad (II.)$$

Let r_2 and l_2 be the corresponding phase-retardations produced by the second plate. Then, if we consider only that portion of the phase-retardation which varies with the thickness of the plate, we shall have, after the second plate, in each of the directions H_1 and H_2 , four vibrations with the phase-retardations

$$r_1 + r_2, \quad r_1 + l_2, \quad l_1 + r_2, \quad \text{and} \quad l_1 + l_2.$$

Hence we shall have vibrations with those phase-retardations in the principal plane of the analysing nicol. In the expression for the resultant intensity there will occur a number of terms, of the form

$$A \cos \{ (r_1 + r_2) - (l_1 + l_2) + a \} + B \cos \{ (r_1 + r_2) - (r_1 + l_2) + b \} \\ + C \cos \{ (r_1 + r_2) - (l_1 + r_2) + c \} + D \cos \{ (r_1 + l_2) - (l_1 + r_2) + d \} \\ + E \cos \{ (r_1 + l_2) - (l_1 + l_2) + e \} + F \cos \{ (l_1 + r_2) - (l_1 + l_2) + f \}.$$

Write $r_1 - l_1 = \delta_1$ and $r_2 - l_2 = -\delta_2$. Then we may write the six phases

$$\delta_1 - \delta_2 + a, \quad -\delta_2 + b, \quad \delta_1 + c, \quad \delta_1 + \delta_2 + d, \quad \delta_1 + e, \quad \text{and} \quad -\delta_2 + f.$$

As the one wedge is moved past the other, δ_2 changes ; and each of the six terms gives a similar system of maxima and minima. It is the system with the phase $\delta_1 - \delta_2 + \alpha$ on which the working of the instrument depends. Its intensity is $\frac{1}{\kappa}$ times the intensity of the others ; and the position of its minima is not appreciably affected by the other systems.

Hence we can disregard the other systems, *i. e.*, we can disregard the second terms in the expressions (I.) and (II.). On entering the second plate the first terms become

$$\begin{aligned} & (E^2 + E'^2 \kappa^2)^{\frac{1}{2}} \left\{ \underbrace{(1 - \kappa^2) \sin \left(T - r_1 - \tan^{-1} \frac{\kappa E'}{E} \right) + \kappa^2 \sin \left(T - r_1 - \tan^{-1} \frac{\kappa E'}{E} \right)}_{\text{and}} \right\} \\ & + (E'^2 + E^2 \kappa^2)^{\frac{1}{2}} \left\{ \kappa \cos \left(T' - l_1 - \tan^{-1} \frac{\kappa E}{E'} \right) - \kappa \cos \left(T' - l_1 - \tan^{-1} \frac{\kappa E}{E'} \right) \right\} \\ & \text{and} \\ & (E^2 + E'^2 \kappa^2)^{\frac{1}{2}} \left\{ \kappa \cos \left(T - r_1 - \tan^{-1} \frac{\kappa E'}{E} \right) - \kappa \cos \left(T - r_1 - \tan^{-1} \frac{\kappa E'}{E} \right) \right\} \\ & + (E'^2 + E^2 \kappa^2)^{\frac{1}{2}} \left\{ \underbrace{(1 - \kappa^2) \sin \left(T' - l_1 - \tan^{-1} \frac{\kappa E}{E'} \right) + \kappa^2 \sin \left(T' - l_1 - \tan^{-1} \frac{\kappa E}{E'} \right)}_{\text{and}} \right\} \end{aligned}$$

If we disregard the κ^2 terms, on leaving the second plate the two components are

$$\begin{aligned} & (E^2 + E'^2 \kappa^2)^{\frac{1}{2}} \sin \left(T - r_1 - r_2 - \tan^{-1} \frac{\kappa E'}{E} \right) + (E'^2 + E^2 \kappa^2)^{\frac{1}{2}} \\ & \left\{ \kappa \cos \left(T' - l_1 - l_2 - \tan^{-1} \frac{\kappa E}{E'} \right) - \kappa \cos \left(T' - l_1 - r_2 - \tan^{-1} \frac{\kappa E}{E'} \right) \right\} \\ & \text{and} \\ & (E^2 + E'^2 \kappa^2)^{\frac{1}{2}} \left\{ \kappa \cos \left(T - r_1 - r_2 - \tan^{-1} \frac{\kappa E'}{E} \right) - \kappa \cos \left(T - r_1 - l_2 - \tan^{-1} \frac{\kappa E'}{E} \right) \right\} \\ & + (E'^2 + E^2 \kappa^2)^{\frac{1}{2}} \sin \left(T' - l_1 - l_2 - \tan^{-1} \frac{\kappa E}{E'} \right). \end{aligned}$$

We can disregard the third term in the first component and the second term in the second component for the same reason that the second terms were disregarded above. If we multiply the first component by $\cos \psi$ and the second by $\sin \psi$ and add, we get the vibration in the principal plane of the analysing nicol

$$\begin{aligned} & \frac{1}{2} (E^2 + E'^2 \kappa^2) \cos^2 \psi + E^2 \kappa^2 \sin^2 \psi \left\{ \frac{1}{2} \sin \left(T - r_1 - r_2 - \tan^{-1} \frac{\kappa E'}{E} + \tan^{-1} \kappa \tan \psi \right) \right. \\ & \left. + \frac{1}{2} (E'^2 + E^2 \kappa^2) \sin^2 \psi + E'^2 \kappa^2 \cos^2 \psi \right\}^{\frac{1}{2}} \sin \left(T' - l_1 - l_2 - \tan^{-1} \frac{\kappa E}{E'} + \tan^{-1} \kappa \cot \psi \right) \end{aligned}$$

The resultant intensity is therefore

$$\begin{aligned} & (E^2 + 2E'^2\kappa^2) \cos^2 \psi + (E'^2 + 2E^2\kappa^2) \sin^2 \psi \\ & + 2 \left\{ (E^2 + E'^2\kappa^2) \cos^2 \psi + E^2\kappa^2 \sin^2 \psi \right\}^{\frac{1}{2}} \left\{ E'^2 + E^2\kappa^2 \right\} \sin^2 \psi + E'^2\kappa^2 \cos^2 \psi \left\{ \right. \\ & \times \cos \left\{ T - T' - (r_1 + r_2 - l_1 - l_2) - \tan^{-1} \frac{\kappa E'}{E} + \tan^{-1} \frac{\kappa E}{E'} \right. \\ & \left. \left. + \tan^{-1} \kappa \tan \psi - \tan^{-1} \kappa \cot \psi \right\} \right\}. \end{aligned}$$

The extinction position of the compensator is therefore given by

$$\begin{aligned} T - T' - (r_1 + r_2 - l_1 - l_2) - \tan^{-1} \frac{\kappa E'}{E} + \tan^{-1} \frac{\kappa E}{E'} \\ + \tan^{-1} \kappa \tan \psi - \tan^{-1} \kappa \cot \psi = 0. \end{aligned}$$

It deviates from the position which we should obtain if the elliptic propagation of light in quartz were disregarded, by

$$-\tan^{-1} \frac{\kappa E'}{E} + \tan^{-1} \frac{\kappa E}{E'} + \tan^{-1} \kappa \tan \psi - \tan^{-1} \kappa \cot \psi,$$

or

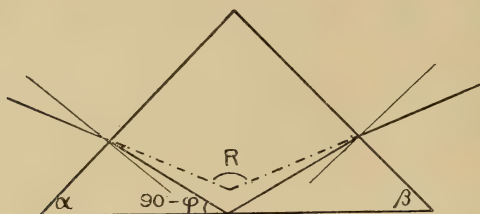
$$-2 \tan^{-1} 2\kappa \cot 2\psi,$$

since for the extinction position $\tan \psi \doteq \frac{E}{E'}$. If $E = E'$, as is the case with observations on total reflexion, then the deviation is zero; and this cause does not affect the accuracy of the results.

Observations were made on this deviation. The zero of the compensator was determined for different positions of the crossed nicols, *i. e.* for different values of ψ . ψ and κ were determined from the observations. κ was found to be 0.0016 as against values ranging from 0.0019 to 0.0012 given by Voigt in the above-mentioned article. κ was then substituted in the formula, and the theoretical value of the deviation calculated. κ is the ratio of the axes of the ellipse in which the light vibrates. It is very difficult to take readings for values of ψ near 0° or 90° .

ψ .	Observed Deviation.	Mean Error.	Theoretical Deviation.	ψ .	Observed Deviation.	Mean Error.	Theoretical Deviation.
88 ...	8.7	1.7	5.2	55 ...	-0.2	0.2	0.1
86 ...	2.2	0.4	2.6	40 ...	-0.0	0.0	0.0
83 ...	1.2	0.4	1.4	25 ...	-0.4	0.2	-0.3
78 ...	1.0	0.2	0.8	15 ...	-0.6	0.0	-0.7
75 ...	0.9	0.2	0.6	5 ...	-2.8	0.2	-2.0
65 ...	0.0	0.2	0.3	2 ...	-5.4	0.8	-5.6

The first observations were made on a very old prism, the surface-films of which must have grown through age. The incident light was polarized at an angle of 45° to the plane of incidence.



ϕ , the angle of incidence, was calculated from R , the angle between the directions of the telescope and collimator, and n the index of refraction, by the following formula:

$$R = \alpha + \beta + \sin^{-1} (n \sin \{\phi - \alpha\}) + \sin^{-1} (n \sin \{\phi - \beta\}).$$

Observations were made for a number of angles, starting from the limiting angle of total reflexion. After some unsuccessful attempts, two good series of observations were obtained, —ten different angles and ten observations to each angle. Before the experiment the prism was left all night in alcohol, then washed thoroughly with water and dried with a linen cloth. It was placed in such a position upon the spectrometer-table, that when it was rotated the light always fell upon the same part of the hypotenuse surface during the same series of observations. The two series of observations given were made for light that was totally reflected on different parts of the surface.

Δ is the observed phase-difference. In the next column to Δ stands the mean error to give an idea of the accuracy of the observations. Δ_0 is the phase-difference calculated according to Fresnel's formula.

Fresnel's formula is

$$\tan \frac{1}{2} \Delta_0 = - \frac{\sqrt{\sin^2 \phi - \frac{1}{n^2}}}{\sin \phi \tan \phi}.$$

The formula given by Drude is

$$\tan \frac{1}{2} \Delta' = \tan \frac{1}{2} \Delta_0 - \frac{\epsilon}{n} \frac{1}{\sin \phi \tan \phi}.$$

TABLE OF OBSERVED AND CALCULATED VALUES.

Index of Refraction of Prism, $n=1.619$.Angles of Prism $\begin{cases} \alpha = 45^\circ 0' 15'' \\ \beta = 45^\circ 5' 5'' \end{cases}$.

ϕ .	R.	Δ_0 .	I.					II.				
			Δ .	Mean Error.	$\Delta_0 - \Delta$.	Δ' .	$\Delta' - \Delta$.	Δ .	Mean Error.	$\Delta_0 - \Delta$.	Δ' .	$\Delta' - \Delta$.
38.21	67.87	7.2	4.0	0.6	3.2	4.1	+0.1	4.0	0.3	3.2	4.5	+0.5
39.96	73.57	36.1	33.6	0.6	2.5	33.5	-0.1	33.7	0.2	2.4	33.7	0.0
41.98	80.17	46.1	41.8	0.5	4.3	43.9	+2.1	44.2	0.3	1.9	43.5	-0.7
44.03	86.83	50.7	49.1	0.5	1.6	48.7	-0.4	48.9	0.3	1.8	48.9	0.0
46.09	93.48	52.7	51.9	0.4	0.8	51.1	-0.8	50.7	0.2	2.0	51.1	+0.4
47.94	99.47	53.2	51.9	0.4	1.3	51.7	-0.2	51.9	0.3	1.3	51.8	-0.1
49.97	106.07	52.8	51.5	0.6	1.3	51.6	+0.1	51.3	0.5	1.5	51.6	+0.3
51.95	112.57	51.8	50.3	0.5	1.5	50.6	+0.3	50.5	0.4	1.3	50.6	+0.1
53.97	119.17	50.3	48.9	0.4	1.4	49.2	+0.3	49.3	0.3	1.0	49.2	-0.1
55.95	125.77	48.5	47.9	0.2	0.6	47.4	-0.5	48.5	0.8	0.0	47.5	-1.0

$$\frac{\epsilon}{n} = 0.0133,$$

$$\frac{\epsilon}{n} = 0.0162.$$

The values of ϕ and Δ_0 were substituted in the latter formula. Then Δ was substituted for Δ' , and values were obtained for ϵ . The arithmetic mean of these values was then substituted in the second formula, and Δ' calculated for each of the two series of observations.

The effect of the surface-film is very evident.

We have two values for $\frac{\epsilon}{n}$, 0.0133 and 0.0126. The mean is

$$\frac{\epsilon}{n} = 0.0130.$$

Now ϵ is defined by the formula

$$\epsilon = \frac{2\pi}{\tau\lambda} \frac{n_1^2 n_0^3}{n_1^2 - n_0^2} \int_0^L dl \left(\frac{1}{n_1^2} - \frac{1}{n_l^2} \right) \left(\frac{1}{n_l^2} - \frac{1}{n_0^2} \right) n_l^2,$$

where L is the thickness of the surface-film, n_l the index of refraction at the depth l , n_0 at the depth 0, and n_1 at the depth L . λ is the wave-length of the light *in vacuo*.

The fact that ϵ is positive shows that the mean index of refraction of the film lies between that of the glass and that of the air.

Let us assume that

$$n_l = 1.309_5 + 0.309_5 \cos \frac{\pi l}{L}.$$

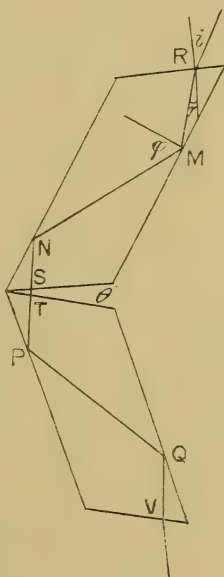
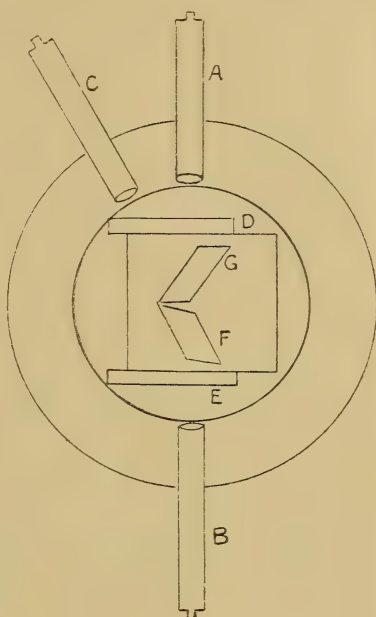
This satisfies the boundary conditions $n_0 = 1.619$, and $n_1 = 1$, and goes continuously from the one value to the other. Then L is found to be $= 0.028\lambda$, where λ is the wave-length of the D-lines. In the article of Drude referred to above, a lower limit is found for the thickness of the surface, from mathematical considerations alone. In the case of glass with the index of refraction 1.714 it is given as $L = 0.0103\lambda$.

The effect of the surface-film is small, and hence difficult to investigate. If the light were several times totally reflected, the effect of the surface-film would be multiplied, and I expected to be able to measure it more accurately. The difficulty was to devise an experimental arrangement which did not call for special apparatus. The diagram shows how this was done.

A pair of Fresnel's rhombs was used. They had been in the laboratory a long time, and had thus developed a good surface-film. The rays of light were totally reflected at M, N, P, and Q, and thus suffered neither a displacement nor an alteration of direction. The collimator A

and telescope B remained fixed and opposite to one another during the whole experiment. According to the theory, the phase-differences produced by the surface-film at R and V, and at S and T cancel one another. The index of refraction of the glass was determined by the method of minimum deviation for each of the four acute angles. The result was 1.5195 ± 2 . The sides were not exactly parallel, the greatest error being as much as $5'$. To simplify the calculation, the rhombs were considered perfect, the angles being $54^\circ 40'$ and $125^\circ 20'$. To avoid the possibility of error from this approximation, no measurements were made on the first part of the curve, where the phase-difference increases rapidly with the angle of incidence.

The rhombs were placed on a platform that slid in rails on the spectrometer-table. The table could be rotated relatively to the telescopes, and its position read on a scale. The platform was part of the original equipment of the instrument. To determine the angle of incidence and place the rhombs symmetrically, the auxiliary telescope C was used. C remained always fixed, and the angle between A and C was determined



by fitting C with a Gauss ocular, and moving B until the cross-wires in C coincided with its cross-wires. The angle

between the two positions of B, which could be read on the scale, was the angle between the directions of A and C. This angle was $41^{\circ} 47'$.

Before taking readings of the phase-difference, the table was rotated and the platform adjusted, so that the light from the collimator was reflected on a long side of the first rhomb, making the round opening of the collimator A coincide with the cross-wires of the telescope C. The position of the table was then noted. The same was then done for the second rhomb. If the readings of the scale were in these cases α and β , and the angle between A and C γ , then if the reading on the scale were $\frac{\alpha + \beta}{2}$, the bisector

of angle θ would bisect angle γ . Consequently, when the rhombs are in the correct position, the scale-reading must be $\frac{\pi + \alpha + \beta - \gamma}{2}$. The table was then placed correctly and the platform displaced until the light from the collimator fell on the most favourable part of the end surfaces.

To find the angle of incidence ϕ we proceed as follows.

In the position of symmetry $i = \frac{\theta}{2}$,

$$\sin i = n \sin r = n \sin (\phi - 54^{\circ} 40').$$

And $\theta = \pi - (\beta - \alpha) - 2(54^{\circ} 40')$: hence ϕ can be calculated.

Two series of observations are given in the table. Δ , Δ_0 , Δ' , and ϕ have the same meaning as before.

Observations on Fresnel's Rhombs.

I.					II.				
ϕ .	Δ .	Δ_0 .	$\Delta_0 - \Delta$.	$\Delta_0 - \Delta'$.	ϕ .	Δ .	Δ_0 .	$\Delta_0 - \Delta$.	$\Delta_0 - \Delta'$.
48.4	42.0	45.8	3.8	4.1	51.4	43.0	46.6	3.6	3.7
51.9	43.2	46.4	3.2	3.5	52.8	42.8	46.2	3.4	3.4
54.8	42.1	45.4	3.3	3.0	54.8	42.1	45.4	3.3	3.1
56.1	42.5	44.6	2.1	2.8	56.5	41.4	44.2	2.8	2.8
59.1	39.3	42.0	2.7	2.4	58.4	40.3	42.6	2.3	2.6
59.9	38.5	41.2	2.7	2.4	59.6	39.3	41.4	2.1	2.4
62.9	35.8	38.0	2.2	2.0	62.8	35.5	38.1	2.6	2.1

$$\frac{e}{n} = 0.035.$$

$$\frac{e}{n} = 0.036.$$

The accuracy does not seem to be very much greater than in the former case, when the light was only once totally reflected, probably because here the light was not always totally reflected on the same parts of the surface, and the thickness of the surface-film will vary from place to place.

In both the above cases the film was the natural surface-film, and not a homogeneous layer due to polishing, as the glass surfaces had not been polished for a very long time.

An attempt was then made to verify the theory further by depositing very thin artificial surface-films of collodion on the hypotenuse surface of a prism—which had previously shown no trace of such an effect. The artificial film should produce the same effect as the natural films. The films were made after the manner of Wiener, by putting two or three drops of a very dilute solution of collodion in ether and alcohol on the hypotenuse surface of the prism, pressing them down with a similar surface, until they filled all the space between, and then drawing the two surfaces apart. The ether and alcohol then evaporated, leaving a homogeneous skin of collodion.

The prism used was right-angled and isosceles, and the index of refraction for the D-lines was 1.513. The refractive index of collodion is greater than this; and so we had a uniform layer, the index of refraction of which did not lie between that of the glass and that of the air, and which gave an effect in the opposite direction to that produced by the natural film. The film was kept dry during the observations by placing a quantity of calcium chloride on the spectrometer-table. The presence of dampness was easily detected; it lowered the index of refraction of the film to below that of the glass, and we had a deviation from Fresnel's formula in the other direction.

Some trouble was experienced in getting a film of a suitable thickness; if the film is too thin, the effect is comparable with the errors of measurement, and the formula does not hold when it is too thick. The set of observations given, however, shows the effect of the film very well.

Collodion Film.

Limiting angle of total reflexion, $\phi = 41^\circ 22'$.

ϕ .	R.	Δ_0 .	Δ .	Mean Error.	$\Delta_0 - \Delta$.	Δ' .	$\Delta' - \Delta$.
42° 30'	82° 26'	25.4	22.2	0.1	3.2	21.6	-0.6
44 0	86 58	35.3	30.7	0.3	3.6	31.7	+1.0
45 30	91 31	40.5	37.2	0.4	3.3	37.0	-0.2
47 0	96 2	43.5	40.3	0.2	3.2	40.2	-0.1
48 30	100 36	45.2	42.4	0.2	2.8	42.2	-0.2
50 0	105 10	45.9	43.5	0.1	2.4	43.1	-0.4
51 30	109 44	46.1	43.4	0.2	2.7	43.5	+0.1
53 0	114 18	45.8	43.2	0.1	2.6	43.4	+0.2
54 30	118 54	45.2	43.0	0.3	2.2	42.9	-0.1
56 0	123 28	44.3	42.1	0.3	2.1	42.2	+0.1

$\epsilon = 0.029$.

ϕ , R, Δ_0 , Δ , and Δ' have the same meaning as before.

Attempts were then made to observe the effect of the collodion film in ordinary reflexion and refraction. In the first case the one component was too weak at the polarizing angle to give good results. The phase-difference produced by a surface-film in refraction is given by

$$\tan \Delta_d = -\epsilon \sin \phi \tan (\phi - \phi')^*,$$

where ϕ is the angle of incidence and ϕ' the angle of refraction. The following measurements were obtained:—

R.		ϕ .	Δ_d (air to glass).	Δ_d (glass to air).
154	0	30	0.7	-1.3
154	12	38	0.6	-0.3
153	22	46	0.8	-0.0
151	32	54	0.7	-1.1

Owing to some unknown circumstance the measurements from glass to air are not so accurate as those from air to glass. The theoretical value, obtained by using the value of ϵ already found from observations on total reflexion, is ± 0.6 .

III. *Total Reflexion at the Second Surface of a Thin Plane Parallel Plate.* By ROBERT A. HOUSTOUN, M.A., B.Sc., Glasgow University 1851 Exhibition Scholar †.

THE method used in the experiments described in the preceding paper was also employed to investigate total reflexion at the second surface of a thin plane parallel plate. A prism, the same as the prism used in the collodion experiment, was coated by Bortger's method with a thin homogeneous layer of silver on its hypotenuse surface. Sodium light, plane-polarized at an angle of 45° to the plane of incidence, entered the prism, was reflected by the silver layer on the hypotenuse, and the relative phase-difference produced between the components, parallel and perpendicular to the plane of incidence, was measured with the Babinet compensator and analysing nicol. The angle of incidence on the hypotenuse surface was usually such that the light would have been totally reflected, if there had been no silver film there. The silver film was then changed into silver iodide, and similar measurements were made.

Theoretically this is the case of the plane parallel plate, the thickness of which is small in comparison with the breadth of the incident light-wave, for the case that the three media

* P. Drude, Wied. *Ann.* vol. xliii. p. 145 (1891).

† Communicated by Professor A. Gray, F.R.S.

are all different, and that total reflexion takes place at the second face. The mathematics is rather complicated; the rigorous formula in the case of silver iodide was found capable of taking a relatively simple form and agreed perfectly with the experimental result. An approximate formula was used for the silver film. The necessary formulæ are given in Voigt's *Kompendium*, ii. p. 643 and the following pages, but must be considerably transformed. The formula for the silver iodide will be derived from first principles, as that can be done more shortly than is generally supposed by means of the reciprocal property of Maxwell's equations, and will be compared with experiment. Then the case of the silver films will be treated from the formulæ in the *Kompendium*.

When the wave in the glass falls on the film on the hypotenuse, it gives rise to a reflected wave in the glass, to a refracted wave in the film that is repeatedly reflected back and forwards in the film, and to a wave in the air beyond. We write down exponentials to represent those waves and substitute them in the boundary conditions. Consider the magnetic vector as the light vector. We can consider separately the components polarized in and perpendicular to the plane of incidence. Let us consider the component perpendicular to the plane of incidence. We can write the incident and reflected waves in the glass

$$E_1 e^{T_{e1}}; \quad R_1 e^{T_{r1}}.$$

Then summing all the waves in the film incident on the second surface, and all those reflected by it, we may write them

$$E_2 e^{T_{e2}}; \quad R_2 e^{T_{r2}}.$$

Let the wave in air be represented by

$$D e^{T_d},$$

where

$$T_{e1} = \frac{2\pi}{\tau} \left(t - \frac{x \sin \phi_1 + z \cos \phi_1}{v_1} \right), \text{ etc.}$$

τ is the period of the vibration, v_1 the velocity, and ϕ_1 the angle of incidence in the medium. The z axis is perpendicular to the surfaces and is measured positive from glass to air. Let l be the thickness of the film, μ the magnetic inductivity, and K the electric inductivity of the medium in question.

If H is the component of the magnetic vector perpendicular to the plane of incidence, the boundary conditions are that

H and $\frac{1}{K} \frac{dH}{dz}$ go through the boundaries $z = 0$ and $z = l$

continuously. Therefore we have

$$E_1 + R_1 = E_2 + R_2. \quad (1)$$

$$E_2 e^{-i \frac{2\pi l \cos \phi_2}{\tau v_2}} + R_2 e^{+i \frac{2\pi l \cos \phi_2}{\tau v_2}} = D e^{-i \frac{2\pi l \cos \phi_3}{\tau v_3}}. \quad (2)$$

$$(E_1 - R_1) \frac{\cos \phi_1}{K_1 v_1} = (E_2 - R_2) \frac{\cos \phi_2}{K_2 v_2}. \quad (3)$$

$$\left(E_2 e^{-i \frac{2\pi l \cos \phi_2}{\tau v_2}} - R_2 e^{+i \frac{2\pi l \cos \phi_2}{\tau v_2}} \right) \frac{\cos \phi_2}{K_2 v_2} = D e^{-i \frac{2\pi l \cos \phi_3}{\tau v_3}} \frac{\cos \phi_3}{K_3 v_3} \quad (4)$$

Put $L = \frac{2\pi l \cos \phi_2}{\tau v_2}$. $\cos \phi_3$ is imaginary.

Put $\tan \alpha = \frac{i \cos \phi_3}{\cos \phi_2} \frac{K_2 v_2}{K_3 v_3}$.

Then dividing (1) by (3) and (2) by (4),

$$\frac{E_1 + R_1}{E_1 - R_1} \frac{\cos \phi_2}{K_2 v_2} = \frac{E_2 + R_2}{E_2 - R_2} \frac{\cos \phi_1}{K_1 v_1},$$

$$\frac{E_2 e^{-iL} + R_2 e^{+iL}}{E_2 e^{-iL} - R_2 e^{+iL}} = i \cot \alpha.$$

Therefore

$$\begin{aligned} \frac{R_2 e^{iL}}{E_2 e^{-iL}} &= - \frac{1 - i \cot \alpha}{1 + i \cot \alpha} \\ &= \frac{\cos \alpha + i \sin \alpha}{\cos \alpha - i \sin \alpha} = e^{2ia}. \end{aligned}$$

Now

$$\frac{v_1^2}{\mu_1 K_1} = \frac{v_2^2}{\mu_2 K_2};$$

therefore

$$\begin{aligned} \frac{E_1 + R_1}{E_1 - R_1} &= \sqrt{\frac{K_2 \mu_1 \cos \phi_1}{K_1 \mu_2 \cos \phi_2}} \frac{1 + e^{2i(\alpha - L)}}{1 - e^{2i(\alpha - L)}} \\ &= i \sqrt{\frac{K_2 \mu_1 \cos \phi_1}{K_1 \mu_2 \cos \phi_2}} \cot(\alpha - L). \end{aligned}$$

Put $\frac{R_1}{E_1} = \rho_s e^{i\Delta_s}$.

$$\therefore -\tan \frac{\Delta_s}{2} = \sqrt{\frac{K_2 \mu_1 \cos \phi_1}{K_1 \mu_2 \cos \phi_2}} \cot(\alpha - L).$$

Δ_s is the absolute phase-difference produced in the component polarized at right angles to the plane of incidence. The absolute phase-difference produced in the magnetic vector polarized in the plane of incidence is the same as that

produced in the electric vector perpendicular to the plane of incidence; and by the reciprocal property of Maxwell's equations the above discussion holds for this electric vector, if we interchange K and μ .

$$\therefore -\tan \frac{\Delta}{2} \rho = \sqrt{\frac{K_1 \mu_2 \cos \phi_1}{K_2 \mu_1 \cos \phi_2}} \cot (\beta - L),$$

where

$$\tan \beta = \frac{i \cos \phi_3 \mu_2 v_2}{\cos \phi_2 \mu_3 v_3}.$$

Hence putting $\mu=1$, and writing $\Delta_p - \Delta_s = \Delta$, we have

$$\frac{\Delta}{2} = \text{arc tan} \frac{\sqrt{\frac{K_2}{K_1}} \cot (\alpha - L) - \sqrt{\frac{K_1}{K_2}} \cot (\beta - L)}{\frac{\cos \phi_2}{\cos \phi_1} + \cot (\alpha - L) \cot (\beta - L) \frac{\cos \phi_1}{\cos \phi_2}}. \quad (5)$$

where

$$\tan \alpha = i \sqrt{\frac{K_2 \cos \phi_3}{K_3 \cos \phi_2}}$$

and

$$\tan \beta = i \sqrt{\frac{K_3 \cos \phi_3}{K_2 \cos \phi_2}}.$$

This formula gives the relative phase-difference for any angles of incidence, when l , the thickness of the plate, is known. To find l in terms of Δ , *i. e.* to find the thickness of the film from the observations, put $x = \alpha - L$, $y = \beta - L$.

Then

$$\begin{aligned} \tan \frac{\Delta}{2} &= \frac{\sqrt{\frac{K_2}{K_1}} \cos x - \sqrt{\frac{K_1}{K_2}} \cot y}{\frac{\cos \phi_2}{\cos \phi_1} + \cot x \cot y \frac{\cos \phi_1}{\cos \phi_2}} \\ &= \frac{\sqrt{\frac{K_2}{K_1}} \cos x \sin y - \sqrt{\frac{K_1}{K_2}} \cos y \sin x}{\frac{\cos \phi_2}{\cos \phi_1} \sin x \sin y + \frac{\cos \phi_1}{\cos \phi_2} \cos x \cos y} \\ &= \frac{\sqrt{\frac{K_2}{K_1}} (\sin (x+y) - \sin (x-y)) - \sqrt{\frac{K_1}{K_2}} (\sin (x+y) + \sin (x-y))}{\frac{\cos \phi_2}{\cos \phi_1} (\cos (x-y) - \cos (x+y)) + \frac{\cos \phi_1}{\cos \phi_2} (\cos (x+y) + \cos (x-y))} \\ &= \frac{\sin (x+y) \left(\sqrt{\frac{K_2}{K_1}} - \sqrt{\frac{K_1}{K_2}} \right) - \sin (x-y) \left(\sqrt{\frac{K_2}{K_1}} + \sqrt{\frac{K_1}{K_2}} \right)}{\cos (x+y) \left(\frac{\cos \phi_1}{\cos \phi_2} - \frac{\cos \phi_2}{\cos \phi_1} \right) + \cos (x-y) \left(\frac{\cos \phi_1}{\cos \phi_2} + \frac{\cos \phi_2}{\cos \phi_1} \right)}. \end{aligned}$$

Therefore

$$\begin{aligned} \sin(x+y) \left(\sqrt{\frac{K_2}{K_1}} - \sqrt{\frac{K_1}{K_2}} \right) - \tan \frac{\Delta}{2} \cos(x+y) \left(\frac{\cos \phi_1}{\cos \phi_2} - \frac{\cos \phi_2}{\cos \phi_1} \right) \\ = \left(\sqrt{\frac{K_2}{K_1}} + \sqrt{\frac{K_1}{K_2}} \right) \sin(x-y) + \tan \frac{\Delta}{2} \left(\frac{\cos \phi_1}{\cos \phi_2} + \frac{\cos \phi_2}{\cos \phi_1} \right) \cos(x-y). \end{aligned}$$

Now

$$x+y = \alpha + \beta - 2L,$$

$$x-y = \alpha - \beta,$$

$$L = \frac{2\pi l \cos \phi_2}{\tau v_2}.$$

Put

$$\tan \gamma = \tan \frac{\Delta}{2} \frac{\frac{\cos \phi_1}{\cos \phi_2} - \frac{\cos \phi_2}{\cos \phi_1}}{\sqrt{\frac{K_2}{K_1}} - \sqrt{\frac{K_1}{K_2}}}.$$

Therefore

$$\begin{aligned} x+y-\gamma &= \alpha + \beta - \gamma - \frac{4\pi \cos \phi_2 l}{\tau v_2} \\ &= \arcsin \frac{\left(\sqrt{\frac{K_2}{K_1}} + \sqrt{\frac{K_1}{K_2}} \right) \sin(\alpha - \beta) + \tan \frac{\Delta}{2} \left(\frac{\cos \phi_1}{\cos \phi_2} + \frac{\cos \phi_2}{\cos \phi_1} \right) \cos(\alpha - \beta)}{\left(\left(\sqrt{\frac{K_2}{K_1}} - \sqrt{\frac{K_1}{K_2}} \right)^2 + \tan^2 \frac{\Delta}{2} \left(\frac{\cos \phi_1}{\cos \phi_2} - \frac{\cos \phi_2}{\cos \phi_1} \right)^2 \right)^{\frac{1}{2}}} \end{aligned} \quad (6)$$

ϕ_1, K_1, K_2 are known; $\phi_2, \alpha, \beta, \gamma$ can be calculated; Δ is observed; hence from one observation l can be determined to a multiple of 2π , and if we have a series of observations, l can be definitely determined.

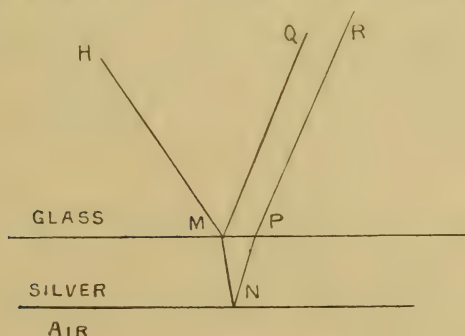
The method, according to which the silver-iodide film was prepared, was found to be very important. If warm iodine vapour in excess was allowed to act on a silver film, we obtained an opaque dull yellowish film; but if a small grain of iodine were left near the silver film for ten or twelve hours, a transparent film that gave beautiful interference-colours was obtained. Observations on a film produced according to this method agreed with theory. A set of such observations is given below. ϕ_1 , the angle of incidence, is calculated from the angle between the directions of the telescope and collimator. The thickness of the film, l , was determined by formula (6) from the observations Δ , and then substituted in formula (5) and Δ' , the theoretical value of the relative phase-difference, calculated. It was found necessary to take the positive value of the imaginary cosine, otherwise a

negative value of l would have been obtained. The silver iodide was screened from light as much as possible, and test readings showed that sensibility of the films to light was not an appreciable source of error. The agreement is very satisfactory, when we consider how imperfectly the mathematical conditions are realized experimentally. The auxiliary functions α , β , γ used in the calculation are given, also the difference between the observed and calculated values. l was 1.40λ .

Observations on Silver-Iodide Film.

ϕ .	$-\alpha$.	$-\beta$.	$-\gamma$.	Δ' .	Δ .	$\Delta' - \Delta$.
42 30	31.1	6.8	15.5	63.9	61.3	+2.6
44 0	39.4	9.2	21.0	75.2	73.2	+2.0
45 30	46.1	11.6	25.1	81.0	80.2	+0.8
47 0	50.7	13.6	28.2	84.0	82.8	+1.4
48 30	54.2	15.4	31.2	84.8	85.1	-0.3
50 0	57.0	17.1	32.3	84.8	82.8	+2.0
51 30	59.7	18.8	34.3	82.4	82.1	+0.3
53 0	61.1	19.7	36.5	79.9	82.1	-2.2
54 30	62.6	21.0	35.9	76.8	76.4	+0.4
56 0	64.0	22.1	36.9	73.2	74.0	-0.8
57 30	65.2	23.3	37.2	68.2	70.0	-1.8
59 0	66.2	24.2	36.4	64.4	64.0	+0.4
60 30	67.1	25.2	39.2	59.8	64.8	-5.0
62 0	68.0	26.2	36.7	55.3	56.7	-1.4

The rigorous formula becomes much more complicated in the case of the silver film. Even when we make several approximations, it is still impracticable and not so instructive as the method used. In the case of silver, as in silver iodide, we have a number of repeated reflexions in the film, but as the silver absorbs light we shall disregard all but the first.



Consider the vector polarized at right angles to the plane of incidence. Let ρ_s be the ratio of the amplitude of the reflected

vibration to the amplitude of the incident vibration at M; and ρ_s'' the similar ratio at N. Let δ_s be the ratio of the amplitude of the refracted to the amplitude of the original vibration at M. Those ratios are here complex. Write

$$\rho_s = r_s e^{i\Delta_s}; \quad \delta_s = d_s e^{i\Delta_s'}; \quad \rho_s'' = r_s'' e^{i\Delta_s''},$$

where r_s , d_s , and r_s'' are real. Then Δ_s , Δ_s' , and Δ_s'' are the changes of phase which the waves undergo.

Let us represent the incident wave in the direction HM by $\sin T$. Then the reflected wave in the direction MQ is $r_s \sin(T + \Delta_s)$. The wave which enters the glass at P has been twice refracted and once reflected. Its phase has been retarded by an amount L owing to its having to traverse the distance MNP. Then to take the absorption into account, we multiply by a factor k , like L dependent on the thickness of the film. The wave which enters the glass at P is therefore $kd_s^2 r_s'' \sin(T + 2\Delta_s' + \Delta_s'' - L)$; and the resultant reflected wave, polarized at right angles to the plane of incidence, is

$$r_s \sin(T + \Delta_s) + kd_s^2 r_s'' \sin(T + 2\Delta_s' + \Delta_s'' - L).$$

Similarly the resultant reflected wave, polarized in the plane of incidence, is

$$r_p \sin(T + \Delta_p) + kd_p^2 r_p'' \sin(T + 2\Delta_p' + \Delta_p'' - L).$$

When we sum the two simple harmonic motions, of which each of these waves consists, we find that their phases are

$$T + \Delta_s + \tan^{-1} \frac{kd_s^2 r_s'' \sin(2\Delta_s' + \Delta_s'' - \Delta_s - L)}{r_s + kd_s^2 r_s'' \cos(2\Delta_s' + \Delta_s'' - \Delta_s - L)} = T + \Delta_s + S \quad (7)$$

and

$$T + \Delta_p + \tan^{-1} \frac{kd_p^2 r_p'' \sin(2\Delta_p' + \Delta_p'' - \Delta_p - L)}{r_p + kd_p^2 r_p'' \cos(2\Delta_p' + \Delta_p'' - \Delta_p - L)} = T + \Delta_p + P \quad (8)$$

The relative phase-difference which we measure is approximately the difference of these two expressions.

If α_1 and γ_1 are the sine and cosine of the angle of incidence in the glass, and if $\alpha_2\gamma_2$ and $\alpha_3\gamma_3$ are defined by

$$n_0\alpha_1 = n(1 - iK)\alpha_2 = \alpha_3,$$

$$\alpha_2^2 + \gamma_2^2 = \alpha_3^2 + \gamma_3^2 = 1,$$

then (Voigt's *Kompendium*, ii. page 646)

$$\begin{aligned} \rho_s &= \frac{\alpha_1\gamma_1 - \alpha_2\gamma_2}{\alpha_1\gamma_1 + \alpha_2\gamma_2} & \rho_p &= \frac{\alpha_2\gamma_1 - \alpha_1\gamma_2}{\alpha_2\gamma_1 + \alpha_1\gamma_2} \\ \delta_s &= \frac{2\alpha_1\gamma_1}{\alpha_1\gamma_1 + \alpha_2\gamma_2} & \delta_p &= \frac{2\alpha_1\gamma_1}{\alpha_2\gamma_1 + \alpha_1\gamma_2} \end{aligned}$$

$$\rho_s'' = \frac{\alpha_2\gamma_2 - \alpha_3\gamma_3}{\alpha_2\gamma_2 + \alpha_3\gamma_3} \quad \rho_p'' = \frac{\alpha_3\gamma_2 - \alpha_2\gamma_3}{\alpha_3\gamma_2 + \alpha_2\gamma_3}$$

and $ke^{iL} = e^{-\frac{4\pi i\gamma_2 l n(1-iK)}{\lambda}}$.

n_0 is the index of refraction of the glass, $n(1-iK)$ the index of refraction and absorption for the film, l the thickness of the film, and λ the wave-length of the light in question in air.

When the silver film was thick, the relative phase-difference showed no effect due to the silver-air surface, but was the ordinary phase-difference produced by reflexion in glass on a silver mirror. When the film was thinner, however, there was a marked deviation from this curve. Instead of studying the relative phase-difference produced by the film, we studied the deviation of the relative phase-difference from the ordinary glass-silver curve, or in other words the effect of the "thinness." The formula for the deviation is the difference of the $\tan^{-1}s$ in (7) and (8), P-S.

It has been found by different observers that the optical constants of silver in thin films are different from the values obtained by Drude from observations on silver in mass. This may be due to the thickness of the film being comparable with the mean free path of the free electron. Fortunately this difference affects only one of the functions ρ_s'' , ρ_s &c. appreciably, namely Δ_s'' . It was found P could be disregarded in comparison with S; also that $2\Delta_s'$ was equal to Δ_s to a sufficiently high degree of approximation. This simplifies the formula for the deviation to

$$\tan^{-1} \frac{kd_s^2 r_s'' \sin(\Delta_s'' - L)}{r_s + kd_s^2 r_s'' \cos(\Delta_s'' - L)}.$$

The variation of k and L with the angle of incidence could be disregarded. As a result of trials, the values $nK=3.67$ and $n=0.61$, as against Drude's $nK=3.67$ and $n=0.18$, were found to give the best results. Six series of observations on the deviation are given in the next table, together with the theoretical value calculated for the values $\frac{l}{\lambda} = 0.070$ and 0.055 and the values of d_s^2 and r_s'' used in the calculation.

When we compare the experimental and theoretical results, there is not much similarity. The deviation is in the right direction and has its maximum near the proper place, but the shapes of the curves are different. We cannot improve matters by taking into account the terms that we have disregarded. *The agreement—such as it is—is the best that*

32 Total Reflexion at Second Surface of Parallel Plate.

can be obtained using the theory of the plane-parallel homogeneous plate. When the silver film is as thin as one-twentieth

Silver Films.

Angle of incidence.	Observed Deviation.						Calculated Deviation.			
	(1)	(2)	(3)	(4)	(5)	(6)	$\frac{l}{\lambda} = 0.055.$	$\frac{l}{\lambda} = 0.07.$	$d_s^2.$	$r_s''.$
39 50	...	...	1	...	8	2	4	5	2.8	0.85
41 0	2	0	3	2	4	1	1	2	2.8	0.89
42 30	1	0	1	4	15	12	-3	-4	2.8	1.00
44 0	0	1	3	6	25	23	-7	-9	2.7	2.32
45 30	-2	1	4	9	35	36	27	60	2.7	4.85
47 0	+3	3	6	12	39	39	18	41	2.7	3.61
48 30	+2	5	9	11	41	42	15	36	2.6	3.21
50 0	-2	2	4	9	32	39	12	26	2.6	2.69
51 30	-1	...	5	5	30	37	10	24	2.5	2.56
53 0	-1	...	1	8	25	34	10	20	2.5	2.40
54 30	...	...	0	...	18	32	9	18	2.4	2.26
56 0	...	...	1	...	16	24	8	16	2.4	2.15
57 30	...	...	...	...	10	19	8	15	2.3	2.08
59 0	...	...	...	...	3	—	7	14	2.3	1.99
60 30	...	...	...	...	1	10	7	13	2.2	1.96
62 0	...	...	...	...	1	5	6	12	2.2	1.93

of the wave-length of sodium light, as in the above experiments, then we can no longer consider it homogeneous; and apparently cannot expect an agreement with the formula derived on that supposition.

The values of d_s and r_s'' are very striking, particularly the value of r_s'' . In the ordinary case of total reflexion d_s can be as large as 2; but r_s'' can have much larger values than that. If we write $\alpha_2\gamma_2 = m + ip$, and $\alpha_3\gamma_3 = -is$, where m , p , and s are real positive quantities, then

$$r_s'' = \left| \frac{m + i(p + s)}{m + i(p - s)} \right|.$$

m is small in comparison with p for the metals gold, silver, and sodium. Consequently $\bar{r}_s''$, the maximum value of r_s'' , is $\frac{2p}{m}$. $\frac{p}{m}$ is the ratio of the imaginary to the real part of $\alpha_2\gamma_2$, or the ratio of the imaginary to the real part of α_2 , since γ_2 is approximately real. Now α_2 is given by

$$n_0\alpha_1 = n(1 - iK)\alpha_2.$$

Hence

$$\bar{r}_s'' = \frac{2p}{m} = 2K.$$

If we take Drude's values of n and nK , we obtain the following values of $\bar{r}_s''$:—

Metal.	n .	nK .	r_s'' .
Silver	0·18	3·67	41
Gold	0·37	2·82	15
Sodium	0·005	2·61	1043

If n were zero, and nK not zero (Lord Kelvin's "ideal silver"), $\bar{r}_s''$ would be infinitely great! If we had a very thin film of sodium, and a ray of light were repeatedly reflected within the film at the proper angle, its intensity would be very much increased. This would be contrary to the conservation of energy. The explanation of course, is, that in such thin films the geometrical laws of reflexion and refraction do not hold.

The observations recorded in this and the preceding article were carried out in the Physical Institute of the University of Göttingen at the suggestion, and under the continual advice of Prof. W. Voigt.

IV. Lord Rayleigh on the Virial Equation.

By S. H. BURBURY, F.R.S.*

IN his paper on this subject in the Philosophical Magazine for April 1905, Lord Rayleigh considers a system of molecules, elastic spheres, which also exert on each other finite forces. And concerning these forces he assumes that within the effective range of any type sphere there are many others, and that the forces which the type sphere exerts on the more distant molecules within this range are not inappreciably small compared with those which it exerts on its immediate neighbours. In his own language, the forces are "of the character considered in the theory of capillarity, that is extending to a range which is a large multiple of molecular distances, and not increasing so fast with diminishing distance as to make the total effect sensibly dependent upon the positions occupied by neighbours." Then he goes on to say: "Under these restrictions symmetry ensures that the resultant force upon a sphere, situated in the interior and not undergoing collision, is zero; and the whole effect of such forces is represented (Young, Laplace, Van der Waals)

* Communicated by the Author.

by an addition to the pressure of a quantity independent of the temperature and inversely proportional to the square of the volume."

By this he means one of two things, namely, (1) the resultant force on a molecule is zero on average of time; or (2) the resultant force is actually zero at every instant for a sphere not at that instant undergoing collision. If he means the first alternative, the resultant force must be zero on average of time if the motion be stationary. If, then, X, Y, Z be the component forces on a molecule, $\bar{X}, \bar{Y}, \bar{Z}$ their values on average of time, $\bar{X} = \bar{Y} = \bar{Z} = 0$. But it does not follow that $\bar{X}x + \bar{Y}y + \bar{Z}z = 0$ on average of time, that is that the time average of the Virial for molecules in the interior is zero.

In fact, as admitted in Lord Rayleigh's paper, it is not true for the collision forces between elastic spheres. Neither then can it be true if the molecules are centres of repulsive force, becoming evanescent at distances very small compared with molecular distances. There is in fact no proof that $\bar{X}x + \bar{Y}y + \bar{Z}z = 0$ for molecules in the interior, and therefore no proof that the whole effect "can be represented by an addition to the pressure of a term independent of the temperature and proportional to $\frac{1}{v^2}$."

If we take the second alternative, that at every instant the resultant force is actually zero for every sphere not at that instant undergoing collision, Lord Rayleigh must mean by "symmetry," that his spheres, although in motion relatively to each other, are at every instant in some symmetrical arrangement such, to take an example, as at the angles of cubes or regular tetrahedrons. Is a motion possible in which this shall be the case at every instant? Without going so far as to say that no such motion is possible, we may say, I think, with confidence that it cannot be motion in accordance with Maxwell's law. Lord Rayleigh's system, then, is not a rare gas. Perhaps it may be a liquid, or a dense gas, to which Maxwell's law is inapplicable.

In order to form a theory of the motion of a system of mutually acting molecules, to which we cannot apply the ever recurring assumption of infinite rarity, we require to know what part is to be played by the potential, χ , of the intermolecular forces. In a statical system in stable equilibrium χ is minimum. What is the corresponding law for stationary motion in a dynamical system? I think Lord Rayleigh is the man to answer that question, if he could be induced to do so.

In the silence of the authorities, I have myself suggested that Boltzmann's $e^{-2h\chi}$ law supplies the necessary guidance. The chance that any group or system of molecules shall be in a configuration in which the potential of their mutual forces, and of the external forces if any, is χ , is proportional to $e^{-2h\chi}$. Or, if you prefer so to state it, the time during which on the average of any very long time they will be in that configuration is proportional to $e^{-2h\chi}$. That gives the minimum χ for a statical system as a particular case. For if χ_0 be the potential in the configuration A_0 , and χ_1 in the configuration A_1 , and if $\chi_0 < \chi_1$, A_0 is more probable than A_1 in the ratio $e^{2h(\chi_1 - \chi_0)}$, that is in an infinite ratio in the statical system, for which h is infinite. The statical system must therefore be in minimum potential. Also if there be only external forces acting, Boltzmann's law gives $e^{-2h\chi}$ as the density at the point where the potential is χ , as in Maxwell's vertical column of air. I worked out the consequences of the application of the law to the general case in a former paper (Phil. Mag. for October 1901), and I think my conclusions were in the main right. If so, the law would be inconsistent with Lord Rayleigh's symmetry, and with its consequences. In fact it seems to me that Lord Rayleigh's symmetry and Boltzmann's law cannot both be true for one and the same system in the same state.

It may be said perhaps that Boltzmann's law holds only for external, and not for intermolecular forces. Some English writers, notably Dr. Watson, while not expressing their disagreement with the law as applied to intermolecular forces, prefer to let it alone. That I think arises from excess of caution, or perhaps because the law, if so applied, leads to results inconsistent with some favourite doctrines of the orthodox theory of gases. The proof of the law given by Boltzmann at p. 134 of his *Vorlesungen*, Part I., is formally applicable to intermolecular forces. Why may we not so apply it?

V. Actinium and its Successive Products.

By T. GODLEWSKI, Ph.D. (Cracow)*.

RUTHERFORD and Soddy, in their well-known investigations† on the activity of thorium, have shown that it is possible to separate from it a very active constituent

* Communicated by Prof. E. Rutherford, F.R.S. Presented before the Academy of Sciences in Cracow, April 3, 1905.

† Rutherford and Soddy, Phil. Mag. Sept. and Nov. 1902; Trans. Chem. Soc. lxxxi. pp. 321 & 807 (1902).

which they call ThX. The activity of this product decays with the time according to an exponential law; *i. e.*, the equation of monomolecular chemical reaction, falling to half value in about four days. At the same time, the thorium, which by the removal of thorium X had been deprived of about 75 per cent. of its activity, recovered its activity, the recovery curve being complementary to the curve of decay of ThX. The substance UrX, discovered earlier by Crookes, acts in a manner analogous to ThX. The β activity of this substance decayed according to an exponential law with the time, falling to half value in twenty-two days.

Rutherford and Soddy have explained these phenomena on the supposition that the radioactive bodies are producing fresh radioactive matter at a constant rate, and that the activity of the matter so formed decreases according to an exponential law with the time. The discovery of these phenomena supplied the basis for the disintegration theory, which supposes that the atom of a radioactive body breaks up through a series of well-marked stages. The resulting products are quite distinct bodies, though they escape detection by chemical methods on account of the minute amount of the substance under investigation. Their existence is proved first of all by electrical measurements which allow us to make the quantitative investigations of the rate of change of these products.

On looking over the series of successive products arising from different radioactive bodies, striking similarity between the products of thorium and actinium is at once manifest. Thorium produces ThX, ThX the emanation, this gives rise to the active deposit which undergoes two further transformations, the first slow change being a rayless one, the other emitting all kinds of rays. Actinium in like manner produces an emanation which is transformed into an active deposit which undergoes two further changes, the first being a slow rayless change and the other a rapid change.

This analogy in the number and nature of the products pointed to the possibility* that there existed between the actinium and its emanation an intermediate product which bore the same relation to actinium that ThX bears to thorium. In a letter to 'Nature' (26 Jan. 1905) I gave the preliminary results of the investigation which proved the existence of this product.

Taking into consideration the similarity of actinium and thorium, I applied to the actinium the same method which

* See Rutherford, Bakerian Lecture: "The Succession of Changes in Radioactive Bodies," Phil. Trans. Royal Soc. ser. A, vol cciv. pp. 190 & 204.

had been used by Rutherford and Soddy for the separation of ThX from thorium.

The experiments were made with the emanating substance of Giesel, which, according to numerous investigations*, has been found to contain the same radioactive constituent as the actinium of Debierne. The saturation current due to the α -ray activity of the products under investigation was measured with a quadrant electrometer of sensibility 120 divisions per volt; the needle was kept at the standard potential of 300 volts. The β activity was measured by means of a sensitive electroscope. Four different sets of experiments were made which gave very concordant results.

In each case 0.15 gr. of the emanating substance, of activity about 300 times that of uranium, was dissolved in 250 cm. of hydrochloric acid (about 8 per cent. concentration). The solution was evaporated on the water-bath to about 100 c.cm. When ammonia was added to the solution, a reddish-brown substance (probably hydroxide) was precipitated. The precipitate, collected on a filter-paper, was dried as quickly as possible, and then its activity was measured. The filtrate was then evaporated to dryness, and when ammonium salts were driven off by ignition a small amount of a brown-black residuum was left behind on the dish. On raising this to a red heat, the colour of the residue changed from black to white. This residue was intensely active compared with the weight. Immediately after the dish was cold the activity of the residue was measured and was found to decrease slowly with the time according to an exponential law. In the same period the actinium, which by precipitation had been rendered almost inactive, recovered its activity, the recovery curve being complementary to the curve of decay.

From analogy to ThX, which it so closely resembles in radioactive properties, the active substance separated from actinium will be termed Actinium X (AcX).

The results of one of the series of experiments are given in Table I. and graphically represented in fig. 1. In Table I. the first column gives the days measured from the time of separation; the second column gives the activity. The activity of AcX is expressed as a percentage of the initial activity, the maximum activity soon after separation being taken = 100. But for actinium (deprived of AcX) the final value is taken = 100.

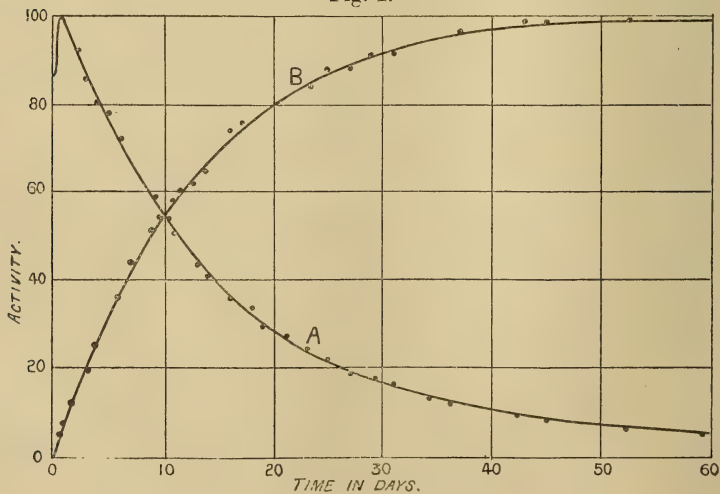
In fig. 1, curve A represents the activity of AcX as a function of the time, the activity of AcX being expressed in the same units as in Table I. In the recovery curve B the difference between the maximum and the first value is taken = 100.

* See f. i. Rutherford, Bakerian Lecture, *loc. cit.* p. 188.

TABLE I.

AcX.		Actinium.	
<i>t</i> in days.	Activity.	<i>t</i> in days.	Activity.
0.25	87.5	0.1	5.25
0.9	100	0.7	9.22
1.9	92.6	1.7	16.7
3.0	86.0	2.7	24.0
4.0	81.5	3.7	29.6
5.9	72.6	5.7	39.8
6.9	70.7	6.7	48.0
8.9	59.2	8.7	52.5
9.9	54.5	9.7	56.5
10.9	50.8	10.7	61.4
12.9	43.6	12.7	62.5
13.9	41.2	13.7	68.6
14.9	40.5	15.8	76.2
15.9	36.2	17.0	78.0
17.1	35.4	20.7	82.3
17.9	34.0	22.7	85.7
21.0	26.5	25.0	88.6
22.9	24.4	27.0	90.0
24.9	21.8	30.8	92.3
27.2	18.5	36.5	97.1
29.2	17.6	45.0	99.5
30.9	16.1	52.0	99.0
34.0	13.5	64.0	100
36.0	12.1	67.0	100
42.0	8.82		
45.0	7.77		
52.0	5.89		
59.0	4.63		
67.0	3.69		

Fig. 1.



The activity of AcX immediately after removal was weight for weight more than a hundred times as great as that of the original actinium. In the various experiments the value of the initial activity of AcX was proportional to the amount of actinium used, but was by no means proportional to the total weight of matter obtained from the filtrate. In some cases, for example, only a few milligrams of the substance were obtained, which exhibited as great activity as a few centigrams obtained in other cases. This shows clearly that in the case of actinium, the substance obtained from the filtrate, which we see and weigh, does not all consist of AcX. The substance contains some impurities; in the present case, probably some of the rare earths. The amount of actinium X actually present is so minute that it precludes the possibility of a direct chemical investigation of its properties.

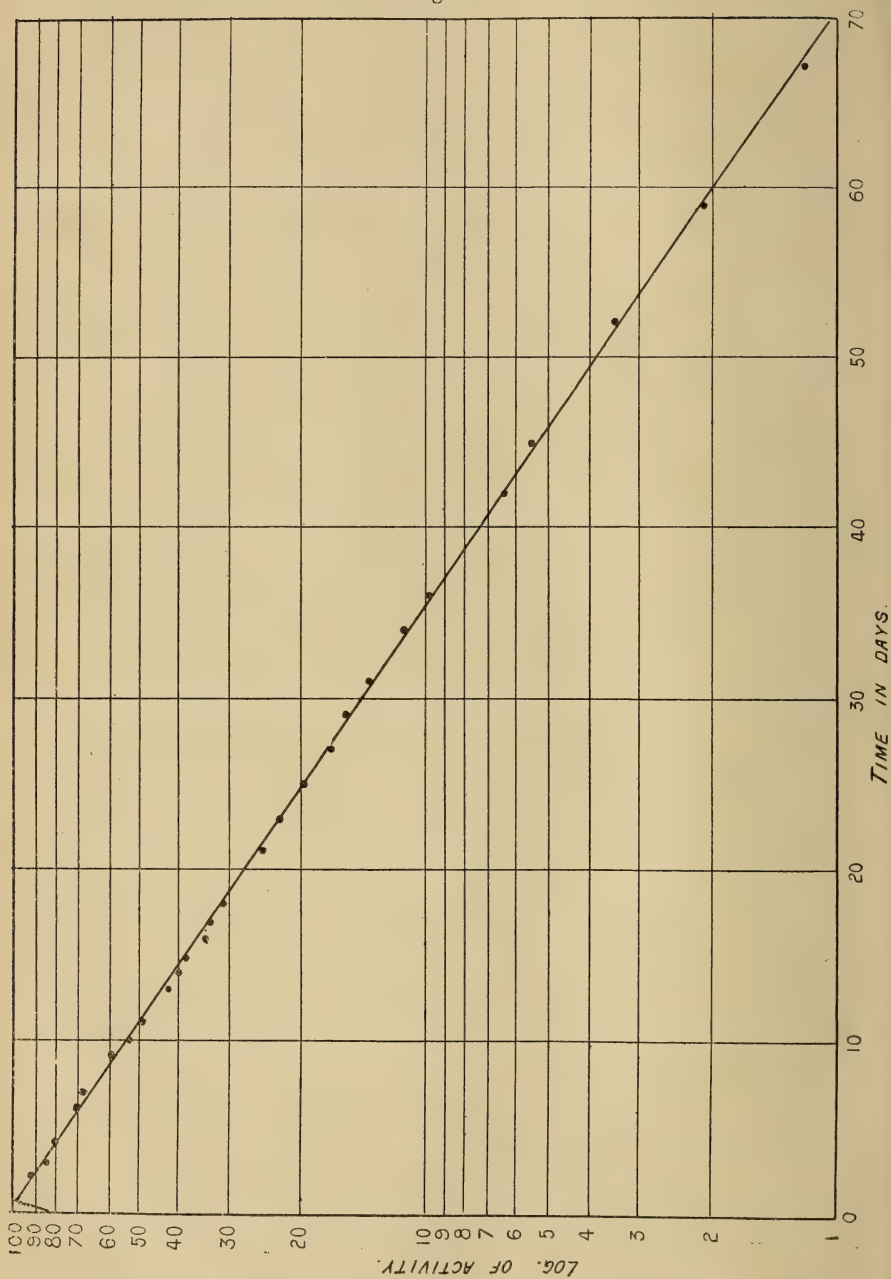
We see from Table I. that the activity of AcX increases in the first day after removal to about 15 per cent. of its original value, reaches a maximum, and then decays with the time according to an exponential law, falling to half value in 10.2 days. This exponential law of decay is clearly seen in fig. 2, where the ordinates represent the logarithms of the activity of the product AcX, and the abscissæ the time after separation. On subtracting from the quantities given in the Table I. the number 2.70, which represents the activity of the residue which did not decay with the time, the points fall accurately on a straight line, as in the figure. This non-decaying residue comes probably from the small amount of actinium, which is not precipitated, and is therefore present in the filtrate.

The activity of actinium, from which actinium X was removed, increased so that the recovery-curve was approximately complementary to the decay-curve of AcX. The small difference between the experimental and the theoretical curve, as expressed by the equation $I_t = I_0 (1 - e^{-\lambda t})$, where λ has the same value as in the decay-curve, is probably due to a variation in the rate of escape of the excited activity, which is extremely volatile. In all cases the decay-curve agreed more closely with the theoretical equation than the recovery-curve.

The initial increase of activity of AcX immediately after removal (see fig. 1 A and fig. 2) is analogous to the similar increase of activity of ThX. The only difference is that the recovery-curve of actinium does not show the same initial decay as found in the case of thorium*. This fact is explained by the different properties of the excited activity

* See Rutherford, 'Radioactivity,' pp. 180 & 295.

Fig. 2.



of actinium and thorium. The active deposit of actinium is soluble in ammonia, and is volatile when heated*. The active deposit of thorium, on the other hand, is not soluble in ammonia and is not so readily volatilized. The initial increase of activity of AcX is thus explained in the following manner.

When actinium is precipitated with ammonia, the active deposit is left behind in the filtrate together with AcX . In the moment, however, when we heat, the volatile active deposit is driven off also. But as soon as AcX is separated, it at once produces the emanation which gives rise to the active deposit. The activity of the latter, at first, more than compensates for the decay of activity of AcX , which has a comparatively slow change, and in consequence the activity of AcX first of all increases.

On the other hand, the actinium, when treated with ammonia, was deprived not only of AcX but also of most of the active deposit. Any of the latter, if still remaining in the precipitate, would be driven off during the process of drying. In consequence, when we start the measurements of the activity of the precipitate itself, no excited activity is present. The activity at once commences to increase, since a fresh amount of AcX is produced which in turn gives rise to excited activity. In consequence we do not observe the initial decay in the recovery-curve of actinium as in the corresponding curve for thorium.

Disregarding these small peculiarities, the behaviour of the product AcX , and of actinium deprived of AcX , is, as we have seen, completely analogous to that of ThX and of thorium deprived of ThX . There is, however, the following distinct difference. After removal of ThX , thorium always has a certain amount of residual activity about 25 per cent. of the maximum value. A similar effect is observed in the case of radium, where the "de-emanated" radium has always a non-separable activity of about the same (25 per cent.) value. In the case of actinium, immediately after removal of AcX the actinium is almost inactive, its activity being only 5 per cent. of its maximum value. I tried experiments to see whether this activity could not be removed by means of successive precipitations with ammonia, but although eight precipitations were made in the course of seven hours the residual activity always remained. Nevertheless, the smallness of the initial amount of activity pointed to the probability that in reality

* The more complete account of the physical and chemical properties of the active deposit of actinium will be published later, as the experiments are not yet completed.

the actinium itself is not active, and that the residual activity observed is due to a small quantity of AcX, which is left behind. The interval between the last precipitation and the first measurement was always one hour or more, but this alone would not account for the observed current. It seems very probable that at the moment of the removal of AcX, *if the separation were complete*, actinium would be entirely devoid of activity. From the point of view of the theory of radioactive changes, this shows that the change of actinium into AcX is not accompanied by either α , β , or γ rays or, in other words, is a "rayless" change.

By means of an electroscope, it was found that actinium X gave out all three kinds of rays α , β , and γ . Now the products of excited activity are very quickly formed owing to the very rapid change of the emanation. The activities of these products are consequently measured together with AcX. It was separately proved that the active deposit gave out β rays*. Taking into consideration the analogy with thorium and even with radium, we should expect that the measured β activity of AcX arises not from AcX itself, but from the excited activity resulting from it. There is, however, strong evidence that in the case of actinium the β and probably the γ rays are emitted also by AcX itself. For instance, the curves of decay of activity of AcX measured by α and β rays are throughout identical, even from their beginning; and further, the activity of AcX measured by β rays, five minutes after strong heating, when all the volatile excited activity should be driven off, exhibits a very great initial value, which could not be the case if the rays were emitted only by excited activity.

It is thus most probable that AcX itself gives rise to all three kinds of rays.

Source of the Actinium Emanation.

In the case of thorium the product ThX was intermediate between thorium and its emanation. In order to see whether AcX occupies the same position in actinium, I measured the rate of change with time both of the emanating power of AcX and of actinium from which AcX was removed. The measurements of activity of the emanation were made in a cylindrical brass testing-vessel†, in the interior of which three insulated

* It was found that the excited activity of actinium, measured by β rays, after a long exposure decayed according to an exponential law with the time, falling to half value in 36 minutes. The complete account of these investigations will be given in another place.

† See Rutherford, 'Radioactivity,' p. 199, and fig. 37.

electrodes were placed; during the measurements one of the electrodes was connected with an electrometer and the other two were earthed.

Both actinium X and actinium deprived of AcX were placed in solutions of ammonium chloride in two bubbling flasks, and these could be successively connected with the testing cylinder in which the amount of emanation was measured. For the purpose of comparison with a substance of a standard emanating power, the following arrangement was used.

The current of air passed through the bubbling bottle and carried with it the emanation of the product to be investigated. It thus passed through the testing cylinder, in which the activity of the emanation was measured. On leaving the cylinder, the current of air entered a glass tube 2 cms. in diameter and about 3 metres long. At the end of this glass tube some fresh solid actinium was placed, and the emanation from this was carried into the second testing cylinder where its activity was measured.

In this manner the same current which carried the emanation from the product under investigation also carried the emanation from the standard actinium. In passing through the long and wide glass tube the emanation which left the first cylinder decayed completely before reaching the second cylinder. By this method of measurement, the emanating power of AcX was directly compared with the standard emanating power of solid actinium.

The experiments made in this manner showed:

- (1) That the actinium immediately after removal of AcX gives practically no emanation.
- (2) That the rate of increase of the emission of emanation of actinium after removal of AcX is the same as the rate of increase of its activity.
- (3) That the emanating power of AcX decreases at the same rate as the activity of AcX.

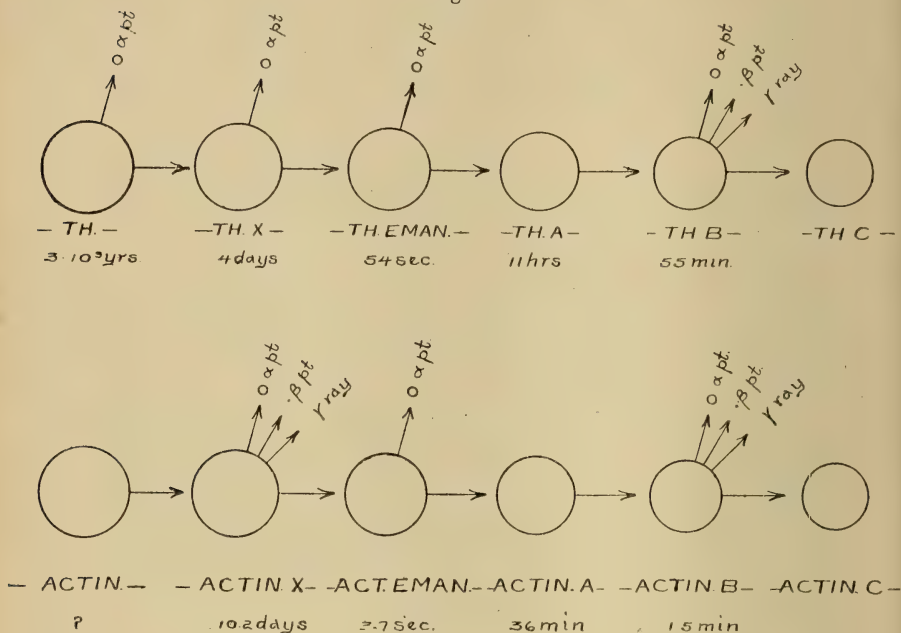
Since the emanation is only observed when actinium X is present, and is always proportional to the amount of actinium X, it must be a product of actinium X.

The changes occurring in actinium are shown in the following graphical representation*, together with the period required for transformation to half value. For comparison the changes taking place in thorium are also given.

* See Rutherford, Bakerian Lecture, *loc. cit.* pp. 180-190 & 204; also Miss Brookes, *Phil. Mag.* Sept. 1904, pp. 382-384; also Bronson, *Amer. Journ. of Science*, vol. xix. Feb. 1905, p. 187.

It is seen that there is a very striking similarity between the number and nature of the changes for actinium and thorium. But the periods of decay, the radioactive, chemical

Fig. 3.



and physical properties of the products of actinium all point conclusively to the fact that we have in actinium a distinct chemical element.

Whilst writing this paper, the December number of the *Jahrbuch für Radioaktivität und Elektronik* was received containing a paper by Giesel on Emanium*. In this paper Dr. Giesel gives an account of his investigation, in which he finds that it is possible to separate from emanium by precipitation with ammonia a small amount of very strongly active substance. The method of separation employed by him was then identical with the method I have used. I cannot, however, compare quantitatively my results with his, inasmuch as Dr. Giesel does not publish any measurements.

A short account will now be given of some experiments which are still in progress on the nature of the β and γ rays of actinium.

* Giesel, "Untersuchungen über das Emanium (Actinium)," *Jahrbuch f. Radioakt.* vol. i. pp. 375-358.

The β rays of actinium are completely distinct in their character from the β rays emitted by the other radio-elements, inasmuch as they are completely homogeneous. This fact was established by the measurements of absorption of β rays in passing through solid bodies. The activity measured by β rays decreased strictly according to an exponential law with the thickness of matter traversed. The equation $I = I_0 e^{-\lambda d}$, where d is the thickness, was applicable even in the case when I was less than 1 per cent. of its original value.

The β rays from actinium differ also from the β rays of other radioactive elements in the absolute value of the absorption constant λ , which is about 2.5 times as great with actinium as with uranium. Thus the β rays of actinium have less than half the penetrating power of those emitted by any other radio-element.

The existence of the γ rays from actinium was also distinctly proved. The absorption measurements showed that the γ rays of actinium are fairly homogeneous, and their penetrating power was only about one quarter of that observed for the rays from radium. A more complete account of these investigations will be published later on.

In conclusion it is my most pleasant duty to express my deepest gratitude to Prof. Rutherford for suggesting these investigations, for his kindness in the advice he has so freely given to me, and for placing at my disposal all the plentiful resources of his laboratory at McGill University.

McGill University, Physics Building,
February 24, 1905.

VI. Some Radioactive Properties of Uranium.

By T. GODLEWSKI, *Ph.D.* (Cracow)*.

1. The Discovery of UrX.

IN 1900 Sir William Crookes † showed that it is possible to separate from uranium by a single chemical operation a small amount of radioactive substance to which he gave the name UrX. This substance was, weight for weight, many hundred times more active photographically than the uranium from which it had been separated. The uranium deprived of this substance was almost inactive.

Similar results were afterwards observed by Becquerel ‡,

* Communicated by Prof. E. Rutherford, F.R.S. Presented before the Academy of Sciences in Cracow at the sitting of 9th of May, 1905.

† Crookes, *Proc. Roy. Soc.* lxvi. p. 409 (1900).

‡ Becquerel, *C. R.* cxxxi. p. 137 (1900); cxxxiii. p. 977 (1901).

who also noted the important fact that uranium recovered its activity with the time, while the activity of the separated substance decayed. This phenomenon was then quantitatively investigated by Soddy *, by Rutherford and Grier †, and by Rutherford and Soddy ‡.

These investigations proved that the activity of UrX, when measured by β rays, decayed with the time according to an exponential law, falling to half value in 22 days. In the same period uranium which, by removal of UrX, was deprived of all its β activity recovered it, and the recovery curve was complementary to the curve of decay of UrX. From the point of view of the disintegration theory, this fact indicated that UrX is a successive product of uranium, and the change of UrX into its successive product was accompanied by the emission of β particles.

2. *The Experiments of Meyer and Schweidler on Uranium.*

In 1904 Meyer and von Schweidler § repeated the quantitative measurements of Rutherford and Soddy with the difference, that while the latter used for separation of UrX the method of Becquerel, they made use of Crookes's method.

The aqueous solution of uranium nitrate was shaken with ethyl ether, and then the ether and water portions were separated from one another. The ether portion contained uranium nitrate deprived of UrX, and the β activity of this portion increased according to the theoretical curve, to half of its total value in 22 days. The uranium nitrate, however, when crystallized from the remaining water portion, lost its β activity at a different rate, decaying to half value in 2 days instead of 22 days. This unusual fact, that the recovery and decay curves of a radioactive product were not complementary to one another, either pointed to the existence of a new product, or indicated some unknown radioactive phenomenon. In order to elucidate this question, Meyer and Schweidler started a series of investigations on the radioactive properties of uranium nitrate freshly crystallized from the water solutions. They substantiated the fact that uranium nitrate crystallized from the hot-water solutions in the form of compact plates exhibited a peculiar radioactive behaviour. The activity of these

* Soddy, Trans. Chem. Soc. lxxxi. p. 860 (1902).

† Rutherford and Grier, Phil. Mag. Sept. 1902, p. 315.

‡ Rutherford and Soddy, Phil. Mag. April 1903, p. 411.

§ Meyer and v. Schweidler, "Untersuchungen über radioaktive Substanzen, II.: Ueber die Strahlung des Uran." *Sitzber. der Wiener Akad. Mathem.-naturwiss. Klasse*, Bd. 113. Abt. II. a. pp. 1057-1079, Juli 1904.

plates decayed in the first few days after crystallization to about half of its original value, reaching a minimum after four or five days, and then increased slowly for a very long time. The time in which the minimum was reached and the initial form of the curve were both dependent on the thickness of the plate. As regards the meaning of this phenomenon, the authors suggest two possibilities: either that there is a change in the activity itself, or that the absorption of the rays is modified by the physical alteration of the crystallized plates*.

Prof. Rutherford kindly suggested to me, that I should make some investigations to explain these phenomena.

3. *The Separation of UrX from Uranium by means of Fractional Crystallization.*

The experiments were first made in order to find out the conditions under which this first rapid decay of the β activity of uranium is obtained.

As in the experiments of Meyer and von Schweidler, equal weights of uranium nitrate † and water were taken, and this solution was shaken with an equal weight of ethyl ether. The ether solution was then carefully separated from the water solution, and both were evaporated to dryness. In the case of water solution, the evaporation was continued until even the water of crystallization was driven off.

The α and β activity of both portions were then measured.

The β activity was measured by means of an electroscope ‡ of the type of C. T. R. Wilson; the bottom of the electroscope was removed and replaced by aluminium foil 0.08 mm. thick, which absorbed all the α rays.

The measurements showed that uranium nitrate from the aqueous solution which contained the excess of UrX , derived from the ether solution, lost the corresponding excess of its activity according to an exponential law with the time, falling to half value in 22 days. The ether portion, which was at first almost completely inactive, when measured by β

* "Bezüglich der Deutung dieses Verhaltens ist zunächst die Möglichkeit gegeben, dass es sich um Änderungen der Aktivität selbst handelt, oder dass durch physikalische Zustandsänderungen der Kristallplatten ihr Absorptionsvermögen beeinflusst wird. Eine definitive Entscheidung zu geben wäre verfrüht." Meyer und v. Schweidler, *loc. cit.* p. 1075.

† The uranium nitrate under the experiments was obtained from Merck in Darmstadt and was labelled "extra pure."

‡ See Rutherford, 'Radioactivity,' p. 71 & fig. 11.

rays, recovered its activity according to a complementary curve *.

The only difference between my experiments and those of Meyer and Schweidler was that my uranium nitrate was deprived even of its water of crystallization by evaporation, while in the experiments of the above named authors the uranium nitrate was crystallized from the solution. This proves that the rapid decay of activity occurs only when uranium nitrate is crystallized, but it does not occur when it was obtained from the solution by evaporation which had been carried so far that the water of crystallization was driven off.

This fact being established, the subsequent experiments were made in the same manner as the experiments of Meyer and Schweidler.

After separation of the ether solution, the aqueous solution containing an excess of UrX was concentrated on the water-bath, and was then left for a short time at the temperature of the room. The great part of the uranium nitrate crystallized at the bottom of the dish forming a compact plate, on the surface of which the rest of the solution remained. This mother liquor was poured off into another dish and was kept on the water-bath until the solution lost all except the water of crystallization. The solution after it was taken off the water-bath crystallized at the temperature of the room, forming a compact dry plate.

The whole process of preparation and measurement was repeated many times. Table I. gives one of the series of experiments. T denotes the time in days from separation to measurement. The β activity is expressed as the ratio of the activity of the investigated product to the β activity of a standard amount of uranium oxide taken as 1000. The β activity is expressed in the same units throughout this paper.

These results are graphically represented in fig. 1, where the ordinates give the activity, in the same units as before, the abscissæ the time in days after separation. Curve I gives the activity of the ether portion, curve II that of the first plate of crystal, curve III the activity of the plate of crystal obtained from the mother liquor.

* I omit the detailed numbers obtained in these measurements because the results are completely normal; and further, during the time when these investigations were being made Meyer and Schweidler published a short paper (*Wiener Sitzungsber.* Dec. 1904) in which they showed that when a very small amount of water was present in the solution, the decay of activity of UrX was quite regular.

TABLE I.

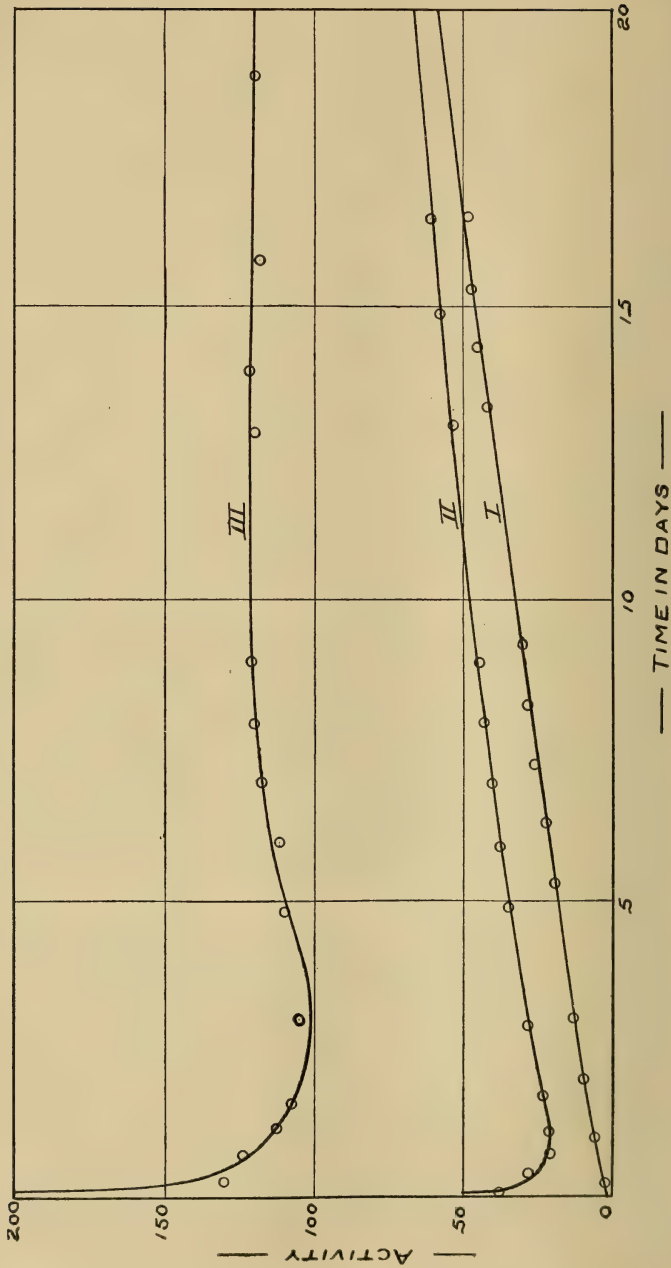
Ether portion.		Water portion.			
		First crystal plate.		Plate of crystal from mother liquor.	
T.	β Activity.	T.	β Activity.	T.	β Activity.
0.25	19	0.10	366	0.12	2190
1	57	0.32	281	0.33	1370
2	84	0.75	210	0.75	1270
3	131	1.12	219	1.12	1125
5.3	190	1.64	236	1.68	1090
6.3	220	2.9	285	2.9	1090
7.3	260	4.9	344	4.9	1100
8.3	273	5.9	368	6.0	1120
9.3	311	7.0	399	7.0	1180
13.3	420	8.1	427	8.1	1210
14.3	446	9.1	440	9.1	1210
15.3	460	12.8	529	12.9	1200
16.5	480	13.8	545	13.8	1210
		14.8	572	15.7	1190
		16.4	598	18.7	1200

It is seen that the activity of the ether portion increases with the time according to the theoretical curve, reaching half final value in twenty-two days. The activity of the first part of aqueous solution, which contained the first crystals, falls to about half value in about one day, this value being almost the minimum, and then increases slowly with the time. As we see from fig. 1, the second part of curve II is parallel to curve I, which shows that the activity of the first plate of crystal increases at the same rate as that of the ether portion.

The plate obtained from mother liquor (curve III) lost its activity during the first few days after crystallization. Its activity after reaching a minimum and after a small increase remained practically constant. After two months it was observed to have decreased only 10 per cent.

Disregarding for a moment the first rapid decrease of activity of both aqueous portions, which is exactly of the same nature as observed by Meyer and Schweidler, we see that we have the β activity in two cases increasing at the same rate, *i. e.* the increase of activity of the ether portion and of the plate of crystal first obtained. This points to the fact, that UrX was removed not only from the ether portion, but also in some degree from the first obtained crystals.

Fig. 1.



The mother liquor must contain then the greater excess of UrX.

In connexion with this, experiments were made with fresh uranium nitrate, and they showed that by even one crystallization it is possible to separate the uranium nitrate into two parts, namely the crystals and the mother liquor; the latter part containing seven times as much of UrX as the former. By means of several fractional crystallizations we can deprive uranium almost completely of the substance UrX, which is so readily soluble in water.

This at once explains the radioactive behaviour of the crystals first obtained from the aqueous solution after treatment with ether. A large part of the UrX remained in the mother liquor, and the crystals themselves contained even less than the equilibrium amount of UrX. In consequence, the activity of the crystals must increase according to a recovery curve of UrX. The experiments show that this is really the case. (See curve II on fig 1.)

In a similar way we can equally well explain the increase of activity observed by Meyer and Schweidler* in the crystals of uranium nitrate obtained from water solution.

If in these experiments some part of mother liquor was poured off the surface of the plates of crystals, the uranium crystals would contain less of UrX than the uranium itself in a state of equilibrium. The increase of activity would thus be due to the recovery of the separated UrX. And in fact the authors state that this part of curve corresponds to the constant of 22 days.

The activity of the crystals obtained from the mother liquor at first decayed very rapidly and, after reaching a minimum, increased a very small amount and finally remained almost constant, falling only 10 per cent during two months. This is shown in curve III.

The percentage decrease observed in this experiment is smaller than the percentage rise observed in curves I and II. This is due to the fact that the layer of crystals finally obtained from the mother liquor was about three times as thick as in the previous fractions.

In other experiments, where the thickness of the plate was relatively very small, the excess of β activity diminished regularly and in a more marked degree.

* Meyer and Schweidler, *loc. cit.* p. 1074, figs. 6 & 7.

4. *The Effect of Crystallization on the β Activity of Uranium Nitrate.*

We shall return now to the initial rapid decay of the β activity of uranium nitrate immediately after it was crystallized in the form of plates from the aqueous solution.

A decay quite analogous to that shown in the first part of the curves II and III was also obtained, when uranium nitrate had been crystallized from the pure aqueous solution, when no ether separation was applied.

The following is one example:—

25 grams of uranium nitrate was dissolved in a small amount of water; the solution was evaporated on a water-bath till it lost all its excess of water. This solution of uranium nitrate in its water of crystallization was kept for some minutes at the temperature of the room, where it crystallized, forming a compact dry plate. The variation of activity with the time was then measured, and one of the examples is given in Table II., where T is time reckoned in hours from the moment of crystallization.

TABLE II.

T (in hours).	β Activity.
0	1310
2.25	1130
4.5	1030
23	895
44.5	880
52	885
71	875
95	880
120	890

We see that immediately after crystallization the β activity decays, reaching a minimum after about two days.

The measurements of Meyer and Schweidler were then once more confirmed. The fact that the minimum was reached in a shorter time after crystallization in the experiments of the writer than in those of Meyer and Schweidler, is fully explained by the difference in experimental conditions which greatly influence this period.

This decay of activity after crystallization at first suggests the existence of some other product besides UrX . But the absence of the complementary recovery curve contradicts this supposition. And further, the rate of decay of radioactive products is generally independent of conditions. In these experiments, however, the time when the minimum was

reached, as well as the form of the curve, was dependent upon many factors. In different experiments, the relative values of the activity at the minimum point and the rates of decay were dependent upon the thickness of the plate of crystals, and upon the concentration of the solution from which the crystals were obtained.

In consequence, it would be difficult to suppose the existence of some other product.

The supposition of Meyer and Schweidler* that the phenomena are produced by some changes in absorbing power of the plates of crystals cannot explain the observed fact, when we take into consideration that the activity measured by α rays does not exhibit the same behaviour. The experiments of Meyer and Schweidler showed that the α activity remained practically constant. The writer made also experiments which completely confirmed this fact. And every change in absorbing power of the plates would be, of course, first of all shown by variation of α activity. Since these results can neither be explained by the existence of a new product nor by a change in absorbing power, there remained the possibility that the process of crystallization alone influences the β activity of uranium nitrate. In order to show whether this was really the case, the following experiments were undertaken.

The hot solution of uranium nitrate containing only the water of crystallization was put under the electroscope. After about two minutes the disturbance of the gold leaf, produced by the neighbourhood of the hot body, ceased, and it was then possible to investigate the effect due to the process of crystallization by measurements of the activity. The experiments showed that at the moment when the crystallization started, the β activity commenced to increase very rapidly, reaching the maximum when the crystallization was finished.

The following is an example of the experimental results obtained. 25 grams of uranium nitrate were dissolved in some water and evaporated in a flat glass dish on a water-bath until it lost all the excess of water. The dish containing this hot solution of uranium nitrate in water of crystallization was then put under the electroscope. After three minutes the activity could be measured with accuracy. The results are shown in Table III., where T is the time in minutes from the moment when the solution was taken off the water-bath.

* Meyer and Schweidler, *loc. cit.* p. 1075.

TABLE III.

T (in minutes).	Activity.
3	900
4	890
5	910
6	900

At this period the crystallization started.

8	1000
10	1180
12	1250
14	1350
17	1530
22	1710
27	1780

At this period the crystallization ended.

30	1780
35	1780

On the surface of the plate some drops of distilled water were now added, and the dish was placed again on the water-bath, so that the crystals melted in the water of crystallization. The measurements were repeated in the same manner as before.

In the moment when the second crystallization started, the activity of the solution was 1530 ; when it was finished the activity of the plate was 2830.

After the third crystallization the activity was 2940.

The fourth and fifth and sixth crystallizations did not cause a further increase of the activity.

This maximum activity then decayed with the time and reached the value 935 after three days, and, disregarding small irregular oscillations, remained constant at this value through many weeks.

Similar experiments were repeatedly made, and gave the same qualitative results.

These experiments show that the β activity of uranium nitrate is very considerably augmented by the actual process of crystallization, and it will be proved later that the decay of activity noted immediately after crystallization is due to the loss of this excess of activity produced by crystallization.

The explanation of the increase of activity at the moment of crystallization is very simple. We know that all the β activity of uranium proceeds not from the uranium itself, but from UrX . But UrX is so readily soluble in water that it is possible, as we have seen, to separate UrX from uranium by fractional crystallization. If, as usually the case, the hot

uranium solution starts to crystallize from the bottom of the dish, first of all uranium itself crystallizes and UrX is pushed in the direction of the surface. When the whole mass is solidified, we get a plate which contains on the surface an excess of UrX , and in the lower layers a deficit of this substance. The β rays, which come from the UrX present near the surface, emerge with little absorption in the mass of uranium itself, and thus the β activity must be larger than when UrX is uniformly distributed throughout the plate. In the same way we can explain the steady growth of activity during the actual process of crystallization when UrX is continually passing to the upper layers.

Many observed experimental facts prove with certainty the correctness of this explanation of the increase of β activity produced by crystallization.

For instance, we do not get the increase of β activity when the solution is continuously stirred during the crystallization, so that instead of a compact plate there is a powder composed of very small crystals. Moreover, under suitable thermal conditions the crystallization may be started at the surface instead of at the bottom, and then the increase of activity is not observed after crystallization but, on the contrary, there is often a decay.

This last fact suggested to me a decisive test. If the increase of activity during the crystallization is due to the fact that UrX is pushed to the upper layers when the crystallization starts from the bottom of the vessel, then the lower layers of the plate of crystal should contain less of UrX . In order to see whether this was really the case, I took a plate of crystal of which the β activity was 1840. The plate was cut across so that it could be removed from the dish, and it was then taken out and inverted so that the under surface faced the electroscope. The β activity was found to be 528.

This experiment shows quite clearly the truth of the explanation of the rise of activity during the process of crystallization. By the crystallization, UrX was pushed to the upper layers; when we turn the plate, the upper layers containing the excess of UrX are now underneath and, before reaching the electroscope, the β rays, which start from UrX , must pass through the whole thickness of the plate whereby they are to a great extent absorbed. And for this reason the activity of the plate, when it was turned over, was only one-third of the activity measured from the upper side.

5. *Diffusion of UrX.*

The results obtained in the preceding section can now be used to explain the first rapid decay of β activity of uranium nitrate after crystallization from the water solution.

We saw that in the case when uranium nitrate was obtained by evaporation from the solution, and not by crystallization, this first decay was not exhibited. Moreover, it was pointed out that when the hot solution was stirred during crystallization, no increase of activity at the end of the crystallization was observed. It must now be noted that in this case we did not get any decay after crystallization.

We see then that the first rapid decay is the decay of the excess of activity produced artificially by crystallization, when the latter caused the uneven distribution of UrX throughout the plate.

This suggests the probability that the decay of β activity in the first days after crystallization is produced by the diffusion of UrX from the upper layers of the plate, where it was in greater concentration, to the lower, where its concentration is smaller. Thus, if we observe the decay of activity when the upper surface is turned to the electroscope, we should expect to see the analogous increase when the bottom of the plate faces the electroscope. Experiment showed this to be the case. Some of the experimental results are shown in Table IV., where T gives the time reckoned in hours from the moment of crystallization.

TABLE IV.

T.	β activity of the plate when turned with the upper surface to the electroscope.	T.	β activity of the plate when turned with the lower surface to the electroscope.
0	1840	0.1	528
16	1310	16	747
42	1030	42	731
68	1010	68	950

The same experiment made with a very thin plate.

0	760	0.1	570
1	740	1	600
2	730	2	620
3.7	720	3.6	630
4.5	700	4.5	650
23	690	23	680

In order to completely establish that we here have to do with the diffusion of UrX through the plate from the layers

of higher to lower concentration, the following experiment was made.

Fifty grams of uranium nitrate were treated with ether and from the remaining ether solution 15 grams were obtained consisting of uranium nitrate but almost completely freed from UrX . When the ether had been evaporated, some drops of nitric acid were added to the uranium nitrate and this was dissolved in hot water. The solution was evaporated till it lost all the excess of water, and then was kept at the temperature of the room for some minutes while it crystallized forming a dry plate of crystal. The β activity of this plate measured 65.

In the other vessel 25 gr. of uranium nitrate were heated on the water-bath till it melted in its water of crystallization. This solution was then taken off the water-bath, and when the crystallization started 9 gr. of the hot solution were poured on the surface of the first plate of crystal. The solution crystallized then in a few minutes, forming the upper layer of the former plate.

In this manner a plate was made artificially which did not contain in the lower layers any UrX at all, but on the surface it did contain an excess of UrX .

The plate was cut off from the dish and the activity from both surfaces was measured. The results are shown in the table, where T gives the time in hours after the crystallization.

TABLE V.

T(in hours).	Activity of the plate when turned with the upper surface to electroscope.	T(in hours).	Activity of the plate when turned with the lower surface to electroscope.
0	1035	0.1	196
2.5	865	0.8	221
18	665	2.5	239
20.7	637	18	367
27.5	619	20.7	417
43.3	585	27.5	430
		43.7	488

It is seen that the activity measured from the upper surface decreases, and that from the bottom surface increases, both approximating to a common value.

This experiment shows that when we have a plate of crystal of uranium nitrate in which the substance UrX is unequally distributed, UrX diffuses from the places where it is in higher concentration to places where its concentration is lower.

This diffusion of UrX therefore explains the first rapid

decay after crystallization. We see also that the period during which the minimum activity is reached should depend on the thickness of the plate, and such is the case.

6. *The Possible Causes of the Diffusion.*

The question now arises, in what manner and under the influence of what forces does this diffusion take place? Only two explanations appear possible.

It may be supposed that some part of the UrX is dissolved in a small amount of water and diffuses in a state of solution *between* the crystals under the influence of capillary forces. The crystal plates under investigation, however, seemed to be completely dry*, and the diffusion took place even when a part of water of crystallization had escaped from the crystals on the surface. Therefore, the supposition that the UrX diffuses in the state of solution does not seem to be probable.

And if the diffusion does not take place under capillary forces, we are here dealing with a "solid solution." The crystals and the total mass of uranium are the solvent and UrX is the dissolved body. And then the UrX diffuses *through* the crystals from places of the higher to lower concentration.

We define the solution as a mixture of two substances, which is not a mechanical one, but is accompanied by the molecular penetration of both substances.

The process of formation of UrX points to the fact that we really have here a mixture of this kind. An atom of uranium, breaking up by expulsion of one α particle, changes into an atom of UrX, but it always remains surrounded by the other atoms and molecules of uranium. It is not possible to imagine the deeper molecular penetration as existing for the atoms which previously were the atoms of the parent body.

Throughout a given mass of uranium, single atoms of UrX are dispersed. Thus if we consider the total amount of UrX present at a given moment in a given quantity of uranium, we may assume that all this UrX is "dissolved" in the uranium. The observed fact of the diffusion of UrX confirms this supposition. The diffusion of UrX goes in the direction from higher to lower concentration; we may

* Meyer and Schweidler, who first observed this decay of activity after crystallization, due, as we saw, to diffusion of UrX, pointed out that the plates investigated were completely dry (vollkommen trocken). *Loc. cit.* p. 1068.

conjecture from higher to lower osmotic pressure. But this osmotic pressure, whilst it might control the diffusion, cannot be imagined as completely analogous to the osmotic pressure as known in fluid solutions. In the case of extremely weak concentration of UrX , the ordinary osmotic pressure would be a vanishing quantity. But in the present case, the forces which guide the diffusion must be extremely great in order to overcome the immense resistance due to friction. These forces can only result from the reciprocal action between the molecules of the parent body and the atoms of its product, and appear to be a special radioactive type.

Just as UrX is dissolved in its parent uranium, so the other radioactive products should be dissolved in their parent. There are some experimental facts which confirm this supposition. We know that radium and thorium give out a gaseous emanation as one of their successive products. The emanation is produced at a constant rate, which does not depend on any physical or chemical agencies, but the escape of the emanation from the body is variable in character and dependent on different conditions. For instance, radium and most of the compounds of thorium give off little emanation when in a solid state. The emanation is stored in the body itself in considerable amount. We may suppose that in reality just as UrX was dissolved in uranium, so the radium and thorium emanations are dissolved in radium and thorium.

When the parent body is dissolved, the emanation is no longer held bound in the solid solution, and it can readily escape from the water. And it is a fact that all substances have the maximum emanating power when dissolved. The increase of emanating power in presence of moisture can be explained in the same manner.

We know further that generally the solubilities of the gases decrease with the temperature. And indeed the emanating power of almost all radioactive bodies increases when the temperature is raised, reaching a maximum at a dull red heat. At this temperature the solubility should be a minimum and all the emanation should escape.

But the solubility of thorium emanation is not the same in all compounds of thorium. A compound like the hydroxide or carbonate possesses an equal emanating power in the solid state as in solution. This would indicate that thorium emanation, readily soluble in thorium nitrate and soluble in thorium oxide, is not soluble in thorium hydroxide or carbonate. We should then expect that in the last cases the emanating power should not be influenced by variation

of temperature. The experiments of Rutherford and Soddy * show that this is the case.

The existence of "de-emanated" products after strong ignition whereby many physical and chemical properties of the compound are changed, can be also explained by the change in dissolving power of these compounds. Further investigations will show if this generalization of the fact observed in the case of uranium is justifiable.

Further experiments on this subject are in progress. Analogous experiments will also be tried with other radioactive products in order to see whether this explanation is general.

In conclusion I wish to express my best thanks to Prof. Rutherford for the interest he took in this work and for the encouragement I received from him.

McGill University, Physics Building,
April 3, 1905.

VII. *The Boomerang.* By J. W. SHARPE, M.A.†

IT is easy to show in what manner the leading peculiarities of the boomerang's motion depend upon fundamental dynamical principles.

Though the instrument is not perfectly flat, it will be convenient to use the term "principal plane" for its average plane. The C.G. is somewhere within the concavity, and the two horns are symmetrically disposed with regard to it. The normal through the C.G. to the principal plane will be called the axis of the boomerang. The three principal axes of inertia at the C.G. are one along this axis (this one has the maximum moment of inertia), and two others in the principal plane, respectively parallel and perpendicular to the line joining the horns.

Imagine the boomerang held in the right hand, upright, with the concave edge forward and the principal plane vertical, which is in fact the ordinary posture for making a returning throw. The surfaces are both slightly convex, the one next the thrower (to be called throughout the motion the "upper" surface) being always the more curved one of the two. Also the whole instrument has a slight twist in it, so that the "upper" side of the upper horn and the "lower" side of the lower horn are slightly turned towards, and facing, the thrower. This twist is such as to make the boomerang screw itself upwards in the air when thrown with its principal plane horizontal instead of vertical. Its bent form has the

* Rutherford and Soddy, Phil. Mag. April 1903, p. 453.

† Communicated by the Author.

important effect of increasing the moment of inertia about the axis without increasing its weight ; and the swift spin with which it is thrown causes the initial angular momentum of the rotational motion relative to the C.G. (this will be called simply "the angular momentum") to be very large relatively to the total angular momentum which the air reactions can impart during the flight.

Were the air absent the C.G. would describe a parabolic path, and the rotational motion, being a rotation about a principal axis of inertia, would remain entirely unchanged. But on account of its twisted form, and the convexity of its upper and lower surfaces, the boomerang never presents a fair cutting-edge to the air. Consequently the air pressures that are due to the motion relatively to the air continually act upon both horns, always the more effectually upon the one whose rotational motion at the moment conspires with the velocity of the C.G., in such a way as to set up a very small additional angular momentum in each revolution. Of the three components parallel to the principal axes at the C.G. into which this angular momentum may be resolved, the one of which the axis is in the axis of the boomerang merely effects a very slight reduction, per revolution, in the rate of rotation ; but the other two cause a slow alteration, always in the same direction, or rather sense, of the orientation of the axis (barely one complete revolution in an ordinary returning flight of about 9 seconds), without affecting to any appreciable extent the rate of the spin. Consequently the plane of the missile, and therefore also the direction of its flight, undergoes a steady alteration, always in the same sense, *i. e.* that of a right-handed screw about an upward vertical axis. Therefore, as the boomerang must on the whole move edgeways through the air, it constantly steers itself round to the left of an observer standing behind it, from whom it is moving away. At the same time the twist, acting like that of the propeller of a screw-steamer, causes the boomerang to screw itself leftwards and upwards through the air ; for the axis continually rises from its initial, horizontal, position towards the vertical, turning upwards from left to right in front of the thrower, and so sets the principal plane more and more nearly horizontal. The effect of this lifting action is to prolong the flight of the boomerang by maintaining it longer in the air ; and, by raising it to a considerable height, commonly about 30 feet, it makes it possible for the missile to lose, without falling to the ground, a large portion of its forward velocity, even coming almost to rest, the spin nevertheless being well maintained ; for in its

downward flight the C.G. will again recover much of its former speed. When it is approaching the summit of its path, at its greatest elongation from the point of projection, its speed is attaining a minimum ; and therefore, in this portion of the path, the orientation of the principal plane has its maximum rate of change per unit of distance traversed by the C.G. : consequently at this period the steering effect is much more marked than at any other. From this description of the circumstances of the boomerang's flight, it is clear that the reaction of the air sets up a precessional motion of the axis, like that imparted to the Earth's polar axis by the attractions of the Sun and Moon upon the equatorial protuberance, the orientation of the axis being altered by a very small angle, always in the same sense, in each revolution of the body.

If a thin piece of slate be thrown into still water, its motion underneath the surface will give a good illustration of the boomerang's motion in air, the slate steering itself through the water in the same way as the boomerang steers itself through the air.

To explain more particularly the action of the air :— Consider the boomerang just after it has started. It is whirling with great rapidity in a nearly vertical plane, the axis of the whirl being coincident with the maximum principal axis of inertia at the C.G., *i. e.* with the axis of the instrument ; and this axis points leftward, and is very nearly horizontal. Take the motion at an instant when the line joining the horns is vertical, and the convex edge is facing forwards. The upper horn, owing to the twist, has its forward surface slightly turned against the air ; this surface is slightly convex, and the other surface, the hinder one, is still more convex. The result of this is to cause the components, in the principal plane, of the air pressures upon the forward face of the upper horn to be a maximum towards the convex edge, as indeed would be the case even if both surfaces were flat, according to the law of the reaction of a fluid upon a plane moving through it, whereby the plane tends to set itself at right angles to its course, *i. e.* to the path of its centre of inertia. Now resolving the air pressures in three directions, parallel respectively to the principal axes at the C.G., it will be seen that those components which are at right angles to the line joining the horns tend to twist the upper horn backwards, that is they impart a very small angular momentum about that principal axis which is parallel to the line of the horns, and the axis of this angular momentum is directed upwards and outwards from the centre of the boomerang ; and that the action upon the lower horn opposes

the action upon the upper one, and this too in all phases of the rotation. But the components which are parallel to the line of the horns tend to twist the upper horn over, so that its convex edge, which is in the present position the forward one, tends to turn slightly inwards towards the left; and the lower horn experiences a similar and similarly directed action. This effect occurs chiefly just before and just after the line of the horns is at right angles to the path of the C.G. The bend in the boomerang is the chief factor in this effect; but the rounding, *i. e.* convexity, especially that of the "upper" surface, materially assists it. Thus this action upon the two horns imparts to the missile, in each revolution, a very small angular momentum about that principal axis at the C.G. which is at right angles to the line joining the horns, and the axis of this momentum is directed backwards.

Again, the components which are in the direction of the boomerang's axis, and which, on account of the slightness of the twist, are much larger than the others, set up, by their action upon the upper horn, an angular momentum about the same axis as the last set of components, *i. e.* directed backwards in the principal plane; and therefore their effect conspires with that of the last set. In this case, however, the lower horn opposes the upper one. Now the actual speed of the upper horn is the speed of the C.G. plus that due to the rotation about the C.G.; whilst that of the lower one is the difference of these two speeds, the speed due to the rotation being far greater than that of the C.G. Consequently in each revolution the angular momentum imparted by the 1st and 3rd sets of components of the air reactions are not the sum, but the difference, of the momenta imparted to the two horns, the upper horn, *i. e.* the one with the greater velocity, prevailing in this respect over the lower one; and the most important phase of each rotation is that in which the line of the horns is passing through the position in which it is at right angles to the path of the C.G. When this line is nearly parallel to the path, these differential effects hardly exist at all.

Combining all the above described results, it will be seen that the axis of angular momentum (and with this the instantaneous axis of rotation and the axis of the boomerang itself are always either very closely, or exactly, coincident) is in each revolution displaced through a very small angle upwards to the right by the first set of components, and by the other two sets is rotated backwards towards the thrower, the latter effect being decidedly the larger of the two by reason of the superiority of those components which are

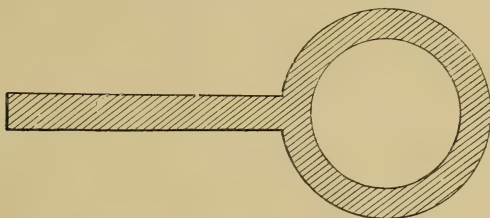
parallel to the axis of the boomerang. This agrees with the result in Mr. G. T. Walker's paper in the Phil. Trans. vol. 190, p. 25, viz.: that the latter is about double the former. From this paper have been taken the descriptive details here given, of the form and length and height of range, of the Australian boomerang.

Considering now, not the rotational motion, but the motion of the C.G. itself, for which purpose the whole mass may be imagined to be condensed into one particle at the C.G., and to be there acted upon by all the air reactions and by its own weight, it will be seen that the third set of components, *i. e.* those parallel to the axis of the boomerang, impel the C.G. in the direction of this axis, at first moving it leftwards and afterwards lifting it in the air; whilst the other two sets, with much less effect, check its flight, and very slightly divert it, at first downwards, and then leftwards. Owing to the bent form of the boomerang, it might be thought that the third set of components would have some slight effect in setting up angular momentum about the principal axis parallel to the line of the horns; but whatever effects in this way they may have will cancel themselves in each complete revolution of the boomerang.

For non-returning flight (see Mr. Walker's paper) the boomerang is thrown with the principal plane leaning a good deal away from the vertical, outwards towards the right. As before, it steers itself at first towards the left, whilst its plane steadily becomes more and more nearly horizontal, the axis leaning more and more over towards the right. When the principal plane has passed the horizontal, and the axis is leaving the vertical, and inclining to the right of it, the boomerang will then begin to steer itself also to the right; and hence the path is now composed of two curves joined at a point of inflexion, the first concave towards the left, and the second concave towards the right. The range is greatly extended by the effect of the twist, through the 3rd set of components of the air reactions, in raising the missile and maintaining it in the air, and also by the peculiar action of the convex form of the surfaces, especially that of the upper surface, which is the more curved of the two. As explained above, this, in conjunction with the bent form of the weapon, through the action of the 2nd set of components, causes the axis to incline backwards towards the thrower, as indeed do also the 3rd set of components; hence the forward edge tends always to lift itself against the air, so that, even if there were no twist, there would nevertheless be a lifting effect arising from the upper, or rightward horn, the speed

of which is always greater than that of the other one, owing to its velocity being the sum of the velocity of the C.G. and of that due to the rotation.

The motion of a cardboard model follows exactly the same principles; but, on account of the flatness of the surfaces and the absence of twist, the precessional motion has to be brought about in a different manner. The shape is not confined to any particular pattern. In fact the following is a most convenient form, yielding curious and complicated flights. It can be cut out from a visiting-card. The head,



which ought to be comparatively large, consists merely of a narrow rim about a large hole; and a long arm is attached to it. This arrangement secures a considerable moment of inertia about the axis without making the weight too great. If supported with its plane sloping upwards, and struck sharply and fairly at the end of the arm so that its edge cuts the air truly, it will rise in front of the thrower without turning either right or left, come to rest, still whirling, at the summit of its path, and return to the point of projection. But if the line of the projecting blow be very slightly inclined towards the plane of the model, the precessional motion will be obtained.

To explain the precessional motion, take a model of the ordinary boomerang form. This is projected with its plane nearly horizontal, and inclined slightly upwards; and it is so struck that the initial axis of angular momentum is not exactly coincident with the axis of the model, *i. e.* with the normal to its plane through the C.G. The consequence is that throughout the motion these two axes, as well as the instantaneous axis of rotation, are never quite coincident, but always very nearly so. In each revolution the axis of angular momentum receives only a very small displacement, whilst the other two circle once round it, the plane of the model always touching the surface of a right circular cone of very flat angle, whose axis is coincident with the axis of angular momentum, and is directed upwards. There is now no screwing effect, like that of the propeller of a screw-steamer; nor is there any assistance to the precessional

motion from convexity of the surfaces; but otherwise the analysis given above for the case of the boomerang will suffice also for this case. In both cases the momental ellipsoid at the C.G. has its least axis normal to the principal plane; and owing to the thinness of the boomerang this axis is very much smaller than the other two. The boomerang, but not the model, is thrown with the axis of angular momentum, and therefore also the instantaneous axis of rotation, initially coincident with this least axis of the momental ellipsoid: and, in the absence of the air, these three axes would remain coincident, and their orientation would remain unchanged, throughout the motion. The actual motion, however, as disturbed by the reaction of the air, *i. e.* the rotational motion, is very stable, by reason of the relative smallness of the minimum axis of the momental ellipsoid at the C.G., and the high rate of spin imparted by the thrower. Those disturbing air couples which are in planes parallel to the principal plane merely reduce the angular momentum without deflecting its axis; but all the other couples, in planes, that is, parallel to the axis of the boomerang, effect in each revolution a very slight displacement of this axis without appreciably altering the actual amount of the angular momentum, or that of the kinetic energy of the rotational motion, because their axes are at right angles to the axis of angular momentum, or, at all events, very nearly so. The invariable plane always touches the momental ellipsoid very near the extremity of the least axis*, and the C.G. remains very nearly at a constant distance from this plane, and the ellipsoid is there much flattened. Consequently the instantaneous axis, the axis of the boomerang, and the axis of angular momentum, are always nearly coincident, the two former following the displacement of the latter.

In conclusion it is interesting to note that similar actions on the part of the air cause rifle-bullets and conical shells to set themselves tangentially to the path of their C.G., instead of keeping their axes unchanged in orientation throughout their motion, which would be the ordinary effect of the coincidence of the axis of figure with that of angular momentum. Take the case of a bullet fired from a barrel with right-handed rifling. The result is just the same with left-handed rifling. The C.G. and the axis of angular momentum are in the axis of figure, the latter being directed forward. Now, owing to the downward concavity of the path of the C.G., the bullet tends continually to meet the air more and more

* Because the angular momentum and the kinetic energy of the rotational motion remain nearly constant.

with the under surface of its conical head. On account of the angular momentum the effect of this is, not to turn the point of the bullet upwards, but to deflect it very slightly to the right (in the case of left-handed rifling, to the left), because the axis of the very small angular momentum impressed upon the bullet in one revolution points horizontally to the right. The bullet now begins to experience an excess of pressure upon its left front, upon the whole in a horizontal plane. Since the angular momentum thus imparted in one revolution has its axis directed vertically downwards (N.B. every diameter of the bullet through the C.G. which is at right angles to the axis is a principal axis of inertia) the bullet is now compelled to turn its point downwards, and to bring its axis into parallelism with the tangent to the path of its C.G.; and the disturbing couples steadily decrease as the axis approaches this position. Should the axis dip below the tangent, similar actions will at once begin to bring it up again. The pressure upon the left front of the bullet must deflect the trajectory slightly to the right.

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VIII. *On the Effect of a Transverse Magnetic Field on the Discharge of Electricity through a Vacuum-Tube.* By JOHN PECK, B.Sc. *

AS is well known, the general effect of a transverse magnetic field on the discharge through a vacuum-tube is to produce an increase in the potential-difference of the electrodes; this being probably due to the deflexion of all the ions to one side of the tube so as to diminish its effective section and thus increase the resistance in the gas.

It has, however, been shown by Dr. Willows † that a transverse magnetic field acting at the cathode reduces the resistance in the tube if the pressure of the gas is below a certain value, although it increases it at higher pressures.

Quite recently ‡ the same investigator has suggested that this reduction of the total resistance in the tube is caused by the magnetic field decreasing the fall of potential taking place near the cathode owing to the deflexion of the cathode rays, which, instead of passing directly through the dark space with great velocity, are now bent back so that they have a much longer path, and therefore a much better chance of producing fresh ions by collision, in the cathode dark space

* Communicated by the Author.

† Phil. Mag. Feb. 1901.

‡ Phys. Proc. Jan. 27, 1905.

than they normally possess. Hence the resistance in the dark space would be diminished.

According to this view, the reduction of the resistance caused by a transverse magnetic field at the cathode depends on the existence of the normal large fall of potential near the cathode; therefore anything which does away with the cathode fall of potential should also eliminate, in whole or in part, the effect noticed by Dr. Willows, and cause a transverse magnetic field in this region either to reduce the resistance less than it generally does or to have no effect, or finally increase it as it does at other parts of the tube.

Now Wehnelt* has found that when the cathode is coated with certain metallic oxides (calcium oxide for example) and then raised to incandescence, the cathode fall of potential is very largely reduced. It therefore seemed desirable to investigate the effect of a transverse magnetic field on the resistance in a vacuum-tube when the cathode was heated and coated with calcium oxide.

The results of the experiments are given below.

The Apparatus.—The vacuum-tube contained air, and was supported vertically with the cathode at the bottom. The cathode consisted of a small inverted **U**-loop of platinum wire joined on to stouter copper leads. The cathode was heated by joining the leads to a battery; an adjustable resistance being placed in the circuit. The platinum was coated with calcium oxide by alternately dipping it into a strong solution of calcium nitrate and heating in the blowpipe-flame. When prepared, the cathode leads were cemented into the lower end of the glass tube with sealing-wax. The lower part of the tube was surrounded with cold water to prevent the wax from melting when the heating-current was passed through the copper leads to the cathode.

An electromagnet was arranged with its pole-pieces level with the cathode, and the direction of the magnetic lines of force was parallel with the plane of the **U**-loop.

The cathode was earthed, and current passed through the tube from an induction-coil.

The variations in the potential-difference between the electrodes of the tube were measured with a Kelvin electrostatic voltmeter. A tube containing xylene was joined up to the induction-coil in parallel with the vacuum-tube, and one terminal of the voltmeter was connected with an end of this tube while the other terminal was connected with the xylene at a suitable point between the ends of the tube so as to give a readable deflexion on the voltmeter. In every

* *Ann. d. Physik*, xiv. 3, pp. 425-468.

case, therefore, only a fraction of the total volts across the ends of the vacuum-tube was measured.

A large number of measurements were made with the cathode at different temperatures, keeping the pressure of the gas and the strength of the magnetic field constant. As the cathode was gradually heated, the reduction in the voltmeter-reading was small at first and altered only slowly, but when a certain temperature was reached a very slight increase in the cathode temperature produced a large fall in the voltmeter-reading. When the cathode was only heated sufficiently to produce a comparatively small decrease in the voltmeter-reading, it was found that turning on the magnet *decreased* the potential-difference between the ends of the tube to a considerable extent, as in Dr. Willows' experiments. Upon making the cathode hotter, the fall of potential produced by the magnet became smaller until it was reduced to zero and finally became a *rise*. The following is a typical series of measurements:—

Pressure.		D.	M.
0.05 mm.	Cathode at dull red heat ...	29½	Drop 11.6
0.05 „	Cathode somewhat hotter ...	41	Drop 10.6
0.05 „	Cathode bright red heat ...	47½	Drop 4.5
0.05 „	Cathode white hot	68	Rise 28.6

Where D = Percentage drop in voltmeter-reading caused by heating the cathode ;

and M = Percentage alteration in voltmeter-reading produced by turning on the magnet, the cathode still being hot.

These results are evidently in agreement with the theory stated above, for they show that the decrease in the resistance caused by a magnetic field at the cathode depends on the existence of a large cathode-fall of potential, and that when this cathode-fall of potential is sufficiently reduced a transverse magnetic field produces an *increase* in the resistance here, just as it does in other regions of the tube.

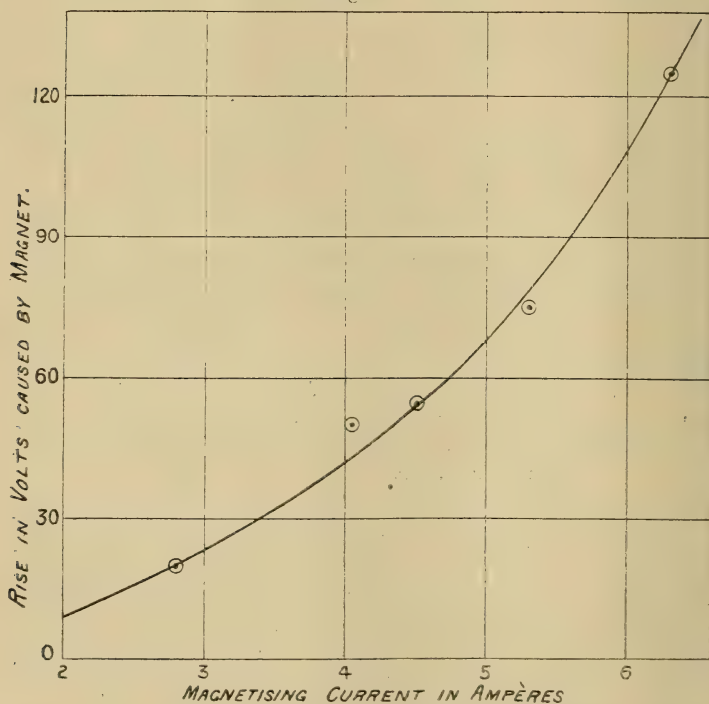
The change in the magnitude of the effect with variations in the strength of the magnetic field was also investigated. The pressure of the gas and temperature of the cathode were kept constant while the current passing round the magnetizing

coils was altered. A typical set of observations is given below:—

Pressure.	Magnetizing current.	Change in Voltmeter Reading caused by putting Magnet 'on.'
0·11 mm.	6·3 amp.	From 225 to 350 = 125 volts Rise.
0·11 „	5·3 „	„ 225 „ 300 = 75 „ „
0·11 „	4·5 „	„ 225 „ 280 = 55 „ „
0·11 „	4·05 „	„ 225 „ 275 = 50 „ „
0·11 „	2·8 „	„ 230 250 = 20 „ „

The relation between the rise in the voltmeter-reading and the magnetizing current is shown graphically in fig. 1.

Fig. 1.



The connexion between the magnitude of the effect and the pressure of the gas was not investigated.

I desire to thank Dr. Willows for suggesting this experiment, and for assistance during its performance.

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IX. *The Negative Results of Second and Third Order Tests of the "Æther Drift," and Possible First Order Methods.* By D. B. BRACE*.

PROFESSOR LARMOR† in his analysis for a system moving through the æther has shown how first and second order effects may be annulled in certain optical and electrical tests of the æther drift. He has not shown, however, how to annul third and higher order effects; but he states, "if indeed it could be proved that the optical effect is null up to the third order, that circumstance would not demolish the theory, but would rather point to some finer adjustment than it provides for: needless to say the attempt would indefinitely transcend existing experimental possibilities"‡.

Attention should therefore be called to the results in my experiment on the double refraction of water moving through the æther. The sensibility attained was such that "the greatest difference in velocity or in index between the two components which could exist, referred to that of water for green light, $\lambda = 0.0005$ cm., was less than 7.8×10^{-13} of the whole. If (10^{-4}) § be taken as the third order magnitude, this result is then easily within the limit. It was not possible to attain this sensibility with a solid—glass; but, since the physical state of the substance does not enter into the theory under consideration, the results for a liquid should be equally valid.

The conclusion as to the amount of double refraction as deduced from what might be expected in comparison with accidental double refraction in a solid—glass, should not be considered as defining the mode and the amount of any double refraction arising from molecular reactions due to a system moving through the æther.

Granting the FitzGerald-Lorentz "contraction-hypothesis," we should have for this experiment a complete correspondence as regards molecular activities between the moving system thus shrunk and the same at rest, up to and including second order quantities, as the analysis of Larmor shows. But the negative results of such a third order test, showing as it does the absence of any difference between the moving and the fixed system, up to and including third order quantities, may

* Communicated by the Author. Read in part before Sect. B, Amer. Assoc. for Adv. Sci., Philadelphia Meeting, 1904-5.

† 'Æther and Matter,' Chap. xi.

‡ Phil. Mag. June, 1904, p. 624.

§ Phil. Mag. April, 1904, p. 318.

indicate a complete correspondence, to all orders, of the molecular phases in the moving and in the fixed systems.

On the other hand, Lorentz* has shown in his analysis for "electromagnetic phenomena in a system moving with any velocity smaller than that of light" that, with the aid of the contraction-hypothesis, many electrical and optical effects will be independent of the motion of the system for all orders. This assumption of a shrinkage, although bold and thus far entirely hypothetical, is not impossible, and is the only suggestion yet made which is capable of reconciling the negative results of second and third order experiments with a quiescent æther. Poincaré† has raised objection to the electromagnetic theory for moving bodies, that each time new facts are brought to light a new hypothesis has to be introduced. This criticism seems to have been fairly met by Lorentz in his latest treatment of the subject. The deductions, however, from his theory make it untenable without further development. The physical consequences, at least, seem at present to be beyond experimental examination. So far no valid reasons have been brought forward which necessitate the shrinkage hypothesis in the electromagnetic theory. In this connexion, reference should be made to the proof which Hasenöhr‡, reasoning from a cyclic process in a moving radiating system, has given, that the second law of thermodynamics is contradicted unless either a second order contraction takes place in the direction of drift or the emission varies with the velocity, which latter he considers impossible.

On the other hand, Abraham§ finds, neglecting fourth and higher order quantities, the ratio of the transverse to the longitudinal mass of the moving electrons to be

$$1 + \frac{2}{5} \left(\frac{v}{V} \right)^2 : 1 + \frac{6}{5} \left(\frac{v}{V} \right)^2 = 1 - \frac{4}{5} \left(\frac{v}{V} \right)^2,$$

while Lorentz requires the ratio to be

$$\left(1 + \left(\frac{v}{V} \right)^2 \right)^{-\frac{1}{2}} : \left(1 - \left(\frac{v}{V} \right)^2 \right)^{-\frac{3}{2}} = 1 - \left(\frac{v}{V} \right)^2,$$

for perfect compensation : thus leaving a double refraction of the order $\frac{1}{5} \left(\frac{v}{V} \right)^2$ to be accounted for, which would have been detected several thousand times over in my experiment. The

* Amsterdam Proc. April 1904, p. 809.

† *Rapports du Congrès de Physique de 1900*, Paris, i. pp. 22, 23.

‡ *Annalen*, Band xiii. p. 367.

§ *Annalen*, Band xiv. p. 236.

analysis of both Lorentz and Abraham seems to be equally consistent with Kaufmann's results on the deflexion of Becquerel rays, Lorentz conforms Abraham's theory to his own, by shrinking the undeformable spherical electrons of the latter into flattened ellipsoids in the line of drift; while Abraham himself shows that these will be unstable unless the greatest axis is in this direction. The latter writer shows* that work must be done against the electrical forces to produce this deformation, so that the total energy in any acceleration is greater than that furnished by the outside forces.

Hence there must be inner forces as well which determine the form of the electron. Thus the hypothesis of Lorentz is incomplete without defining the law of forces further. Hence we must either abandon the contraction hypothesis or modify it. The assumption that the quasi-elastic forces, which maintain the electrons in their positions of equilibrium, experience the same changes as the electrical forces, may possibly be varied, and, together with a modification of the previous hypothesis, be adapted so as to agree with all observations.

While the negative results of the first order experiments involving a study of phase relations between periodic disturbances from the same radiant, optical or electrical, moving with the system, are quite as consistent with a mobile—if we neglect second and higher orders—as with a fixed æther, the explanations of the negative results of second and third order tests are still not in full harmony or free from criticisms, notwithstanding the bold assumption in the premises.

It becomes then a serious question, whether to seek still for decisive results with experiments involving the higher order tests, on the one hand, or direct entrainment tests on the other, in order to settle the question.

The recent repetition by Morley and Miller † of the original Michelson-Morley interference experiment, with a sensibility one hundred times the calculated effect, leaves perhaps no question as to the absence of any such second order optical effect. Lorentz's analysis requires a negative result, likewise, if the rays pass through a transparent substance instead of a vacuum; and it seems desirable therefore that this point should be tested for water, say.

The interferometer method might also be used to test for double refraction if the light were polarized so as to make the electric displacements perpendicular to the plane of the interfering rays, *i. e.* respectively parallel and perpendicular to the

* *Phys. Zeit.* Band v. p. 576.

† *Phil. Mag.* Dec. 1904, p. 753.

greatest axes of the ellipsoidal electrons of the equivalent system at rest. Otherwise the effect would be masked in using natural light, since, with this type of interferometer, the above components may be as low as 25 per cent. of the total interfering light, as Mills has shown*.

The remaining second order tests first proposed by FitzGerald† and developed and carried out by Trouton‡, by examining the couple on a suspended condenser, gave negative results. This is completely explained on the assumption of a shrinkage.

Direct experiments on the entrainment of the æther have also given negative results. Thus Lodge§ sent two interfering rays in opposite directions several times around a rectangle between two rotating steel disks, without being able to detect any displacement of the interference-bands. He estimates that if the disks had communicated one eight-hundredth part of their velocity to the æther, he would have been able to detect it.

Zehnder||, using a different method, attempted to detect any dragging of the æther by a metal plug moving within a cylinder whose ends were circuited together by parallel branching tubes through which two interfering rays could be sent in opposite directions. If the æther had moved entirely with the plug, the effect would have been a thousand times larger than this sensibility. Fizeau's well-known experiment on the entrainment of the æther, repeated by Michelson and Morley¶, showed no effect after allowing for the reaction of the moving water itself upon the interfering waves. Had the æther been carried along completely, the displacement would have been nearly two and a half bands, instead of approximately the single band actually observed, due to the reaction of the water alone. We may conclude from these experiments that the æther was not entrained in any way in the experiments of Morley and Miller, and that their results are therefore valid, although performed within an enclosure.

Nordmeyer**, carrying out the experiment first proposed by Fizeau†† on the change in intensity of a radiant due to the earth's motion, found that this variation could not have been

* *Annalen*, Band xiii. p. 854.

† 'Scientific Writings,' p. 557.

‡ Trouton & Noble, Roy. Soc. Trans. A. 202. p. 165 (1903).

§ Lodge, Roy. Soc. Trans. A. 184. p. 727 (1893).

|| Wied. *Ann.* Band lv. p. 65.

¶ Amer. Journ. Sci. (3) vol. xxxi. p. 377.

** *Annalen*, Band xi. p. 284.

†† Pogg. *Ann.* Band xcii. p. 652.

as great as $1/300000$ *. This agrees with the analytical theory of a quiescent æther, which shows there should be no effect if second order quantities be neglected.

Mascart † and Rayleigh's ‡ negative results on the difference in rotation in quartz with and against the drift are not decisive, since the calculated effect, although not of the second order, is the difference between two first-order effects. A reexamination of the problem with greater experimental refinements should give important results.

With the present uncertainty on both the analytical and the experimental sides, decisive results, which will be free from any hypothetical explanation, seem only possible in the direct comparison of the velocities of light with and against the æther-drift. (Of course if a negative result were obtained, it might be open to such a hypothetical explanation by saying that the group velocity, relative to the medium itself, was a function of the absolute motion of this medium.) Thus Wien § proposes to use two synchronized Foucault mirrors or two Fizeau toothed wheels. This plan is of course of long standing, but has been recently revived. The mechanical difficulties in the way do not give much encouragement to hope for success ; but with present refinements the test is not beyond possibility. The objection which Newcomb and Michelson || have raised to this mode of comparison, that the phases of the synchronizing systems would be affected by the earth's motion in the same way as the propagation of the light, does not seem to be well taken. For, granting a certain phase difference in the rotating-mirror or wheel systems, this difference in phase of the two systems can still be so changed as to give an eclipse, say, along the drift. If now we observe simultaneously the light propagated over the identical path in the opposite direction, there should not be a complete eclipse if the æther were at rest. Any method, therefore, which allows a comparison of two rays, propagated over the same path in opposite directions, is a valid test of the problem. It remains then to devise a method which will certainly show a difference between these two intervals of time equal to one part in ten thousand. The method proposed by Michelson ¶, of

* As this experiment was performed in the latter part of November, when the motion of the solar system has to be subtracted from the earth's motion, this limit is far too high, and the experiment ought therefore to be repeated at some other time of the year.

† *Annales de l'Ecole Normale*, tom. i. p. 157.

‡ *Phil. Mag.* Aug. 1902.

§ *Physikal-Zeit.* Band v. p. 585.

|| Michelson, *Phil. Mag.* Dec. 1904, p. 716.

¶ *L. c.* p. 717.

determining the difference of time required by two interfering rays to traverse circuits in opposite directions, would require a path one kilometre square in a horizontal plane to give a displacement equal to $\cdot 7$ of a band for latitude 45° . This would necessitate a very high degree of refinement indeed over any previous attempts at interference. The method of determining the velocity of light which I devised as far back as 1889, and tried a number of years ago*, and which consisted of an eclipsing system made up of a rotating double mirror and a grating, thus combining the principles of the toothed wheel and rotating mirror methods of Fizeau and Foucault, would be delicate enough to show a variation of one part in ten thousand, providing synchronism could be maintained during a short interval of time. With a suitable system of mirrors and observing telescopes, a single observer would be able to bring into his field of view both beams of light after their passage through the eclipsing systems. If now one of the rotating mirrors were either gaining or losing on the other, the observer would see, alternately, eclipses of one ray and the other, if their times of transit were different. If their times of transit were the same, then the two fields would maintain a constant relative intensity, each going through its maximum and minimum simultaneously as the relative phases of the eclipsing systems varied. The latter would correspond to the condition of a moving, the former to that of a quiescent æther. Thus the experiment would be possible, even if perfect synchronism were not attainable, but only sufficiently so to make the frequency of the successive maxima in the field of view less than a few times a second, or slow enough for the eye to resolve the fluctuations of intensity. If, by means of mirrors, a common source of light were employed, the half-shade principle in the field of view could be used which would be very sensitive in showing any difference in intensity (even if rapidly fluctuating) between the two portions of the field due to any slight difference in the time of propagation of the two rays with and against the æther drift. If we take the conditions in the experiment referred to†, namely, an aperture of 2.5 cm. and a distance of .02 cm. between the lines of the reflecting grating, with a radius of 1 m. and 250 revolutions per second, 10,000,000 eclipses per second could be obtained; and, if we carried this to the limit in speed and resolving power, four times as many

* Vice-Pres. Address, Sect. B, Amer. Assoc. Adv. Sci., Pittsburg Meeting, 1902; Science, July 18, 1902.

† This apparatus was the phototachometer, modified for the purpose, which Newcomb used in determining the velocity of light.

would be possible. With the former conditions, and allowing a frequency in fluctuation of 10 per second for good resolution to the eye, a difference in speed not greater than one part in a million of the two eclipsing systems would be requisite. While this seems extremely small, experience shows that such an approximation is entirely practicable. Thus Newcomb* records a "run" in his measurement of the velocity of light, in which his micrometer showed "beautiful bisection" during the greater portion of the duration (2 minutes) of such a "run." Allowing this setting to one part in ten of his unit, which was $2''\cdot4$, out of a total deviation of $7500''$ of arc for a period of 90 seconds, we have a fluctuation in the speed of only one part in 27,000,000. If the two mirrors could be regulated to this degree, we should still have less than one-tenth the fluctuation of our limit, from the two mirrors combined. Hence, so far as speed regulation is concerned, a much higher eclipse frequency, say the forty-million limit, is possible. The other disturbances would be of the same order as encountered by Newcomb over his total "go" and "return" distance of 5000 m. Supposing the eye could detect a difference in intensity of two per cent. between the two fields (under very favourable conditions this sensibility is one-half of one per cent.), we should need an interval corresponding to one hundred eclipses to detect a change in velocity equal to the aberration constant, since we have to add the effects from each ray. This would mean a distance between the mirrors of 3000 m. for the lower and 750 m. for the higher eclipse frequency referred to, which is much within the distance given above in Newcomb's experiment.

Of the other methods proposed†, several do not require a return of the ray in the determination of the velocity constant. Thus the rotation of a polarizing system, such as a half-shade nicol or tourmaline system, could be carried up to 3000 revolutions per second. Polishing machines are now run up to 2000 revolutions per second. Allowing a sensibility of $0^{\circ}\cdot01$, a distance of 15 kilometres would give a variation in velocity of one part in ten thousand.

The objection to the above methods and other similar ones, is the great distance required. If we could increase the eclipse frequency, the distance could be reduced accordingly. This can be attained by the use of electric oscillations in conjunction with suitable optical systems. Two methods proposed

* Astronomical Papers of American Ephemeris, vol. ii. pts. iii., iv. p. 172.

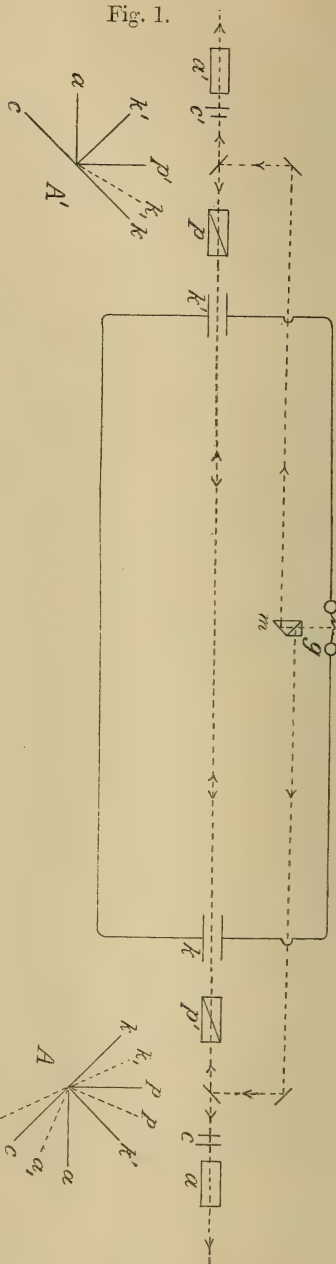
† All the methods referred to in the paper were devised as far back as 1889-90, and have been developed experimentally to a greater or less extent since.

in the article referred to for measuring the velocity of light can be readily adapted to the problem before us. The first depends on the Faraday "effect," the second on the Kerr electrostatic "effect" *. The second method, in conjunction with a half-shade elliptical polarizer †, which I devised several years ago, has given preliminary results indicating a superiority over those described above, quite beyond my expectations, in the arrangement as originally planned for determining the velocity of light a number of years ago.

In fig. 1, k , k' are two condensers containing, say, the dielectric, carbon disulphide or nitrobenzol, giving the Kerr "effect," and placed with their azimuths so as to give a "crossed" system. p , c , a and p' , c' , a' are the polarizer, elliptical half-shade compensator, and analyser of the two respective optical systems placed in juxtaposition. g is the spark-gap and m a half-silvered mirror system for sending identical beams in opposite directions through the optical systems $p' k k' c' a'$ and $p k' k c a$.

A and A' represent the azimuths of the various elements as seen from the one side or the other. Thus at A , p_1 and a_1 are the azimuths of the polarizer and analyser, k and k' the traces of the

Fig. 1.



* Abraham and Lemoine, *J. de Phys.* 1899, p. 366, used an analogous arrangement to determine the retardation of the Kerr and also of the Faraday "effect." † *Phys. Rev.* vol. xviii. p. 70, also vol. xix. p. 218.

planes of the condenser, and c the principal axis of the sensitive strip. Similarly at A' , k_1 , p_1 , a_1 , c_1 are new positions of these elements due to any constant rotation of all of the elements of the second system except k' . If now there be any damping of the electric oscillations, the effect of the one condenser on the polarized ray for, say, the first oscillation, may be made equal to that of the other condenser for the second oscillation by such a rotation, and so on. Thus these two condensers become an equivalent "crossed" system, the one compensating completely the effect of the other, even if the electric stresses in each are not the same. This would hold for electric oscillations which produce successive stresses that are in a constant ratio to each other. If now the interval of passage of the beam of light between k and k' is any multiple of a half period of the vibration, we may obtain compensation, and hence retain the settings for a match in the two half-shade systems c and c' . If there should be any difference in the interval of passage in the two opposite directions, we should not obtain a match in the one if we set for the same in the other, after the frequency had been varied so as to give this exact multiple of the half period in this latter system. This would be determined by noting when the intensity of the field approximated a minimum intensity.

For low frequencies sunlight has been used; but for the higher frequencies this source has not given satisfactory results on account of the very brief duration of the spark in the exciter, and, consequently, the integral time of the electric stresses due to such a discharge. In this case the spark itself may be used to advantage, since here we have a very much greater intensity during the period of the electric stresses. The greater uniformity and intensity of a vapour spark-gap, *e. g.* of mercury, recommend its use in connexion with the half-shade system. To maintain greater uniformity in the amplitude of the oscillations, and hence in the Kerr "effect," a secondary or resonance circuit itself, with its condenser system, may be used, as this will give sufficient double refraction in the dielectric to make accurate settings. Such a half-shade system, where the photometric sensibility is as low, say, as 2 per cent., will show a change of phase of $0.3 \times 10^{-4} \lambda^*$. Since now the maxima of the Kerr "effect" occur every half oscillation, the distance between the condenser k and k' needs only to be equal to the space traversed by the ray in half an oscillation, or $d = \frac{1}{2} L$, where L is the wavelength of an electric oscillation. This would give us a change of phase in the one direction of $0.3 \times 10^{-4} \lambda$ and an

* Phys. Rev. vol. xviii. p. 85.

opposite amount for the reverse direction; or, if we set for a match in the one system, the other system would show a change in phase of $0.3 \times 10^{-4} \lambda \div 2$, which is seven times the required effect 10^{-4} , the aberration constant. With condensers larger than needed for the optical condition, 50,000,000 oscillations have been obtained. Thus

$$d = \frac{1}{2} L = \frac{3 \times 10^8 M}{2 \times 0.5 \times 10^8} = 3M,$$

a distance small enough to allow a rotating mount for the system. Higher frequencies are undoubtedly possible; so that, if the above optical sensibility is not attainable, we can still use a rotating support, with a reduction in this factor of, say, ten times. This method thus contains the requisite conditions, if all the experimental difficulties in connexion with the uniformity of the oscillation and the source of light can be overcome. The present stage of the work seems to warrant this conclusion.

Physical Laboratory,
University of Nebraska, Lincoln.
April 26, 1905.

X. *On the Discharge of Negative Ions* by Glowing Metallic Oxides, and Allied Phenomena.* By A. WEHNELT†.

IN a recently published paper‡ Mr. Owen has investigated the emission of negative ions by glowing Nernst burners both at atmospheric pressure and *in vacuo*. As some time previously to the publication of Mr. Owen's paper I had, in two articles, investigated the discharge of negative electricity by metallic oxides, among which were the oxides composing Nernst burners, perhaps I may be permitted to present here a short account of my experimental results. For details and fuller bibliographical references, the reader is referred to the original articles.

I. *Emission of Negative Ions by Glowing Metallic Oxides at Atmospheric Pressure.*

The metallic oxides to be investigated were in each case supported by carefully cleaned platinum wires of the same length and thickness, which were in turn fixed along the axis of a hollow brass cylinder. The wire and cylinder were enclosed in a glass tube to prevent convection currents of air.

* Negative ion = electron = corpuscle.

† Communicated by the Author. Cf. A. Wehnelt, *Sitzungsber. der physik.-medizinischen Soc. Erlangen*, pp. 150-158 (1903); *Ann. der Physik*, xiv. pp. 425-468 (1904); *Verhandl. d. Deutsch. physik. Gesellsch.* v. pp. 255-258 & pp. 423-426 (1903).

‡ G. Owen, *Phil. Mag.* viii. pp. 230-257 (1904).

The platinum wire was heated by an alternating current from a small transformer having a well-insulated secondary, and could be raised to any desired potential by connecting it to one pole of a high-voltage battery, whose other pole was earthed. The cylinder was connected to earth through a galvanometer.

In the case of a pure platinum wire, as well as in that of one covered with oxide, no well-marked saturation currents are reached at atmospheric pressure. Hence the current was in every instance measured at the same constant potential difference of 1000 volts.

The oxides of the following metals were investigated:—

(1) Barium, strontium, calcium, magnesium, zinc, cadmium, yttrium, lanthanum, zirconium, thorium.

(2) Beryllium, aluminium, thallium, titanium, cerium, iron, nickel, cobalt, chromium, uranium, tin, lead, bismuth, silver, copper.

Oxides belonging to group (2) showed an emission of corpuscles which was either not at all or else only very slightly higher than that of pure platinum, while oxides of the first group showed a considerably stronger discharge of negative electricity. The oxides of calcium, barium, and strontium exhibited an abnormally powerful discharge, and this led me to select these oxides for a more detailed quantitative examination.

Table I. contains the currents which flowed through the

TABLE I.

Relation connecting current (i , in 10^{-8} amp.) with temperature (T), at atmospheric pressure.

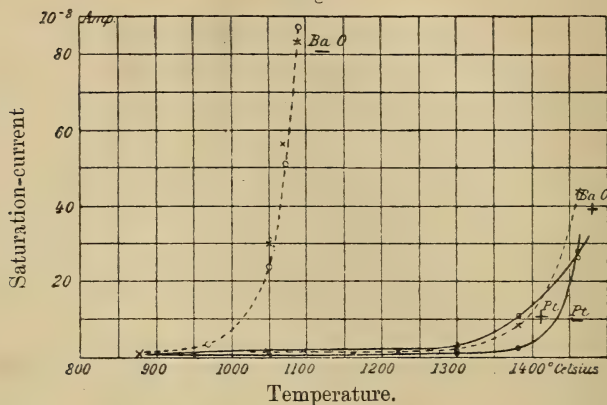
T.	Pure Platinum Wire.		Wire coated with BaO.	
	+ i .	− i .	+ i .	− i .
880	0.95	0.95		
970	...	...	0.95	3.8
1050	1.14	0.95	1.14	23.7
1070	...	...	...	50.7
1090	...	...	...	87.5
1105	...	...	...	200
1120	...	...	...	400
1140	...	...	...	780
1220	1.9	0.95	1.52	
1300	4.75	0.95	2.85	
1380	10.1	2.28	8.56	
1460	26.6	27.6	43.7	

[+ i and − i indicate, respectively, positive and negative potential of the glowing wire.]

galvanometer at various temperatures of the pure or barium-oxide-coated wire when maintained at a potential of ± 1000 volts.

These results are exhibited in the curves of fig. 1. The

Fig. 1.



full-line curves show the connexion between i and T for pure platinum wire. From them it will be seen that for temperatures below 1450° there are sent out more positive than negative ions*, while at higher temperatures the reverse appears to take place.

Much more marked than this slight difference between the numbers of the positive and negative ions in the case of pure platinum is that which occurs when the wire is coated with barium oxide (dotted curves of fig. 1). While the currents corresponding to a positive potential do not appreciably differ from those obtained with a pure platinum wire, the currents with the wire at a negative potential are extraordinarily large even at low temperatures.

Similar results, both qualitatively and quantitatively, were obtained with the oxides of calcium and barium.

II. Emission of Negative Ions by Glowing Metallic Oxides at Low Pressures.

According to the united testimony of all investigators†, a platinum wire heated *in vacuo* emits principally negative ions.

* This result is in accordance with those obtained by other workers. Detailed bibliographical references will be found in J. J. Thomson's 'Conduction of Electricity through Gases,' Chapter viii. (1903).

† J. Elster and H. Geitel, Wied. Ann. xxxvii. p. 315 (1889). J. J. Thomson, Phil. Mag. xlviii. p. 547 (1899). O. W. Richardson, Proc. Cambr. Phil. Soc. xi. pp. 286-295 (1901); Phil. Trans. cci. p. 516 (1903). McClelland, Proc. Cambr. Phil. Soc. xi. p. 296 (1901). H. A. Wilson, Proc. Roy. Soc. lxxii. pp. 242-276 (1903), and Phil. Trans. ccii. p. 252 (1903).

The number of corpuscles emitted by glowing platinum was investigated in detail by Messrs. O. W. Richardson* and H. A. Wilson†, according to whom—as had already been found by J. J. Thomson—the relation between the number of negative ions and the absolute temperature may be represented by an exponential formula, into which the number of corpuscles contained in unit volume enters as a constant. This number, according to Richardson's researches, amounts in the case of platinum to about 10^{21} .

Now I have investigated the emission of negative ions by platinum wires coated with the oxides of calcium, barium, and strontium, and found that, for equal temperatures, the oxide-coated wires emit far more corpuscles than the pure platinum wires.

The experimental arrangements were essentially the same as in the previous investigations, only in this case various leading wires to the electrodes were melted into the glass, and the tube was fused on to a mercury pump having no greased joints‡.

At pressures less than 0.1 mm. of mercury, well-marked saturation-currents were observed which were independent of the pressure. At pressures exceeding 0.1 mm., ionization already occurred by ionic impact, the saturation currents were not well marked, and were in addition dependent on the pressure. Hence, in order to find the relation connecting the saturation current with temperature, pressures less than 0.1 mm. were invariably used.

In what follows, the results will be given for BaO alone, as those obtained with CaO and SrO were very similar, both qualitatively and quantitatively. The results cannot, of course, be expected to be perfectly concordant, as a perfectly uniform coating of the wires with the oxides is unattainable.

Table II. contains the results obtained with BaO; instead of the saturation-current, the saturation current-densities are given—*i. e.*, the currents per unit of area [i cm.⁻²].

TABLE II.—Relation connecting Saturation Current-Density (i cm.⁻²) with Temperature (T). Wire coated with BaO.
 $p=0.04$ mm. of mercury.

T ...	960°.	1050°.	1140°.	1220°.	1300°.	1380°.	1460°.
i cm. ⁻²	$5 \cdot 10^{-7}$	$3 \cdot 8 \cdot 10^{-6}$	$3 \cdot 14 \cdot 10^{-5}$	$2 \cdot 18 \cdot 10^{-4}$	$1 \cdot 6 \cdot 10^{-3}$	$8 \cdot 1 \cdot 10^{-3}$	$2 \cdot 4 \cdot 10^{-2}$

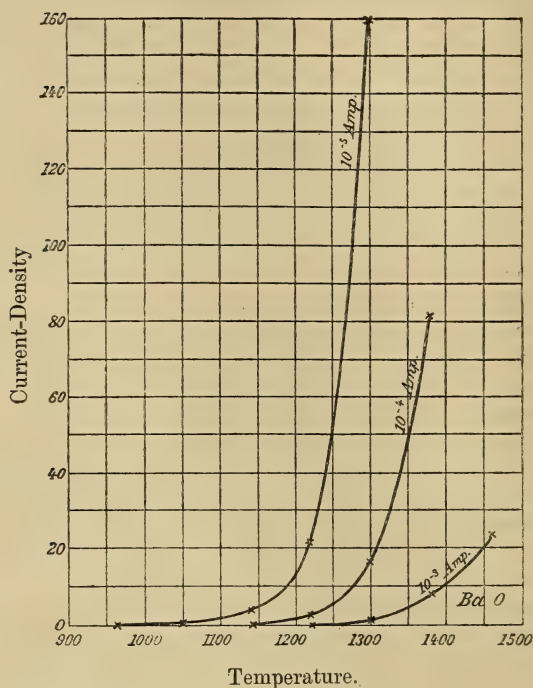
* Loc. cit.

† Loc. cit.

‡ Sealing-wax joints and joints rendered air-tight by means of grease were avoided, as the vapours of organic compounds are decomposed by the glowing wire, whereby the coating of oxide on the wire is destroyed. The carbides formed cause a black deposit on the walls of the glass tube.

These results are graphically exhibited in fig. 2.

Fig. 2.



On comparing these curves with those obtained by Mr. O. W. Richardson* for pure platinum, it is found that they possess precisely the same character. An application of the test suggested by Mr. O. W. Richardson to my results showed that his exponential formula satisfactorily represents the connexion between the number of corpuscles emitted and the temperature in the case of metallic oxides also.

From the much stronger emission of negative ions by the oxides, I concluded that the latter contain more corpuscles per unit of volume than platinum. A calculation of the number contained in unit volume leads, on the other hand—as has recently been shown by Mr. O. W. Richardson†—to

* O. W. Richardson, Proc. Cambr. Phil. Soc. xi. p. 291 (1901).

† O. W. Richardson, *Jahrbuch für Radioaktivität und Elektronik*, i. p. 313 (1904).

the same value as that for platinum. From this Mr. Richardson concludes that the corpuscles proceed not from the glowing oxide, but from the platinum, and that the action of the oxides is confined to a reduction of the amount of energy required to liberate the corpuscles. Experiments to settle this point are in progress at this Institute.

III. *Glowing Metallic Oxides as Cathodes in Discharge Tubes.*

According to our modern views, the cathode dark space of a glow discharge represents a region poor in negative ions*. If this impoverishment is prevented by introducing into the dark space corpuscles produced by any convenient method, their presence results—as has been shown experimentally by Herr G. C. Schmidt—in a lowering of the potential drop at the cathode.

Now glowing metallic oxides emit, as we have seen, numerous negative ions. Hence, when used as cathodes they should produce a lowering of the cathode drop which increases with increasing temperature.

Experiments on this point confirmed the truth of this conclusion.

In what follows are given the results of quantitative experiments made to determine the connexion between the cathode drop on the one hand, and the current, pressure, and temperature of the metallic oxide cathode on the other.

The spherical discharge-tube used in the experiments had for cathode a platinum wire coated with the oxide under investigation, the wire being heated by an alternating current. The temperature was measured by means of a very thin platinum-platinum-rhodium thermocouple. An exploring electrode near the cathode enabled the cathode drop to be measured. A wire of aluminium served as anode.

Connexion between Cathode Drop and Current.—In the first place, I investigated the dependence of the cathode drop on the current at various temperatures of the cathode. The results for the oxides of barium, calcium, and strontium are almost identical, qualitatively as well as quantitatively, so that I here give, in Table III., only the values obtained with CaO.

* J. J. Thomson, *Phil. Mag.* xlvii. p. 253 (1899). J. Stark, *Ann. d. Physik*, ii. p. 62 (1900). G. C. Schmidt, *Ann. d. Physik*, xii. p. 622 (1903). A. Wehnelt, *Ann. d. Physik*, x. p. 569 (1903).

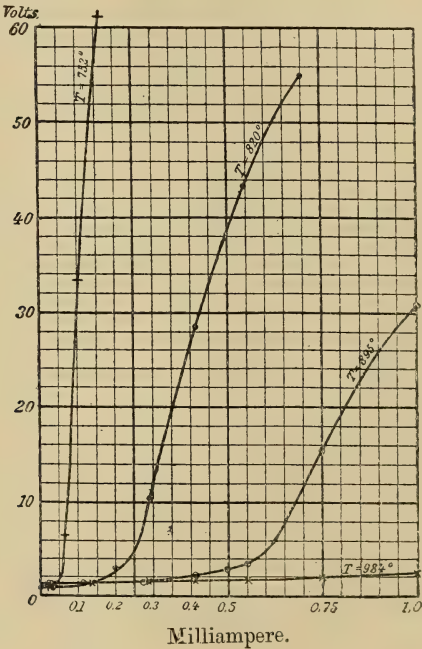
TABLE III.

Relation connecting Cathode Drop (K) with Current (*i*) at a glowing CaO-Cathode, at Constant Temperature (T) and Constant Pressure (*p*).

$T=752^{\circ}$ $p=0.83$ mm. Hg.	$i, 10^{-3}$ amp. K in volts.	0.026 1.2	0.053 3.2	0.057 6.5	0.1 33.7	0.16 61			
$T=820^{\circ}$ $p=0.84$ mm. Hg.	$i, 10^{-3}$ amp. K in volts.	0.035 0.9	0.14 1.36	0.2 2.9	0.28 10.2	0.41 28.4	0.54 43.2	0.68 54.8	1 76.2
$T=895^{\circ}$ $p=0.84$ mm. Hg.	$i, 10^{-3}$ amp. K in volts.	0.025 1.14	0.11 1.53	0.27 1.53	0.41 2.22	0.48 2.95	0.55 3.24	0.68 15.8	1 30.7
$T=984^{\circ}$ $p=0.87$ mm. Hg.	$i, 10^{-3}$ amp. K in volts.	0.028 1.2	0.14 1.5	0.28 1.6	0.41 1.7	0.55 1.8	0.75 1.9	1 2	2.3 25

Fig. 3 exhibits these results graphically. The curves show

Fig. 3.



that up to a certain value of the current, characteristic of each temperature and increasing with rise of temperature, the cathode drop is nearly constant and very small [the small residual potential difference of from 1 to 2 volts between the

exploring electrode and the cathode is, no doubt, due to the fall along the positive column], but that it rapidly increases as soon as the characteristic current is exceeded. This current I have named the *limiting current*.

Since the limiting current increases with rise of temperature I have investigated the relation connecting the two quantities somewhat more closely.

Connexion between Limiting Current and Temperature.—By slowly increasing the current at various temperatures, that value of it was determined beyond which the cathode drop just commenced to increase—*i. e.*, the limiting value of the current. The relations so obtained between the limiting current and temperature are nearly identical, qualitatively as well as quantitatively, for the oxides of calcium, barium, and strontium; hence, in this case also, I give, in Table IV., the results obtained with CaO only.

TABLE IV.

Relation connecting Limiting Current with Temperature
(CaO cathode). p = about 0.01 mm. Hg.

Temperature	945	1012	1075	1133	1192	1252	1313	1367	1485°
Limiting Current, 10^{-3} amp. .	0.1	0.56	1.68	5.6	11.8	26*	35.4†	56.1*	700*
Limiting Current - density, } in 10^{-3} amp. cm. ⁻²	0.24	1.2	3.57	12	25	55	75	120	1500

In the third row of the table are given the limiting current-densities, *i. e.*, the limiting currents per unit of area [i cm.⁻²] of the glowing oxide. As will be seen, the current-density reaches a very large value at high temperatures.

The curves drawn in fig. 4 have been plotted from Table IV. With increasing pressure the limiting current decreases, but otherwise the relation connecting limiting current with temperature remains unaltered†.

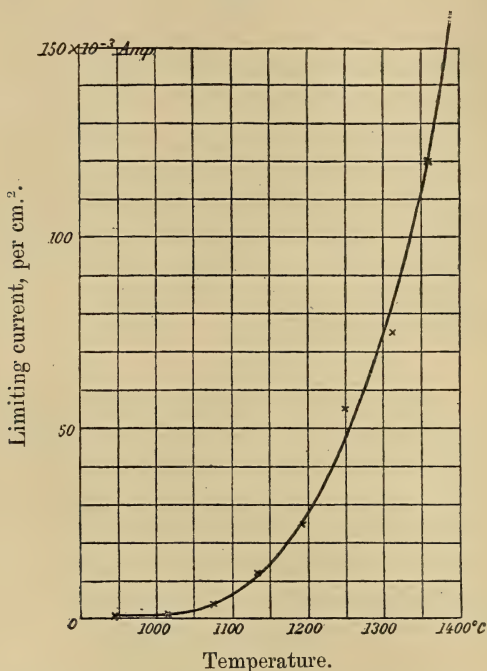
Meaning of Limiting Current.—As already mentioned above, according to modern views the cathode dark space represents a region poor in negative ions. If into it we introduce corpuscles generated by any suitable method, the cathode drop will decrease. If now we increase the current through the discharge-tube, we do away with more negative ions, the

* With large currents, the walls of the tube became strongly heated, so that the pressure could no longer be kept constant, but increased almost to 1 mm.

† A. Wehnelt, *Ann. d. Physik*, xiv. p. 451 (1904).

impoverishment as regards such ions again increases, and hence the cathode drop will increase.

Fig. 4.



Now the glowing oxides emit, as we have seen, numerous corpuscles. If the number emitted exceeds the number absorbed by the passage of the current, there will be no impoverishment, *i. e.*, the cathode drop will be zero. As the current through the tube is increased, it finally reaches a value such that the number of ions absorbed per unit of time by the current becomes exactly equal to the number furnished by the glowing oxide during the same interval. Up to this value of the current, no impoverishment as regards negative ions takes place, so that the cathode drop is zero. If now the current through the tube be further increased, more corpuscles will be absorbed than are generated, and the cathode drop will begin to rise.

Hence the current which I have defined as the *limiting current* is that for which the absorption of negative ions and the impoverishment thereby brought about just ceases to be compensated by the corpuscles emitted by the glowing oxides.

If this view is correct, then the limiting current must increase in proportion to the number of ions emitted by the oxide. But the number of corpuscles emitted by the oxide is directly proportional to the measured saturation current, so that *the limiting current should increase in direct proportion to the saturation current.* An examination of the experimental results from this point of view showed the correctness of this conclusion.

IV. *Applications of Glowing Metallic Oxide Cathodes in the Construction of Discharge Tubes*.*

1. When the glowing oxide cathodes are at a very high temperature, the limiting current-density reaches a value of several amperes per unit of area [cm.^2]. These high limiting current-densities enable us, by the use of glowing oxide cathodes having areas of several square centimetres, to send much stronger currents through the discharge-tubes than has hitherto been possible. This is due to the fact that with ordinary tubes the cathode drop even with feeble currents already far exceeds the normal value of 300 volts. The conditions are essentially different in the case of the strong currents which may be sent through mercury vapour.

The fall of potential along the positive column is, as has been shown by special measurements, under low pressures and within the range of 0.1 to 15 amperes, entirely independent of the current and lies between 1 and 2 volts per cm. More exact measurements on this point are not yet available, as the heating of the walls of the tube by the strong currents liberates a large amount of gas from them, whereby the pressure and with it the potential gradient along the positive column is greatly increased.

The small drop along the positive column, combined with the absence of a drop at the cathode, renders it possible to work fairly long tubes by means of voltages such as are furnished by electric light circuits [110 to 220 volts]. The regular positive layers which are formed with strong currents glow with extraordinary brightness, and promise to supply a valuable aid in the examination of gas spectra in spectrum analysis. The ultra-violet rays might be allowed to escape through quartz windows cemented into the tube.

2. If in a discharge-tube the anode be brought close up to the glowing oxide cathode, the positive column disappears, and there remains only the anode drop of some 20 volts; *i. e.*, a potential-difference of little over 20 volts is sufficient

* The tubes here described are obtainable from E. Gundelach, Gehlberg, in Thuringia.

to send a current through the tube. But if the polarity were reversed, so as to make the glowing oxide electrode the anode, and the cold metal electrode the cathode, then at low pressures several thousand volts would be necessary to send a current through the tube. Hence, if such a discharge-tube be introduced into a low-voltage (110 to 220 volt) alternating current circuit, it will act as a *valve*, allowing only one half-wave of the alternating current to pass through it. The tube may then be used in a manner similar to that of Grätz's aluminium rectifier, for the purpose of charging accumulators, or working induction-coils having electrolytic interrupters, by means of alternating currents.

3. If the limiting value of the current be exceeded in a discharge-tube with glowing oxide cathode, the cathode drop may be given any desired value, and so the cathode rays any desired velocity.

By the use of a cathode consisting of a narrow bare strip of platinum foil, having only a small circular patch (about 1 mm.² in area) of CaO on it, the entire discharge, when the platinum glows, may be made to pass through the CaO patch, so that from this latter there issues a very thin pencil of cathode rays, having a direction normal to the foil. These rays may be given any desired velocity by a proper choice of the temperature and the current. They are extraordinarily bright, so as to be visible even in a fairly large darkened lecture-room.

If the tube be introduced into a uniform magnetic field whose direction is perpendicular to that of the cathode rays as they leave the cathode, the rays are bent into completely closed circles, whose radius of curvature is easily determined. If in addition to this the total discharge potential-difference be measured by means of a voltmeter, and from it there be subtracted 20 volts to allow for the anode drop, the cathode drop becomes approximately known, and thus it becomes possible to demonstrate to a large audience, in a simple and easily intelligible manner, the determination of the ratio of charge to mass $\left[\frac{e}{\mu}\right]$ and of the velocity $[v]$ of cathode rays.

By varying the cathode drop within wide limits (5 to 200 volts or more) it then further becomes possible to demonstrate that the ratio $\frac{e}{\mu}$ remains constant within these limits, while v , on the other hand, increases with increasing cathode drop.

XI. *On the Partition of Energy between Matter and Æther.* By J. H. JEANS, M.A., Fellow of Trinity College*.

§ 1. **T**HE question discussed in the present paper is one which I stated in an earlier paper on the Theory of Gases (Phil. Trans. excvi. p. 397, 1901) as follows:—

“If an interaction between matter and æther exists, no matter how small this interaction may be, the complete dynamical system will consist of the molecules of the gas, together with the æther, and must therefore be regarded as a system possessing an infinite number of degrees of freedom. Applying Boltzmann’s Theorem to this system, we are merely led to the conclusion that no steady state is possible until all the energy of the gas has been dissipated by radiation into the æther. This application of the theorem may or may not be legitimate, but it is, I think, certain that no other application is legitimate.”

The same question has recently been discussed by Lord Rayleigh in ‘Nature’*. In the present paper, I have tried to investigate the legitimacy of applying the theorem of equipartition to a system consisting of both matter and æther, assuming for the present that this matter is in the gaseous state.

Let us, to take the simplest case first, consider a gas of which the molecules are rigid spheres or point centres of force, possessing no internal degrees of freedom, and acting on one another partly through “material” forces and partly through their interaction with the æther, this action being very small except when two molecules are near together. If there are N molecules ($A, B, C, \dots$), we can suppose the configuration of the system to be specified at any instant by

- (i.) $6N$ coordinates $x_a, y_a, z_a, u_a, v_a, w_a, x_b, y_b, \dots$ giving the positions and velocities of the molecules, and
- (ii.) m coordinates $\xi_1, \xi_2, \dots, \xi_m$ (*e. g.* components of electric and magnetic force) giving the state of the æther at every point, these coordinates being independent. We here suppose that any relations which must be satisfied in the free æther (*e. g.*, the vanishing of the divergence of the electric and magnetic forces) have already been taken into account.

* Communicated by the Author.

† April 13th and May 16th, 1905. The present paper was written in March, and was sent to the Phil. Mag. immediately after the appearance of Lord Rayleigh’s first letter. It is therefore not intended as a reply to the second letter, although the questions discussed happen to be much the same. The postscript was, however, written with special reference to the letter of May 16th.

The system is accordingly specified by $6N + m$ Lagrangian coordinates, and the energy is some function of these $6N + m$ coordinates.

§ 2. If there is *no* interaction between matter and æther, this energy can readily be expressed as a sum of squares of these coordinates. The energy is of the form

$$\frac{1}{2}m \sum (u_a^2 + v_a^2 + w_a^2) + f(\xi_1, \xi_2, \dots, \xi_m).$$

The last term represents the energy in free æther which, if we assume the exact linearity of the electromagnetic equations for all electric intensities with which we shall ultimately be concerned, is known to be

$$\frac{1}{8\pi} \iiint (X^2 + Y^2 + Z^2 + \alpha^2 + \beta^2 + \gamma^2) dx dy dz.$$

The energy is not yet expressed as the required sum of squares, for the quantities $X, Y, Z, \alpha, \beta, \gamma$, are not independent Lagrangian coordinates. Let each of the quantities $X, Y, Z, \alpha, \beta, \gamma$, regarded as a function of x, y, z , be expressed in the form

$$f(x, y, z) = \frac{1}{\pi^3} \iiint_0^\infty \iiint_{-\infty}^{+\infty} f(\lambda, \mu, \nu) \cos p(\lambda - x) \cos q(\mu - y) \cos r(\nu - z) \\ dp dq dr d\lambda d\mu d\nu,$$

and let the right-hand integral be further transformed into the sum of an infinite number of terms of the form

$$C_{\sin}^{\cos} (px + qy + rz),$$

or again

$$C_{\sin}^{\cos} \kappa (lx + my + nz),$$

where l, m, n are direction cosines, $\kappa^2 = p^2 + q^2 + r^2$, and C is independent of x, y , and z . The value of any one of the six quantities $X, Y, Z, \alpha, \beta, \gamma$, say the first, can now be expressed in the form

$$X = \iiint_{-\infty}^{+\infty} \iiint_0^{2\pi} (X_1 \cos + X_2 \sin) (x \sin \theta \cos \phi + y \sin \theta \sin \phi + z \cos \theta) \\ d\kappa \sin \theta d\theta d\phi \dots \quad (1)^*$$

where X_1 and X_2 are functions of κ, θ , and ϕ only.

The condition $\frac{\partial X}{\partial x} + \frac{\partial Y}{\partial y} + \frac{\partial Z}{\partial z} = 0$ is now satisfied if

$$X_1 \sin \theta \cos \phi + Y_1 \sin \theta \sin \phi + Z_1 \cos \theta = 0; \dots \quad (2)$$

* Cf. Whittaker's solution of $\nabla^2 V + V = 0$, 'Modern Analysis,' p. 321.

and there is a similar relation for X_2, Y_2, Z_2 , and similarly for $\alpha_1, \beta_1, \gamma_1$ and $\alpha_2, \beta_2, \gamma_2$.

The quantities $X_1, X_2, Y_1, Y_2, \alpha_1, \alpha_2, \beta_1, \beta_2$ can now be taken to be the independent Lagrangian coordinates $\xi_1, \xi_2, \dots, \xi_m$, the values of $Z_1, Z_2, \gamma_1, \gamma_2$ being given by relations of the type (2). Substituting from equations (2) we find that

$$\frac{1}{8\pi} \iiint (X^2 + Y^2 + Z^2 + \alpha^2 + \beta^2 + \gamma^2) dx dy dz$$

becomes a sum of squares of these coordinates.

The total energy is accordingly of the form

$$E = \frac{1}{2} m \sum_N (u_a^2 + v_a^2 + w_a^2) + \sum_m \alpha_1 \xi_1^2.$$

The dynamical equations which regulate the variation of $\xi_1, \xi_2, \dots$ are obtained from the electrodynamical equations

$$\frac{\partial X}{\partial t} = \frac{1}{V} \left(\frac{\partial \gamma}{\partial y} - \frac{\partial \beta}{\partial z} \right), \text{ etc.}$$

As a typical equation we have

$$\dot{X}_1 = \frac{1}{V \cos \theta} \{ (\sin^2 \theta \sin^2 \phi - \cos^2 \theta) \beta_2 + \sin^2 \theta \sin \phi \cos \phi \cdot \alpha_2 \}. \quad (3)$$

If q_1, p_1 are any pair of coordinates of position and momentum of the material system, we have the variation of p_1, q_1 determined by the usual Hamiltonian equations

$$\dot{p}_1 = - \frac{\partial H}{\partial q_1}, \quad \dot{q}_1 = \frac{\partial H}{\partial p_1}, \quad \dots \quad (4)$$

and equations (3) and (4) contain between them the dynamics of the system.

Let us represent the motion in a generalised space corresponding to the $6N + m$ dimensions

$$p_1, q_1, \dots, \xi_1, \xi_2, \dots, \xi_m.$$

If ρ is the density of a fluid moving in this space, the equation of continuity is

$$\frac{1}{\rho} \frac{D\rho}{Dt} + \sum \left(\frac{\partial \dot{p}_1}{\partial p_1} + \frac{\partial \dot{q}_1}{\partial q_1} \right) + \sum \frac{\partial \dot{\xi}_1}{\partial \xi_1} = 0.$$

If the fluid moves in accordance with the dynamical equations, the first sum vanishes as usual by equations (4), and the second sum vanishes by equations (3). Thus $D\rho/Dt = 0$, so that the fluid, if initially homogeneous, remains so through all time.

§ 3. As a result of the assumed absence of interaction between matter and æther, the condition of "continuity of path" is not yet satisfied: the fluid moves as though the space were divided into water-tight compartments. The equations of the divisions between these compartments are simply the integrals of the equations of motion (3) and (4), such, *e. g.*, as the family of surfaces

$$X_1^2 + Y_1^2 + \alpha_2^2 + \beta_2^2 = \text{constant.}$$

If the material system is divided into separate masses of gas, we have as further integrals

$$E_1 = \text{const.} \quad E_2 = \text{const.} \quad \&c.,$$

where $E_1, E_2 \dots$ are the material energies of these systems separately.

§ 4. If we now admit a very small interaction between matter and æther, the conditions are changed. The equations of motion are changed by the introduction of cross-terms which link up the material system with the æther, and we may assume that no integrals are left except the energy equation. Thus the whole system of water-tight compartments is broken down except for the family of surfaces $E = \text{constant}$ where E is the total energy. The new coordinates of position and momentum differ from the old by terms which depend on the interaction just introduced, and so may be regarded as approximately unaltered when this interaction is small. The whole system, regarded now as a single electromagnetic system, will still have equations of motion of the Hamiltonian type, so that the equation $D\rho/Dt = 0$ remains true accurately. The conditions for the theorem of equipartition are now satisfied, so that the expectation of the energy of each of the degrees of freedom is the same, namely

$$\frac{E}{3N + m}.$$

§ 5. When we have a finite amount of matter in infinite æther, N is finite while m is infinite, so that the ratio of energy of matter to that of æther, namely $3N/m$, is infinitesimal.

Definite though this result may seem, the partition of energy is not yet completely known. If we attempt to find the law of distribution of energy in the radiation-spectrum, the equipartition theorem directs us to assign equal amounts of energy to each coordinate, and therefore to each value of k , but gives no information as to the "density" of coordinates within the different range of values of k . This circumstance

suggests that the physical problem is not yet fully threshed out.

§ 6. Let us assume, as a preliminary to carrying the investigation a stage further, that the transfer of energy from matter to æther does not occur at all on the free-paths of molecules, but that vibrations are set up in the æther at collisions. If τ is an average time of duration of a collision, the frequency of the waves "forced" in the æther is less than or comparable with τ , waves of frequencies much greater than τ being, as can readily be shown by analysis which I have already given*, of infinitesimal amplitude. We now see that the ultimate distribution of energy in the spectrum would depend on the temperature from which the material system had started, and the radiation at any instant would depend on the temperature at that instant. A mass of gas might at one time emit a spectrum extending into the ultra-violet; by the time the gas had cooled to half its former temperature, the spectrum would be a heat-spectrum only. The actual temperature at which the light-spectrum would disappear is easily found, from the analysis already quoted, to be comparable with 1000° C.

§ 7. Suppose, however, that we now consider a mass of gas shut up in a perfectly reflecting enclosure. The coordinates of the æther must no longer correspond to plane waves in unlimited æther; they must correspond to the principal vibrations of the æther inside the enclosure. The number of these vibrations is still infinite, but the number of which the frequency is below any given limit is finite. Let us suppose that the average time of a collision is τ , and that of the infinite series n of principal vibrations in the æther, a finite number s have periods which are less than, or at most comparable with τ , the remaining $n-s$ having periods small compared with τ . Then, of the n degrees of freedom of the æther, only s receive any perceptible amount of energy from the molecules at collisions. We may say then that the transfer of energy between the material degrees of freedom and s degrees of æther freedom is comparatively rapid, while that to the remaining $n-s$ degrees is very slow. For an enormous time these $n-s$ degrees of freedom will not receive their due share of the energy, while the energy will rapidly equalise itself between the remaining $3N+s$ degrees of freedom. During this time, the ratio of the energy of the æther to that of the material system is $s/3N$, and this will generally be very small.

§ 8. For instance, let us enclose gas at atmospheric pressure

* 'The Dynamical Theory of Gases,' Chapter IX., or Phil. Mag. August 1903: "On the Vibrations set up in Molecules by Collisions."

and at a temperature of 15° C. in a cubical enclosure of edge equal to l centimetres. For this system $N = 4 \times 10^{19} l^3$ very nearly. The free vibrations of the æther are known (*cf.* Lord Rayleigh's 'Sound,' § 267), each vibration corresponding to values of $X, Y, Z, \alpha, \beta, \gamma$ of the type

$$\frac{\cos}{\sin} \left(\frac{p\pi x}{l} \right) \frac{\cos}{\sin} \left(\frac{q\pi y}{l} \right) \frac{\cos}{\sin} \left(\frac{r\pi z}{l} \right), \dots \dots \dots (5)$$

where p, q, r are integers. These principal vibrations cannot be compounded into plane waves, as was done in § 2 when the space was unlimited.

The frequency κ of this vibration is given by

$$(p^2 + q^2 + r^2) \frac{\pi^2}{l^2} = \frac{\kappa^2}{V^2}, \dots \dots \dots (6)$$

where V is the velocity of light. For air at 15° C. the mean duration of a collision is of the order of 10^{-13} seconds. Let us suppose the s degrees of freedom to consist of all those for which the period is greater than 10^{-14} seconds. The period given by equation (6) is

$$\frac{2\pi}{\kappa} = V \sqrt{\frac{p^2 + q^2 + r^2}{l^2}}.$$

If we take $V = 3 \times 10^{10}$, we find that the upper limit of $\sqrt{p^2 + q^2 + r^2}$ in order that the vibration may have a period of not less than 10^{-14} seconds, is $20000l/3$.

The number of sets of positive integral values of p, q, r for which $\sqrt{p^2 + q^2 + r^2} < \theta$, where θ is large, is approximately $\frac{1}{6}\pi\theta^3$, so that in the present case the number of sets of values of p, q, r is approximately

$$\frac{4\pi \times 10^{12}}{81} l^3.$$

Each system of values of p, q, r gives four principal co-ordinates, so that for our present purpose

$$s = \frac{16\pi \times 10^{12}}{81} l^3.$$

Taking the value $N = 4 \times 10^{19} l^3$, we find

$$\frac{s}{3N} = \frac{4\pi}{243 \times 10^7} = 5 \times 10^{-9} \text{ roughly,}$$

so that the energy of the æther is almost inappreciable, no matter how large the enclosure may be.

§ 9. We can now trace the course of events when one or more masses of gas are left to themselves in undisturbed æther. At first we may suppose that the total energy is entirely that of the principal degrees of freedom. The transfer of energy between the different degrees of freedom of the gas at any point is, as we know, extremely rapid. The first phenomenon, then, is that the energy of these degrees of freedom arranges itself according to Maxwell's Law. The time required is a small fraction of a second. The next phenomenon, at any rate if the masses of gas are small, is an equalization of temperature by conduction through each mass. Simultaneously with this, however, a transfer of energy is taking place between the principal degrees of freedom of the molecules, and the vibrations of low frequency in the æther. This equalises the temperatures of different masses of gas, and endows the æther with a small amount of energy, equal to that of a finite number of molecules of the gas, but small compared with the total material energy. The time required for these phenomena must be measured in minutes, days, or centuries, according to the linear scale of the system. After this, a third transfer of energy begins to show itself, but the time required for this must be measured in millions or billions of years unless the gas is very hot. A transfer takes place between the energy of the principal degrees of freedom of the gas and that of degrees of freedom which may either be in the æther or in the atoms of the gas, but which have the common characteristic that they represent vibrations of high frequency. If the gas is in vacant space, the energy set free streams away into space, but if the whole system is enclosed by an ideal perfectly reflecting boundary, the energy accumulates in the æther.

POSTSCRIPT, added June 7th.

§ 10. As in § 8, the number of degrees of freedom of the æther, of which the frequency is less than k , inside a cube of edge l , is $\frac{2}{3}l^3k^3/\pi^2V^2$. Hence the number of vibrations of frequency between k and $k+dk$ is $\frac{2l^3}{\pi^2V^2}k^2dk$. At absolute temperature T each degree of freedom possesses energy $\frac{1}{2}RT$, where, if the units are those of the C.G.S. system and of the centigrade thermometer, the value of R is 9.3×10^{-17} (cf. "The Dynamical Theory of Gases," § 130). Thus the

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energy per cubic cm. of vibrations of frequencies between k and $k + dk$, is

$$\frac{RT}{\pi^2 V^3} k^2 dk,$$

or, in terms of the wave-length λ in free æther, the energy of wave-lengths between λ and $\lambda + d\lambda$ is

$$8\pi RT\lambda^{-4}d\lambda \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

This is one-eighth of the amount found by Lord Rayleigh ('Nature,' May 16th), but agrees exactly with that given by Planck (Drude's *Annalen*, iv. p. 553) for large values of λ . It seems to me that Lord Rayleigh has introduced an unnecessary factor 8 by counting negative as well as positive values of his integers ξ, η, ζ . From formula (7), it follows that the total energy of radiation at temperature T is

$$8\pi RT \int_{\lambda_0}^{\infty} \lambda^{-4} d\lambda + \int_0^{\lambda_0} f(\lambda, T, t) d\lambda, \quad . \quad . \quad . \quad (8)$$

in which λ_0 is the shortest wave-length for which the vibrations may be supposed to possess their full energy, and the second integral represents the energy of waves of wave-length less than λ_0 , the energy of radiation of these waves being a function not only of T and λ , but also of t , the time which has elapsed since the closing in of the æther. Formula (8) does not, of course, claim to express the partition of energy in the radiation emitted by a hot solid: it is the radiation when a mass of gas has been shut up for time t in a perfectly reflecting enclosure. And the formula applies only to the continuous spectrum of the gas produced by molecular motions; no account is taken of the line spectrum, produced, so far as we know, by atomic vibrations.

XII. On the Radioactive Matter present in the Atmosphere.

By A. S. EVE, M.A., McGill University, Montreal*.

THE presence of radium in the earth, and of the emanation of radium in the atmosphere, has been well established. Exact measurements of the amount of the radioactive substance in the air are, however, needed. Professor Rutherford was kind enough to propose to the writer some methods and experiments by which to estimate the quantities present, and to throw further light on their effects.

The main objects of these investigations were:—

1. To estimate the amount of radioactive matter present in

* Communicated by Professor E. Rutherford, F.R.S.

a known volume of the atmosphere, measured in terms of the mass of radium required to maintain the supply constant.

2. To ascertain if the natural ionization of the air can be entirely attributed to the radioactive matter present in the atmosphere.

3. To determine the rate of formation of ions due to the active matter in the air.

4. To find the distance from which active matter can be collected on a wire maintained at a high negative potential.

I. On the Amount of Radioactive Matter present in the Atmosphere.

If a wire is raised to a high negative potential for two or three hours in the open air, it is known that it collects active matter from the air. The rate of decay of the activity of the deposit thus obtained approximates closely to the rate of decay of the matter similarly collected from the emanation of radium.

It is not necessary to repeat here the summary of evidence collected by Professor Rutherford in the last chapter of 'Radioactivity.' It appears certain that the radium in the earth gives rise to radium-emanation in the atmosphere, and that the emanation in turn disintegrates successively into the three products of rapid decay, radium A, B, C. Bumstead has accurately compared the decay of the active deposit obtained from the air of Newhaven, Connecticut, and has shown that it must be ascribed to radium. He has also observed in the same locality the presence of thorium emanation. It may be of interest to compare the rate of decay of the active deposit obtained from the emanation of radium, as given by Professor Rutherford in his Bakerian Lecture *, with the rate of decay of the active deposit derived from the atmosphere in Montreal, as determined by the present writer.

These results are shown in Table I. and in the curve (fig. 1).

It is possible that some of the observed difference is due to the presence in the air of active matter from radioactive elements other than radium, such as thorium, but in any case the difference is not large, and it may be partly experimental.

In order to measure the quantity of emanation present in a given volume of the atmosphere, a simple method of comparison was employed. A negatively-charged wire was

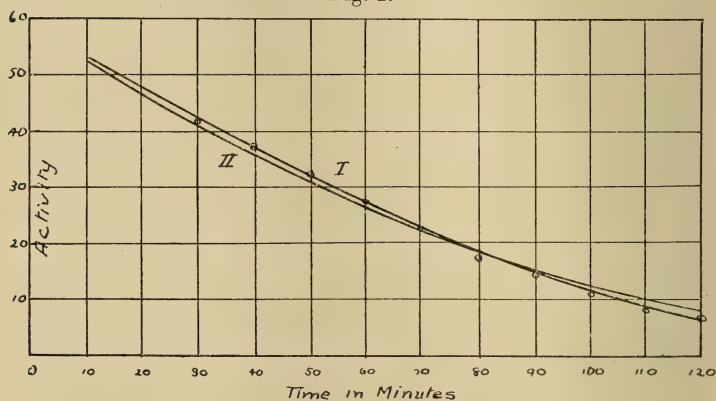
* Phil. Trans. Roy. Soc. ser. A, vol. cciv. pp. 169-219.

used to collect the active deposit from a known volume of air, and the amount was measured by an electroscope. A known weight of pure radium bromide in solution was then

TABLE I.
Decay of Excited Activity.

Time in minutes.	Radium Emanation. Rutherford.	Atmospheric deposit. Eve.	Difference.
10	52	52	
20	45.4	48	+2.6
30	40.4	42.5	+2.1
40	35.6	37.5	+1.9
50	30.4	32.5	+2.1
60	25.4	27.8	+2.4
70		22.8	
80	17.4	18.5	+1.1
90		14.5	
100	11.6	11.0	- .6
110		8.0	
120	7.6	6.6	-1.0

Fig. 1.



taken, and the resulting emanation was collected and transferred to another closed vessel; the excited activity was again obtained on a negatively-charged wire and measured by the same electroscope. In this way the emanation in the air was measured in terms of the amount of radium which would be required to produce it. In all cases the charged wire was exposed for almost three hours, so that practically

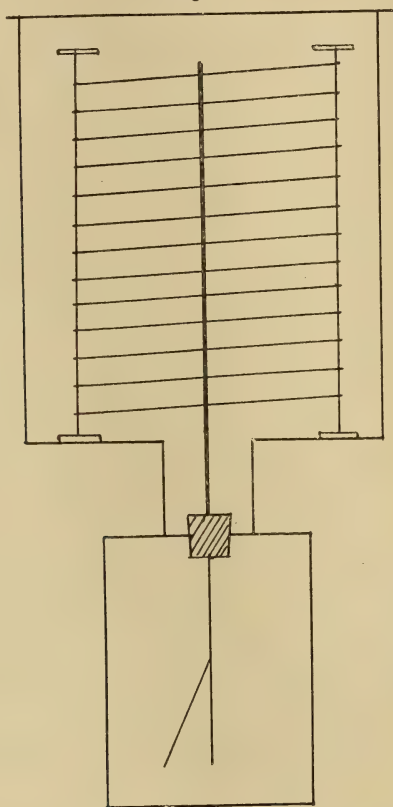
a maximum amount of activity was obtained. A Wimshurst influence-machine, driven by a small electric motor, was used, and the potential of the wire varied from about -9000 to $-11,000$ volts. It was necessary to work with a large vessel in a building into which no radioactive matter had been introduced. The conditions were satisfied by the Engineering Building at McGill University, which is situated at a considerable distance from the Physics Building. In the hydraulic laboratory there is a large water-tank 808 cms. high, and 152 cms. square, with a total volume of 18.7 cubic metres. The sides are made of iron 3.5 cms. thick. The tank is filled from time to time with water supplied by the City of Montreal, and derived from the rivers St. Lawrence and Ottawa. The river water is practically free from radioactive matter, nor could any appreciable activity be discovered in the mud and slime deposit left at the bottom. The tank was placed at the writer's disposal by the courtesy of the authorities of the Engineering Building, and a series of experiments were made, extending from November to April. In all cases the activity was measured ten minutes after the Wimshurst machine was stopped. For the sake of simplicity the activity is expressed in terms of the scale-divisions of the reading-microscope used to observe the fall of the gold-leaf of the electroscope. It may be noted that one scale-division was about equal to a fall of 3.6 volts, and that the capacity of the system as shown in fig. 2 was 3.5 E.S. units. The wire, after removal from the tank, was rapidly coiled on a metal reel, somewhat similar to the outer part of a large fishing-reel, and was then placed in a zinc cylinder connected to earth. Along the axis of the cylinder was a brass rod, insulated by a sulphur support, and directly connected with the gold-leaf system of an electroscope placed beneath the zinc cylinder.

The fall of potential per minute, due to the active matter collected on a wire 8 metres long placed in the iron tank, ten minutes after removal, corresponded to 3.6 scale-divisions. This is the mean of the results of a large number of observations, taken on different days, and the readings varied from 2.8 to 4.3. Thus the active matter collected from 18.7 cubic metres of air caused a fall of potential measured by 3.6 scale-divisions per minute.

A relatively small, air-tight, zinc cylinder 154 cms. high and 25 cms. in diameter was then taken, and the emanation derived from 2×10^{-4} mgs. of pure radium bromide was introduced into it. The volume of the cylinder was 76,000 c. cms., and the emanation was distributed through the

contained air by means of convection currents caused by heating the outside of the cylinder. After charging for three hours the wire was removed and wound on the reel,

Fig. 2.



and then placed as before in the zinc cylinder above the electroscope. The activity, ten minutes after the charge stopped, caused a fall of potential of 7.8 scale-divisions per minute. The solution of radium bromide, initially freed from emanation by passing a current of air through it, had been allowed to stand for fifteen hours in a closed vessel before introducing it into the zinc cylinder. Since the constant of change, λ , for radium emanation is about .18, it can readily be shown that only one-tenth of the full supply of the emanation had been obtained from the radium bromide. Thus the total active deposit, which could be obtained from 2×10^{-4} mgs. of radium bromide in radioactive equilibrium,

corresponds to 78 scale-divisions ; and from 1 gramme of radium bromide to 3.9×10^8 scale-divisions.

Hence we can determine the amount of radium bromide required to maintain the supply of emanation contained in 1 cubic kilometre of air at Montreal. For the emanation in the iron tank, whose volume is 18.7 cubic metres, caused a fall of potential corresponding to 3.6 scale-divisions, and 1 gramme of radium in radioactive equilibrium would supply emanation corresponding to 3.9×10^8 scale-divisions ; therefore 1 cubic kilometre of the air contains emanation supplied by .49 gramme of pure radium bromide.

Thus, making certain assumptions, we can form a rough estimate of the probable amount of emanation in the atmosphere. We can first of all calculate the amount of emanation, supposed equal in quantity per cubic metre to that in the air at Montreal, distributed in a spherical shell around the earth 1 kilometre high. Since the surface of the earth is 5×10^8 square kilometres, the amount in this shell corresponds to the emanation released from 2.5×10^8 grammes of radium bromide in radioactive equilibrium.

Several observers have shown that the excited activity at high altitudes is equal to, if not greater than, that on the plains ; and it thus seems probable that this distribution of emanation might extend for at least 10 kilometres. In that case, the emanation in a shell 10 kilometres high must be released from 2.5×10^9 grammes, or 2460 tons.

Now, three-quarters of the surface of the earth is covered with water, and if we further suppose that the emanation arises from the land alone, we obtain one-quarter of the above value, or 610 tons.

This is an estimate of the total amount of radium required to furnish the emanation in the atmosphere over the land of the earth's surface. It is probable that a vastly larger amount exists in the earth, since the greater part of the emanation would be transformed without ever issuing from the earth's surface.

An objection may be raised to the above estimate, inasmuch as the measurements were taken in the building, and not out-of-doors. It was found that air driven from the room through the tank did not affect the result, and the production of ions was proceeding at a very slow rate, as will be seen in Section III., so that there was no reason to suppose that the conditions within the building were different from those outside in respect to the amount of radioactive matter present in the air.

Nevertheless, it was decided to make some measurements

out-of-doors, and for this purpose a large hollow zinc cylinder was used, 610 cms. long and 77 cms. in diameter, having a volume of 2·8 cubic metres, or about one-seventh of that of the large iron water-tank. This cylinder was placed on the College Campus, at some distance from the Physics Building. The active deposit was collected as before on a negatively-charged wire along the axis of the cylinder. When the ends of the cylinder were left open, the amount of observed activity showed the usual variations, and was affected by the velocity of the wind, humidity, and the atmospheric conditions. When the ends were closed, fairly steady results were obtained.

A comparison of the results is given in the following table :—

TABLE II.

	Total activity in scale-divisions.	Activity per cubic metre in scale-divisions.
Zinc cylinder out-of-doors :		
(1) Ends open.....	·34	·12
(2) Ends closed	·15	·054
Zinc cylinder in the Engineering Building	·50	·18
Iron tank in the Engineering Building	3·6	·19

The ratio of the volumes of the tank to the cylinder is 6·7 : 1, and the ratio of the activities is 24 : 1, so that the excited activity collected in a vessel out-of-doors was between one-third and one-quarter of that collected within the closed tank in the Engineering Building. In order to see whether the effect was apparent or real, the wire cylinder was put in the Engineering Building, not far from the water-tank, and the excited activity was then exactly proportional to that obtained in the large tank. No satisfactory explanation of the difference between the values in the building and out-of-doors has at present been found, and the point will receive further investigation. Radioactive matter has not been introduced into the Engineering Building, and it will be shown later that the rate of production of ions, q , was as low as any observed.

If we take the values obtained in the cylinder out-of-doors, we must reduce the estimate of the radium bromide required to supply the emanation, in a shell 10 kilometres high over the land-surface of the earth, from 610 to 170 tons.

II. *On the Collecting Distance of a Negatively-charged Wire.*

An estimate was next obtained of the approximate distance from which excited activity can be drawn from the air to a wire raised to a potential of $-10,000$ volts. On a still day the amount collected on 6 metres of wire was almost the same whether the wire was suspended at some distance from the ground, or was placed within the zinc cylinder with open ends. In the cylinder the potential gradient was steeper, but the wire was to some extent screened from slight air-currents. The collecting distance was therefore approximately equal to the radius of the cylinder, or to 40 cm.

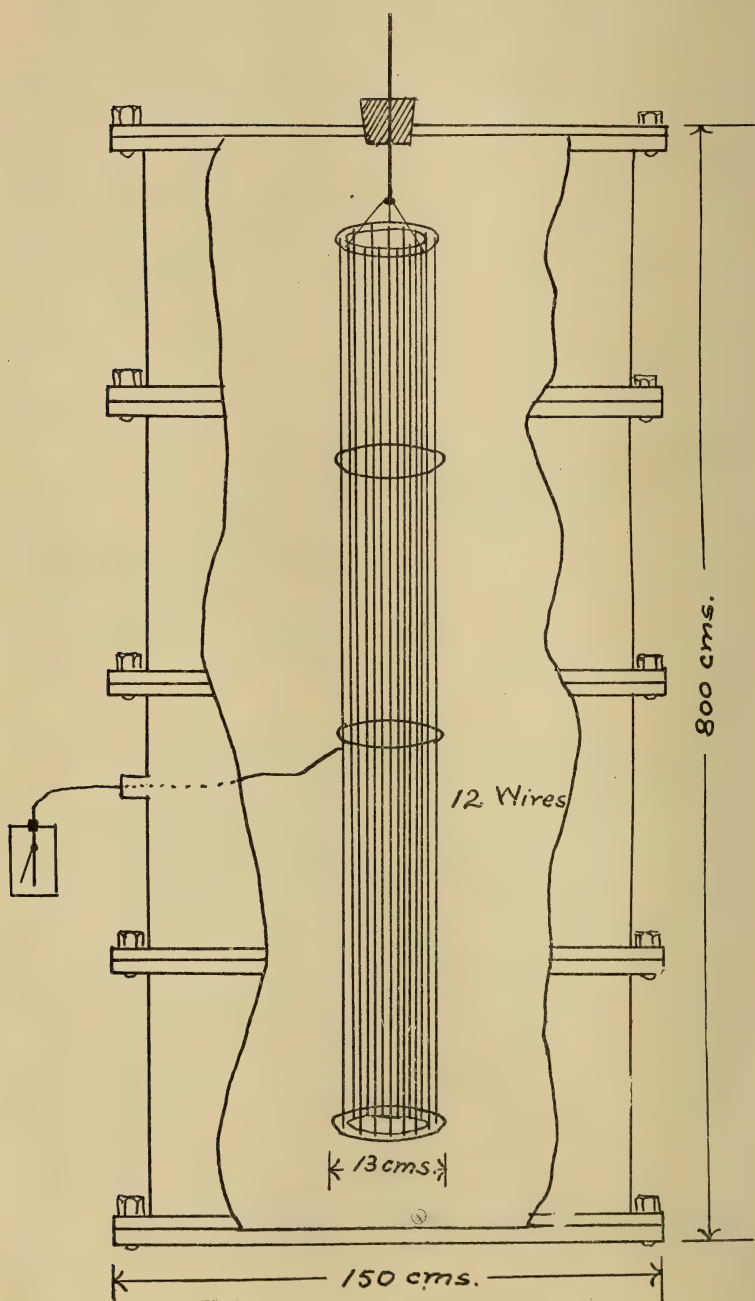
Two wires were then suspended in the air about 90 cms. apart, and the third wire was fixed at a distance of 10 metres from this. It was found that all three wires collected almost the same amount of excited activity, so that the two neighbouring wires were not drawing radioactive matter from overlapping volumes. Similar experiments were made with wires hung in the Engineering Building at various distances from the outside of the tank. The experimental evidence is not easy to summarize, but the general conclusion was obtained that a wire raised to about $-10,000$ volts collected activity from a cylindrical volume of about 40 to 80 cms. radius. This result is not in agreement with previous suppositions, for some observers have wrongly concluded that the collecting distance is very great.

III. *On the Rate of Production of Ions.*

It is important to obtain as many determinations as possible of the value of q , or the rate of production of ions per cubic centimetre of the air. A well-insulated wire was therefore hung down the middle of the large iron tank in the Engineering Building, and the wire was connected to the gold leaf system of the electroscope used throughout the experiments, but from which the zinc cylinder was now removed. On charging the wire to two or three hundred volts, it was found that a saturation current was not obtained. Twelve wires were therefore taken (fig. 3), and fastened to a few zinc disks, 13 cms. in diameter, so that the wires were along the generating lines of a cylinder of that diameter. This system of wires, connected to the electroscope, and charged to 300 volts, was found to give a saturation current.

By this arrangement, the whole tank practically became a very large electroscope, and the discharge of the central wires was due to the radioactive matter present in the air within the tank. The capacity of the system was found by connecting

Fig. 3.



the wires to a condenser of known capacity consisting of two concentric cylinders, and by observing the potential, both before and after the connexion.

The subsequent calculations are similar to those given by Rutherford in 'Radioactivity,' page 72.

Since S , the volume of the tank, equals 18.7×10^6 c.c., and C , the capacity, was 140 E.S. units, the current, i , is equal to CV , when V is the fall of potential per second; q is the number of ions produced per second in a cubic centimetre, and e , the charge on an ion, is taken as 3.4×10^{-10} . But the gold-leaf fell at a rate measured by 2.3 scale-divisions of the microscope in one minute, and a scale-division represented 3.6 volts or $\frac{3.6}{300}$ E.S. units.

Hence

$$i = 6.44 \times 10^{-2},$$

and

$$q = \frac{i}{Se} \\ = 10.1.$$

A large number of similar experiments were made, and the values ranged from 9.1 to 10.2, with a mean value

$$q = 9.6 \text{ ions per c.c. per second.}$$

This is the smallest value yet obtained, and I think that the error is not more than five per cent. H. L. Cooke, using a well-cleaned brass vessel, found $q = 10$ in the Physics Building at McGill University about two years ago. Professor Schuster, in the laboratory at Manchester, found $q = 12$. These small values for q are only obtainable when the instruments employed are well-cleaned, and removed from the neighbourhood of radioactive substance.

IV. *An Estimate of the Total Depth in the Earth from which the Radium Emanation passes to the Atmosphere.*

Professor Rutherford has given in 'Radioactivity' an estimate of a higher limit to the amount of radium which can be present in the earth. If this amount were exceeded, the temperature of the earth would have a value in excess of that observed. He calculates that, on an average, not more than 4.6×10^{-14} grammes of radium can be present per one gramme of the earth's constituents. But the mass of the earth is 6.1×10^{27} grms. Hence the total amount of radium in the earth cannot exceed 28×10^{13} kilos, or 28×10^7 British tons.

The consensus of observers seems to show that the emanation

in the air escapes from the earth; and if we suppose that radium is uniformly distributed to the above amount, we can readily calculate the depth from which the emanation must freely come in order to keep up the supply in the atmosphere.

Let x be the average depth through which that radium is distributed which gives rise to the emanation in the atmosphere. Since 610 tons of radium suffice to produce the amount of emanation over the land of the globe, we have

$$\frac{x \times \pi r^2}{\frac{4}{3} \pi r^3} = \frac{610}{28 \times 10^7},$$

where r is the earth's radius, and since

$$r = 6.4 \times 10 \text{ cins.}$$

$$x = 18 \text{ metres.}$$

If we take the lower estimate of 170 tons of radium, the corresponding depth is about 5 metres.

Emanation arising from greater depths than these would probably disintegrate before reaching the surface of the earth.

IV. *On the Cause of the Natural Ionization observed in the Atmosphere.*

The question arises whether the natural ionization of the air at the surface of the earth is due to the radioactive matter contained in it, and whether this cause will wholly account for the effect, or whether there are other causes at work, known or unknown in character.

Take, for example, the discharge of the cylinder of wires suspended in the large iron tank in the Engineering Building. This wire lost its charge at the same rate whether it received initially a positive or negative charge. The saturation current from the wire to the sides of the vessel was 6.44×10^{-2} E.S. units, and the rate of production of ions was given by $q = 9.6$ per c.c. in a second. Is such ionization of the atmosphere to any extent an inherent property, or would air entirely free from radiating matter, or its influence, cease to produce ions, and would it become a non-conductor? The only ionizing agents under such conditions are (1) radiation due to radioactive matter contained in the air, (2) radiations due to active matter on the surface, or in the material of the sides of the

vessel, (3) penetrating radiation through the sides of the vessel, due to radioactive matter in the surrounding bodies.

In the present experiments, the sides of the tank consisted of iron one inch thick, and were sufficient to cut off all but γ rays coming from without. The deposit on the sides of the tank could not have been appreciably radioactive, or the low values of q (9.6) would not have been obtained. For in no apparatus, however carefully cleaned, has a lower value of the volume ionization been noted. The chief factor, probably the only factor, in producing the discharge of the wire is the emanation in the air, and the successive products of rapid decay. And of this there is undoubted evidence, inasmuch as the excited activity was actually collected on a negatively-charged wire in the tank.

The experiment shows that the radioactive matter is in the tank, and the only question is whether it is sufficient to account for the effect obtained.

In order to test this point, Professor Rutherford proposed to me a new method of directly determining this important point. I venture to describe it at some length, because it is desirable that similar experiments under various conditions should be carried out in other places. Such a radioactive survey of the atmosphere would be of interest at the present time.

If the radioactive matter in the air is the sole cause of the ionization observed, there should be a direct and proportional relation between the excited activity and the ionization; for both these effects must originate from the same cause, namely, the emanation present at any given moment in the mass of air under observation.

The method employed was as follows:—The natural ionization of the air in the tank was observed, and the active deposit in the tank was then collected; the first with a cylinder of wire connected with the electroscope (as in fig. 3), the second on a wire suspended in the tank, and charged to $-10,000$ volts. The wire after removal was wound on the reel, and measured by the same electroscope, as in figure 2.

Thus, if C_1 be the capacity of the cylinder of wires in the large tank, V_1 be the potential fall per minute in the electroscope whose gold-leaf system is connected with the central wires, then $C_1 V_1$ is a measure of the natural ionization.

Again, if C_2 be the capacity of the electroscope, fitted with the upper cylinder and reel as in figure 2, and if V_2 be the fall of potential per minute due to excited activity on a negatively-charged wire, as observed ten minutes from the

cessation of the charge, then C_2V_2 is also a measure of the emanation present in the tank. Therefore, whatever may be the amount of emanation, whether naturally present or artificially introduced, we should have, under all circumstances, a constant value for the following percentage, namely,

$$\frac{C_2V_2}{C_1V_1} \times 100 \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The constancy of the quantity must, of course, depend upon the current being fully saturated, and on the active deposit being completely collected in every case.

If a smaller vessel be now filled with the emanation of radium, so as to give a discharge large compared with that due to the natural ionization, the same value for (1) should be found for the air mixed with emanation in this small tank, as for the natural air in the large iron tank.

The advantages of this method are due to the fact that the measurements are all taken with the same electroscope, and may be expressed in terms of scale-divisions of the same instrument, which need not even be disturbed from its position.

Now, in the actual experiment with the iron tank,

$$C_1 = 140, \quad C_2 = 3.5 \text{ E.S. units.}$$

$$V_1 = 2.17, \quad V_2 = 3.6 \text{ scale-divisions per minute.}$$

$$\therefore \frac{C_2V_2}{C_1V_1} \times 100 = 4.1.$$

This result is the mean of many experiments extended over some months.

The emanation from radium bromide was next blown into a small tank of 80,000 c.c. capacity, and a similar series of experiments were made. C_1 was now equal to 21, and C_2 to 3.5, and the following values of these quantities were obtained for various strengths of the emanation:—

V_2	V_1	$\frac{C_2V_2}{C_1V_1} \times 100$
1.44	4.4	5.5
0.93	2.6	6.0
1.85	6.3	4.9
1.32	5.8	4.4
		Mean..... 5.2

Thus we obtain 4.1 from the natural air in the iron tank, and 5.2 from the air mixed with the radium emanation in the smaller vessel. These figures are of the same order, and the difference between them is probably caused by the difficulty of drawing all the excited activity to a wire charged even to $-10,000$ volts from a tank whose sides measure 150 cms. These results show that the ionization in the large tank was very largely due to the presence of radium emanation, and cannot be ascribed to radiations from the walls of that vessel. But the results obtained were not altered after air had been freely driven through the tank, and we thus come to the conclusion that the rate of production of ions in the air of the tank was probably the same as from the air outside the building; and if such is the case, we have seen that the ionization can mainly be attributed to the presence of radioactive matter in the air itself. Since the activity is due to the emanation which decays to half value in four days, its amount cannot have sensibly decayed in passing from the outside air to the tank.

The results strongly indicate that the radioactive matter in the atmosphere near the earth will fully account for the rate of production of ions observed in it.

V. Summary.

The general conclusions derived from these experiments are as follows:—

(1) An estimate of the amount of radium required to maintain a steady supply of the emanation in one cubic kilometre of air near the earth's surface, lies between .14 and .49 gramme.

(2) This amount of emanation and its successive products cause a production of ions at the rate of about 9.6 per cubic centimetre per second.

(3) The radium emanation in the air is probably sufficient to account wholly for the natural ionization observed in large closed vessels consisting of non-radioactive materials, and for the rate of production of the ions in the atmosphere near the earth.

(4) The collecting distance of a wire charged to $-10,000$ volts is about 40 to 80 cms. The active matter derived from the carriers is not drawn in appreciable quantities from still air at a greater distance.

(5) Assuming that the radium in the earth is equally distributed in amounts sufficient to maintain the temperature gradients actually observed in the earth, then the radium emanation in the air is derived from an average depth estimated to be between 5 and 17 metres.

All these estimates and results are based on the assumption that the conditions at Montreal are normal, and represent the average. It must be remembered that the country was covered with snow from one to three feet deep, and that the temperature was sometimes as low as 10 degrees, or 15 degrees below zero Fahrenheit. But after the snow had melted, and the frost had left the ground, the active deposit on a wire in the zinc cylinder on the College Campus was almost equal in value to that obtained in the depth of winter. Allan also found that the summer and winter values at Montreal showed but slight variation.

It is most desirable that experiments should be repeated elsewhere similar to those described in this paper, particularly that suggested by Professor Rutherford, and described in Section IV. If any investigators have at their disposal a very large tank which can be rendered absolutely air-tight, it would be interesting to observe whether the natural leak of a well-insulated wire suspended in it and connected to an electroscope would fall in a few days at the same rate as the decay of radium emanation. Fresh air could then be introduced from without, and the experiment repeated. Such an investigation would determine absolutely whether the natural ionization is entirely due to the emanation present in the air. There might be a small residual ionization due to the sides of the vessel. A large steam-boiler might serve as a tank. Observations at sea are also needed, both on the rate of production of ions, and on the excited activity which can be collected.

In conclusion, I am indebted and grateful to Professor Rutherford, both for suggesting the experiment, and for his advice, encouragement, and resourcefulness when difficulties occurred.

McGill University, Montreal.
3rd April, 1905.

XIII. *On the Lateral Vibration of Bars of Uniform and Varying Sectional Area.* *By JOHN MORROW, M.Sc. (Vict.); M. Eng. (Liverpool); Lecturer in Engineering, University College, Bristol*.

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I. Introduction.

IN the theory of vibrating rods expressions for the frequency and the form of the displaced bar are obtained from the equation

$$\frac{d^4 y}{dx^4} = \mu^4 y,$$

in which y is the displacement at a distance x from the origin.

The solution is

$$y = A \sin \mu x + B \cos \mu x + C \sinh \mu x + D \cosh \mu x,$$

A, B, C, D being arbitrary constants to be determined by the end conditions.

In the case of a uniform bar supported at each end the numerical solution presents no difficulty. In other elementary cases the results are deduced from the values of the roots of equations which would be troublesome if not previously tabulated.

Lord Rayleigh† has shown that the natural period of a bar may be obtained approximately by simpler means. For example, he supposes that the curve assumed by a clamped-free bar whilst vibrating is the same as that which it would take up if displaced from its position of equilibrium by a lateral force acting on the bar at some point in its length. He shows that the period is given with considerable accuracy if the point of application of the force is one quarter of the length from the free end.

More recently, Garrett ‡ has investigated this subject and,

* Communicated by the Physical Society: read March 10, 1905.

† See Rayleigh's 'Sound,' vol. i. pp. 233-235.

‡ Phil. Mag. Nov. 1904.

from instantaneous photographs of the vibrating bar, has concluded that a better approximation to the centre-line of the bar is obtained if the lateral force is assumed to act at a point one-fifth of the length from the free end.

Rayleigh's method is, of course, applicable to rods under other conditions of support. It rests on the fact* that a close approach to the true period can be obtained by assuming a type of vibration which is admissible as an initial configuration, as a considerable departure from the true type leads to only a small error in the estimate of the frequency.

Garrett's method and results have been further investigated and compared with Lord Rayleigh's by Dr. Chree †, and it is shown that, whilst Rayleigh's depends on a recognized dynamical principle, Garrett's apparently has no such basis.

These simple solutions may be of considerable importance in acoustics and mechanics. They have been employed in problems of the "whirling" and vibration of rotating shafts by Dunkerley ‡ and later by Chree §.

It is the object of the present paper to show that by assuming an equation which completely satisfies the end conditions a better approximation is obtained, and that, by a process of continuous approximation, the vibration curve and the period of the fundamental are given to any required degree of accuracy.

The treatment is capable of very general application, and will be chiefly useful in problems of which the solution has not previously been obtained. The first three cases dealt with will serve as illustrations under various types of terminal conditions. It is a noteworthy fact that, whilst Rayleigh's approximations are necessarily overestimates, the method described below gives initially too small a value for the frequency.

When the density and flexural rigidity of a bar are variable from point to point in its length, its treatment by the ordinary method of analysis presents difficulties which have not yet been completely overcome. Many of the solutions are obtained easily by the method here given, and several special cases are considered.

2. *Approximate Method of Solution.*

If a bar is vibrating so that every point in it has the same period, the ratio of the acceleration to the displacement is constant for all points.

* Rayleigh's 'Sound,' vol. i. §§ 88, 89, 182.

† Phil. Mag. Jan. 1905, p. 134.

‡ Phil. Trans. A, 1894, p. 279.

§ Phil. Mag. May 1904, p. 504.

If y_z be the displacement at a distance z from the point chosen as origin, so that $\frac{d^2 y_z}{dt^2}$ is the acceleration, and if y_1 be the displacement of any chosen point (say that at which the displacement is a maximum), then the above statement may be expressed by the equation

$$\frac{d^2 y_z}{dt^2} \bigg/ y_z = \frac{d^2 y_1}{dt^2} \bigg/ y_1$$

or

$$\ddot{y}_z = \frac{\ddot{y}_1}{y_1} y_z. \quad \dots \dots \dots (1)$$

This is equivalent to the usual relation in frequency problems denoted by

$$y = \cos kt \cdot f(x).$$

Now, if we consider the reversed effective forces due to the acceleration of an increment of the length of the bar, we have, by the ordinary Euler-Bernoulli theory, that the couple at any section in the direction tending to increase $\frac{dy}{dx}$ is equal to the product of the curvature and the flexural rigidity at that section. That is, in view of the approximate straightness of the rod,

$$\frac{d^2 y}{dx^2} = \frac{M}{EI}, \quad \dots \dots \dots (2)$$

where M is the couple due to the reversed effective forces,

E = Young's Modulus,

and I = Moment of Inertia of the cross-section about the neutral axis.

Since M is a function of the mass per unit length, the end forces, and the accelerations, we can express it in terms of $\frac{\ddot{y}_1}{y_1} y_z$ and the density and cross-sectional area of the bar. And assuming for y_z a hypothetical type of vibration satisfying the end conditions, we can insert the value of M thus obtained in the equation (2). Solving for y we have a second approximation to the centre-line of the bar, and can calculate the period by

$$N = \frac{1}{2\pi} \sqrt{-\frac{\ddot{y}_1}{y_1}} \dots \dots \dots (3)$$

The next approximation consists in using this solution for y

to find a more accurate expression for M , inserting this in equation (2) and again solving.

The process can be repeated to any extent, but in common with other methods it neglects the rotatory inertia of the rod.

In the following work the bars will be considered to be of uniform sectional area, density, and flexural rigidity except when otherwise stated.

3. *Clamped-Free Bar.*

Taking first the case of a rod "clamped" or "built-in" at one end and free at the other, let the origin, O , be at the free end.

If ρ = density of material of bar,
 ω = cross-sectional area,

then the Moment of the Inertia forces about X (distant x from O)

$$= \int_0^x \rho \omega \ddot{y}_z (x-z) dz,$$

and therefore the differential equation of the centre line of the beam is, by (1) and (2),

$$-\frac{d^2 y}{dx^2} = \frac{\rho \omega \ddot{y}_1}{EI y_1} \int_0^x y_z (x-z) dz, \quad . \quad . \quad . \quad (4)$$

$\rho \omega$ being taken from under the integral sign on account of the uniformity of the density and sectional area throughout the length.

The object is now to find the solution of this equation in ascending powers of x , and hence to obtain the period of vibration.

Assume a value of y_z which satisfies the end conditions. Thus let

$$y = A + Bx + Cx^2 + Dx^3 + Ex^4.$$

The end conditions at the free end, $x=0$, are

$$\frac{d^2 y}{dx^2} = 0, \quad \therefore \quad C = 0,$$

$$\frac{d^3 y}{dx^3} = 0, \quad \therefore \quad D = 0,$$

$$y = y_1, \quad \therefore \quad A = y_1,$$

and at the fixed end, $x=l$, $y=0$, $\frac{dy}{dx}=0$,

$$B = -\frac{4}{3} \frac{y_1}{l}, \quad E = \frac{y_1}{3l^4},$$

and the equation is

$$y = y_1 \left(1 - \frac{4x}{3l} + \frac{1}{3} \frac{x^4}{l^4} \right). \quad \dots \quad (5)$$

Substituting for y_z in equation (4)

$$\begin{aligned} -\frac{d^2y}{dx^2} &= \frac{\rho\omega}{EI} \ddot{y}_1 \int_0^x \left(1 - \frac{4z}{3l} + \frac{1}{3} \frac{z^4}{l^4} \right) (x-z) \, dz \\ &= \frac{\rho\omega}{EI} \ddot{y}_1 \left(\cdot 5x^2 - \cdot 2 \frac{x^3}{l} + \cdot 01 \frac{x^6}{l^4} \right), \end{aligned}$$

and performing the integrations, evaluating the constants by the end conditions,

$$\begin{aligned} -y &= \frac{\rho\omega}{EI} \ddot{y}_1 \left(\cdot 08194 l^4 - \cdot 112,698,41 l^3 x + \cdot 0416 x^4 \right. \\ &\quad \left. - \cdot 01 \frac{x^5}{l} + \cdot 000,198,41 \frac{x^8}{l^4} \right), \quad \dots \quad (6) \end{aligned}$$

which is approximately the curve of vibration of the bar and gives

$$-y_1 = \cdot 08194 \frac{\rho\omega l^4}{EI} \ddot{y}_1,$$

or from (3)

$$N = \frac{3 \cdot 493}{2\pi} \frac{kU}{l^2},$$

where k = radius of gyration of cross-section,

$U = \sqrt{\frac{E}{\rho}}$ = velocity of transmission of longitudinal vibrations in the rod.

The expression given by Rayleigh may be put in the form

$$N = \frac{3 \cdot 5160}{2\pi} \frac{kU}{l^2},$$

showing that the ratio between the two values of N is 0.9935.

The next approximation gives a still closer value and a ratio of 0.9998. It is obtained by inserting y_z from equation (6) in (4), and using the value just obtained for y_1 , whence

$$\begin{aligned} -\frac{d^2y}{dx^2} &= \frac{12 \cdot 204 \rho\omega}{EI l^4} \ddot{y}_1 \int_0^x \left(\cdot 08194 l^4 - \cdot 112,698,41 l^3 z + \cdot 0416 z^4 \right. \\ &\quad \left. - \cdot 01 \frac{z^5}{l} + \cdot 000,198,41 \frac{z^8}{l^4} \right) (x-z) \, dz \\ &= \frac{12 \cdot 204 \rho\omega}{EI l^4} \ddot{y}_1 \left(\cdot 040972 l^4 x^2 - \cdot 018,783,07 l^3 x^3 + \cdot 00138 x^6 \right. \\ &\quad \left. - \cdot 000,264,55 \frac{x^7}{l} + \cdot 000,002,21 \frac{x^{10}}{l^4} \right), \end{aligned}$$

$$\therefore -y = \frac{\cdot 12204 \rho \omega \ddot{y}_1}{EI l^4} \left(\cdot 663084 l^8 - \cdot 912718 l^7 x + \cdot 341435 l^6 x^2 - \cdot 093915 l^5 x^3 + \cdot 002480 l^4 x^4 - \cdot 000367 \frac{x^5}{l} + \cdot 000002 \frac{x^{12}}{l^4} \right).$$

This is a more accurate expression for the vibration-curve than is (6), but it can be seen that the difference is not great.

$$-y_1 = \cdot 0809227 \frac{\rho \omega l^4}{EI} \ddot{y}_1,$$

and

$$N = \frac{3 \cdot 5153}{2\pi} \frac{kU}{l^2},$$

which is correct to 0.02 per cent. of the value given by exact methods.

4. Bar "Supported" at Both Ends.

This is the case in which the directions at the ends are free, but the ends themselves are constrained to remain at rest by "supporting" forces of the required magnitude.

For a uniform bar these forces must each equal one half of the total force due to the inertia of the bar, that is, taking 0 at one end,

$$\frac{1}{2} \rho \omega \frac{\ddot{y}_1}{y_1} \int_0^l y dx.$$

Equation (2) then becomes

$$EI \frac{d^2 y}{dx^2} = \frac{1}{2} \rho \omega \frac{\ddot{y}_1}{y_1} (l-x) \int_0^l y dx - \rho \omega \frac{\ddot{y}_1}{y_1} \int_x^l y_z (z-x) dz. \quad (7)$$

Taking $y = A + Bx + Cx^2 + Dx^3 + Ex^4 + Fx^5$,

$$x=0, \quad y = \frac{d^2 y}{dx^2} = 0, \quad \therefore A = C = 0$$

$$x = \frac{l}{2}, \quad \frac{dy}{dx} = 0, \quad \therefore 0 = B + \frac{3}{4} D l^2 + \frac{1}{2} E l^3 + \frac{5}{16} F l^4$$

$$x = \frac{l}{2}, \quad y = y_1, \quad \therefore y_1 = B \frac{l}{2} + D \frac{l^3}{8} + E \frac{l^4}{16} + F \frac{l^5}{32}$$

$$x = l, \quad y = 0, \quad \therefore 0 = B l + D l^3 + E l^4 + F l^5$$

$$x = l, \quad \frac{d^2 y}{dx^2} = 0, \quad \therefore 0 = 6 D l + 12 E l^2 + 20 F l^3;$$

and the last four of these give

$$B = \frac{16}{5} \frac{y_1}{l}, \quad D = -\frac{32}{5} \frac{y_1}{l^3}, \quad E = \frac{16}{5} \frac{y_1}{l^4}, \quad F = 0.$$

Hence the type to be assumed as a first approximation is

$$y = \frac{16}{5} \frac{y_1}{l^4} (l^3 x - 2lx^3 + x^4),$$

and (7) becomes

$$\begin{aligned} \frac{d^2 y}{dx^2} &= \frac{\rho \omega \ddot{y}_1}{EI} \left\{ \frac{l-x}{2} \int_0^l \frac{16}{5l^4} (l^3 x - 2lx^3 + x^4) dx \right. \\ &\quad \left. - \frac{16}{5l^4} \int_x^l (l^3 z - 2lz^3 + z^4)(z-x) dz \right\} \\ &= \frac{16}{5} \frac{\rho \omega \ddot{y}_1}{EI l^4} \left(\frac{l^5 x}{10} - \frac{l^3 x^3}{6} + \frac{lx^5}{10} - \frac{x^6}{30} \right). \end{aligned}$$

Integrating and keeping the end conditions in view,

$$y = \frac{16}{5} \frac{\rho \omega \ddot{y}_1}{EI l^4} (\cdot 016 l^5 x^3 - \cdot 0083 l^3 x^5 + \cdot 002,380,953 l x^7 - \cdot 000,595,24 x^8 - \cdot 010,119,051 l^7 x), \quad \dots \quad (8)$$

which is the approximate vibration-curve, and

$$-\frac{\ddot{y}_1}{y_1} = 97 \cdot 1098 \frac{EI}{\rho \omega l^4}.$$

For the second approximation, using (8) in (7) and simplifying as before,

$$\begin{aligned} -\frac{d^2 y}{dx^2} &= \cdot 310,751 \frac{\rho \omega \ddot{y}_1}{EI l^4} (-1 \cdot 02513 l^3 x + 1 \cdot 68650 l^5 x^3 \\ &\quad - \cdot 83 l^5 x^5 + \cdot 19841 l^3 x^7 - \cdot 03307 l x^9 + \cdot 00662 x^{10}) \\ \therefore -y &= \cdot 310751 \frac{\rho \omega \ddot{y}_1}{EI l^5} (\cdot 10386 l^{11} x - \cdot 17086 l^9 x^3 \\ &\quad + \cdot 08433 l^7 x^5 - \cdot 01984 l^5 x^7 \\ &\quad + \cdot 00276 l^3 x^9 - \cdot 00030 l x^{11} + \cdot 00005 x^{12}). \end{aligned}$$

This is a close approximation to the equation of the centre-line, and gives

$$\begin{aligned} -y_1 &= \cdot 0102725 \frac{\rho \omega l^4}{EI} \ddot{y}_1 \\ \therefore N &= \frac{9 \cdot 866}{2\pi} \frac{kU}{l^2}. \end{aligned}$$

The correct value of the numerical constant in the numerator is π^2 , so that the ratio of the two values is 0.9996.

The last expression for the displacement may be conveniently compared with the exact type. Thus it may be written

$$y = Ax \left\{ 1 - \frac{.17086}{.10386} \frac{x^2}{l^2} + \frac{.08433}{.10386} \frac{x^4}{l^4} \dots - \frac{.00030}{.10386} \frac{x^{10}}{l^{10}} + \frac{.00005}{.10386} \frac{x^{11}}{l^{11}} \right\},$$

whilst the method indicated at the commencement of this paper gives, by the insertion of the end conditions,

$$y \propto \sin \frac{\pi x}{l},$$

or

$$y = A'x \left\{ 1 - \frac{\pi^2(x/l)^2}{[3]} + \frac{\pi^4(x/l)^4}{[5]} \dots - \frac{\pi^{10}(x/l)^{10}}{[11]} \right\}.$$

Comparing the coefficients of x^2 , x^4 , &c. inside the bracket shows how the accuracy alters in passing from the lower to the higher powers. For example,

$$\begin{array}{ll} \frac{.17086}{.10386} = 1.645. & \frac{\pi^2}{6} = 1.645. \\ \frac{.08433}{.10386} = .8120. & \frac{\pi^4}{[5]} = .8117. \\ \frac{.00030}{.10386} = .000289. & \frac{\pi^{10}}{[11]} = .000235. \end{array}$$

5. *Free-Free Bar.*

In the case in which both ends are free and no external forces act on the bar, the conditions at each end are

$$y = y_1, \quad \frac{d^2y}{dx^2} = 0, \quad \frac{d^3y}{dx^3} = 0.$$

Assuming

$$y = A + Bx + Cx^2 + Dx^3 + Ex^4 + Fx^5 + Gx^6,$$

the conditions at $x=0$ give

$$A = y_1, \quad C = D = 0,$$

and those at $x=l$ give

$$B = -\frac{1}{2} Gl^5, \quad E = \frac{5}{2} Gl^2, \quad F = -3 Gl.$$

To determine G we have that, since the total force on the bar vanishes,

$$\rho\omega \frac{\ddot{y}_1}{y_1} \int_0^l y \, dx = 0, \quad . \quad . \quad . \quad . \quad . \quad (9)$$

that is

$$\int_0^l (y_1 - \frac{1}{2} G l^3 x + \frac{5}{2} G l^2 x^2 - 3 G l x^3 + G x^4) dx = 0$$

$$\therefore G = \frac{28 y_1}{3 l^6},$$

and hence the equation to be assumed is

$$y = y_1 \left(1 - \frac{14}{3} \frac{x}{l} + \frac{70}{3} \frac{x^2}{l^2} - 28 \frac{x^3}{l^3} + \frac{28}{3} \frac{x^4}{l^4} \right).$$

Equation (2) becomes for this case

$$-\frac{d^2 y}{dx^2} = \frac{\rho\omega}{EI} \frac{\ddot{y}_1}{y_1} \int_0^x y_z (x-z) dz,$$

and substituting for y_z

$$-\frac{d^2 y}{dx^2} = \frac{\rho\omega}{EI} \ddot{y}_1 \int_0^x \left(1 - \frac{14}{3} \frac{z}{l} + \frac{70}{3} \frac{z^2}{l^2} - 28 \frac{z^3}{l^3} + \frac{28}{3} \frac{z^4}{l^4} \right) (x-z) dz$$

$$= \frac{\rho\omega}{EI} \ddot{y}_1 \left(.5 x^2 - .7 \frac{x^3}{l} + .7 \frac{x^4}{l^2} - .6 \frac{x^5}{l^3} + .16 \frac{x^6}{l^4} \right).$$

Integrating twice and evaluating the second constant by virtue of (9)

$$-y = \frac{\rho\omega}{EI} \ddot{y}_1 \left(.001,993,14 l^4 - .00925 l^3 x + .0416 x^4 - .038 \frac{x^5}{l} \right.$$

$$\left. + .0138 \frac{x^3}{l^2} - .00925 \frac{x^6}{l^3} + .00185 \frac{x^{10}}{l^5} \right).$$

and

$$N = \frac{22.399}{2\pi} \frac{kU}{l^2}.$$

The more correct value is 22.373, so that in this case the first approximation gives a ratio of the two values as near as 0.9988.

6. Clamped-Free Bar of Varying Breadth. ($b = Ax$.)

Let b be the breadth of the bar, and d its depth in the plane in which the vibration occurs.

Let $d = \text{constant}$, and $b = Ax$. Equation (4) then becomes

$$-\frac{d^2 y}{dx^2} = \frac{1}{EI} \int_0^x \rho \omega_z \ddot{y}_z (x-z) dz \\ = \frac{2\rho \ddot{y}_1}{Ed^2} \left(x^2 - \frac{2}{3} \frac{x^3}{l} + \frac{1}{21} \frac{x^6}{l^4} \right),$$

by virtue of (1) and (5).

$$\therefore -y = \frac{2\rho \ddot{y}_1}{Ed^2} \left(\cdot 122,619,04 l^4 - \cdot 173,469,38 l^3 x + \cdot 083 x^4 \right. \\ \left. - \cdot 03 \frac{x^5}{l} + \cdot 000,850,34 \frac{x^8}{l^4} \right) \\ -\frac{\ddot{y}_1}{y_1} = 4 \cdot 07767 \frac{Ed^2}{\rho l^4}.$$

In the next approximation the differential equation reduces to

$$-\frac{d^2 y}{dx^2} = \frac{97 \cdot 86408 \rho \ddot{y}_1}{Ed^2 l^4} \left(\cdot 020,436,51 l^4 x^2 - \cdot 014,455,78 l^3 x^3 \right. \\ \left. + \cdot 001,984,13 x^6 - \cdot 000,59523 \frac{x^7}{l} + \cdot 000,007,73 \frac{x^{10}}{l^4} \right);$$

whence

$$-y = \frac{97 \cdot 86408 \rho \ddot{y}_1}{Ed^2 l^4} \left(\cdot 240053 l^8 - \cdot 340800 l^7 x + \cdot 170304 l^4 x^4 \right. \\ \left. - \cdot 072279 l^3 x^5 + \cdot 003543 x^8 - \cdot 000827 \frac{x^9}{l} + \cdot 000006 \frac{x^{12}}{l^4} \right) \\ -\frac{\ddot{y}_1}{y_1} = 4 \cdot 256 \frac{Ed^2}{\rho l^4},$$

$$\text{and} \quad N = \frac{2 \cdot 063}{2\pi} \frac{dU}{l^2}.$$

All clamped-free bars in which the depth is constant and the breadth proportional to the distance from the free end will vibrate with a period which is independent of the breadth at the fixed end (that is of the value of A), as given by the above formula.

7. *Clamped-Free Bar of Varying Breadth.* ($b = Ax^2$.)

Here

$$-\frac{d^2 y}{dx^2} = \frac{12\rho}{Ed^2 x^2} \frac{\ddot{y}_1}{y_1} \int_0^x z^2 (x-z) y_z dz \\ = \frac{\rho \ddot{y}_1}{Ed^2} \left(x^2 - \cdot 8 \frac{x^3}{l} + \cdot 07142857 \frac{x^6}{l^4} \right),$$

$$\therefore -y = \frac{\rho \ddot{y}_1}{E d^2} \left(.098,928,57 l^4 - .143,537,41 l^3 x + 08\dot{3} x^4 \right. \\ \left. - .04 \frac{x^5}{l} + .001,275,51 \frac{x^8}{l^4} \right),$$

and

$$-\frac{\ddot{y}_1}{y_1} = 10.1083 \frac{E d^2}{\rho l^4}.$$

For the second approximation

$$-\frac{d^2 y}{dx^2} = \frac{121.3 \rho \ddot{y}_1}{E d^2 l^4 x^2} \int_0^x z^2 (x-z) (.09892857 l^4 - .14353741 l^3 z \\ + .08\dot{3} z^4 - .04 \frac{z^5}{l} + .001,275,51 \frac{z^8}{l^4}) dz \\ = \frac{.1213 \rho \ddot{y}_1}{E d^2 l^4} \left(8.24405 l^4 x^2 - 7.17687 l^3 x^3 + 1.48809 x^6 \right. \\ \left. - .5 \frac{x^7}{l} + .00966 \frac{x^{10}}{l^4} \right),$$

$$\therefore -y = \frac{.1213 \rho \ddot{y}_1}{E d^2 l^4} \left(.75074 l^3 - 1.09782 l^2 x + .68700 l x^4 \right. \\ \left. - .35884 l^3 x^5 + .02657 x^3 - .00772 \frac{x^9}{l} + .00007 \frac{x^{12}}{l^4} \right) \\ -\frac{\ddot{y}_1}{y_1} = 10.9812 \frac{E d^2}{\rho l^4}.$$

This gives

$$N = \frac{3.313}{2\pi} \frac{dU}{l^2}.$$

For a third approximation the differential equation reduces to

$$-\frac{d^2 y}{dx^2} = \frac{15.984 \rho \ddot{y}_1}{E d^2 l^3} \left(.062562 l^3 x^2 - .054891 l^2 x^3 + .012268 l x^6 \right. \\ \left. - .004984 l^3 x^7 + .000201 x^{10} - .000049 \frac{x^{11}}{l} \right),$$

$$\therefore -y = \frac{.15984 \rho \ddot{y}_1}{E d^2 l^3} \left(.5655 l^{12} - .8275 l^{11} x + .52135 l^3 x^4 \right. \\ \left. - .27446 l^7 x^5 + .02191 l^4 x^8 - .00692 l^3 x^9 \right. \\ \left. + .00015 x^{12} - .00003 \frac{x^{13}}{l} \right).$$

Whence

$$-\frac{\ddot{y}_1}{y_1} = 11.063 \frac{E d^2}{\rho l^4},$$

$$\text{and} \quad N = \frac{3.326}{2\pi} \frac{dU}{l^2}.$$

This is again independent of the breadth of the bar.

8. *Clamped-Free Bar of Varying Depth.* ($d=Ax$.)

The exact form of the equation first assumed for the type of vibration is immaterial to the final result, but, by choosing a suitable equation, the labour involved in obtaining this result to any required degree of accuracy is reduced.

In the present case (as, indeed, in the last two) $\frac{d^2y}{dx^2}$ is not necessarily zero when $x=0$, for there the area of the section of the bar also vanishes.

Hence we may start with an equation

$$y=y_1\left(1-\frac{2x}{l}+\frac{x^2}{l^2}\right), \quad . \quad . \quad . \quad (10)$$

which, inserted in

$$-\frac{d^2y}{dx^2} = \frac{12\rho}{EbA^3x^3} \frac{\ddot{y}_1}{y_1} \int_0^x bAz(x-z)y_z \cdot dz$$

gives

$$-\frac{d^2y}{dx^2} = \frac{\rho\ddot{y}_1}{EA^2} \left(2 - \frac{2x}{l} + \cdot 6 \frac{x^2}{l^2}\right);$$

$$\therefore -y = \frac{\rho\ddot{y}_1}{EA^2} \left(\cdot 48\dot{3} l^2 - 1\cdot 2 lx + x^2 - \dot{3} \frac{x^3}{l} + \cdot 05 \frac{x^4}{l^2}\right)$$

and

$$-\frac{\ddot{y}_1}{y_1} = \frac{60}{29} \frac{EA^2}{\rho l^2}.$$

In the second approximation

$$-\frac{d^2y}{dx^2} = \frac{720}{29} \frac{\rho\ddot{y}_1}{EA^2l^2} \left(\cdot 0805\dot{5} l^2 - \cdot 11x + \cdot 05x^2 - \cdot 01 \frac{x^3}{l} + \cdot 001,190,48 \frac{x^4}{l^5}\right),$$

$$\therefore -y = \frac{72}{29} \frac{\rho\ddot{y}_1}{EA^2l^2} \left(\cdot 1742064 l^4 - \cdot 4468254 l^3x + \cdot 4027 l^2x^2 - \cdot 16 lx^3 + \cdot 0416 x^4 - \cdot 005 \frac{x^5}{l} + \cdot 0003968 \frac{x^6}{l^2}\right),$$

$$\therefore -\frac{\ddot{y}_1}{y_1} = 2\cdot 312 \frac{EA^2}{\rho l^2};$$

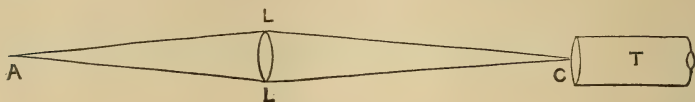
and in the third approximation

$$-\frac{d^2y}{dx^2} = \frac{199\cdot 763}{29} \frac{\rho\ddot{y}_1}{EA^2l^4} \left(\cdot 29034 l^4 - \cdot 37235 lx + \cdot 2014 l^2x^2 - \cdot 05 lx^3 + \cdot 0099 x^4 - \cdot 0001 \frac{x^5}{l}\right),$$

XIV. *On 'An Optical Paradox.'*

By G. JOHNSTONE STONEY, M.A., Sc.D., F.R.S.*

IN the June number of the Phil. Mag. vol. ix. p. 779, Lord Rayleigh describes and discusses an experiment by which he was at first puzzled. The experiment is illustrated by a diagram, which is here reproduced.



A is a point-source of light of nearly wave-length λ , L is a lens forming an image of A upon the middle of C, which last is the objective of a telescope T. The telescope is to be focussed upon lens L; and the boundary of this lens is then the object which will be more or less satisfactorily seen by an observer who looks through the eyepiece of the telescope. The excellent definition of L as seen in the telescope was unexpected. It would seem paradoxical to a person who, remembering that most of the light from A is concentrated within the minute "spurious disk" of its image formed at C, was disposed at first to infer that this little disk may be regarded as determining the effective aperture of telescope T.

Lord Rayleigh interposed a material screen cutting down the aperture of the telescope to nearly the above small size. This greatly impaired the image of L as seen through the telescope; and this satisfied him that the effective aperture of the telescope before the interposition of the screen was not so small as had been supposed. Lord Rayleigh was led by this to make a closer theoretical discussion of the phenomenon, and obtained the definite integral upon which the question turns.

Lord Rayleigh also considers the effect of enlarging A, the source of light; and expresses the opinion that if the enlarged source is a self-luminous body, there will be "no possibility of bringing into play the interferences upon which the advantage of a large aperture depends," inasmuch as with a self-luminous source of light it was supposed that there can be "no phase-relation between the lights which act at different parts of the object-glass." This view, as it appears to the present writer, is based upon an oversight. The interferences upon which resolution depends are of quite another kind as will be explained in a subsequent paragraph,

* Communicated by the Author.

and they in no degree depend upon phase relationships between the puncta of A, the enlarged source of light.

Lord Rayleigh's experiment is one familiar to the present writer, whether made with a collimator, an intermediate lens and a telescope; or with a microscope: he prefers the latter form of the experiment, as it lends itself more conveniently to an investigation of the successive phenomena with the powerful aid of an analysis of the light into its component undulations of flat wavelets. The experiment is one of those which it has been his intention to describe and explain in the next of the series of papers on "Flat-wavelet Resolution" which is being published by the Phil. Mag. The full discussion will be postponed till that paper appears; but some of its results may in anticipation be stated here.

The analysis into undulations of flat wavelets makes it easy to understand the way in which the image of L is formed in telescope T. It is formed by the mutual interference of all the undulations of flat wavelets which advance in the directions indicated by the so-called "rays" that pass into the telescope from the several optical puncta of the image of A produced at C by lens L. In the case where the source of light is a single punctum or physical point, this image consists of a distribution of light upon C which is nearly * Airy's "spurious disk" along with its diffraction appendages.

Telescope T is to be focussed on L, and then, in order that the observer may be able to distinguish between the parts of an object so small as lens L (and, therefore, to exhibit the outline of that object), the analysis indicates that, if A the source of light be a single luminous punctum, the aperture of the telescope-objective C *must take in, in addition to the spurious disk, some part of the appendage rings*; or else portions of two or more of these rings if all light from the spurious disk is excluded.

To exhibit lens L in the most satisfactory way the aperture of objective C must be large enough to include *the spurious disk and all its diffraction appendages* that have any appreciable brightness. If the objective be anything smaller than

* Conceive the lens L to be divided into two lenses L' and L'', of which L' parallelises the light from the point source A, and L'' concentrates this approximately parallel beam to a focus at C. Then the image at C would be exactly Airy's image, if the parallelised beam were an exact beam of parallel light; but this it cannot be, since it can be shown that to form an exact parallel beam would require a small amount of other light than that from A to be incident upon lens L'. Accordingly, the light which is concentrated by L'' upon C is only an approximately parallel beam of light, and the image it forms differs slightly from Airy's image of a star.

this, the definition of the image of L as seen in the telescope will fall short of what the light that passes through lens L is capable of producing; if the aperture of the objective is reduced to so small a size that it admits the light of the spurious disk only and excludes the whole of the diffraction rings, then the definition will have become so bad that even the outline of L cannot be seen. Moreover, we can by this analysis ascertain the poor character of the image which results from employing in its formation the light of the spurious disk and the innermost ring only; as also the greatly improved definition, which is obtained if the aperture of the objective admits the light of the central disk and of two rings; and the slightly further improvement which results from still more enlarging the aperture. All these particulars are easily investigated by employing the analysis into undulations of flat wavelets. A full examination of phenomena of this kind will be given in the next of the Flat-wavelet Resolution papers.

The same method of proof shows that when the source of light is enlarged there is no necessity, such as Lord Rayleigh speaks of, for any phase relation between the portions of light emanating from the several luminous puncta of the source A when enlarged. In fact, the phase relation which needs to be present is of a totally different kind. It is the phase relation which necessarily subsists between the light forming at C the *spurious disk of each separate punctum* of the enlarged A , and the light forming at C the *annular appendages associated with that particular spurious disk*. This is a phase relationship which will prevail, however unrelated may be the portions of light emanating from different puncta of A^* . Accordingly, it may safely be affirmed (and is fully confirmed by experiment) that, without any loss of definition, the source of light may be a self-luminous body. It seems necessary to point this out, because an insufficient appreciation of the way in which images are produced is still prevalent.

In Lord Rayleigh's paper, and in the present paper, the simplest case, viz. where the image of a point-source A falls

* This makes it plain, why when Lord Rayleigh employed as the source of light, light coming through a narrow slit situated at A , he found that the definition of L as seen in the telescope did not suffer, "the vertical parts of the circular edge (parallel to the slit) being as well defined as the horizontal parts." For, in fact, the light issuing from the several puncta of the slit independently formed good images of L , and the simultaneous presence of these independent images of L , made a resultant brighter image and somewhat modified in other respects, but of which the definition was equally good.

centrally upon the telescope objective C, is alone considered. In order to deal adequately with what happens when the source of light though central is of sensible size, the investigation needs to be extended to the case where the image of a point-source falls excentrically upon C. When this is done some instructive results emerge into view.

Lord Rayleigh's experiment, and others related to it, can be most conveniently made with the microscope, which, besides, enables us to experiment with numerical apertures up to 28 times the numerical aperture of an ordinary refracting telescope*.

To see that the experiment can be made with the microscope—Remove lens L of the apparatus represented by the figure in the text, and replace it by two lenses L' and L'', of which lens L' collimates the light from A, while L'' concentrates the collimated beam to a focus at C. It is obviously legitimate to make this substitution. When the experiment is made with a microscope, the source A is to be light passing through a small hole (or slit) in a stop placed under the condenser. The condenser of the microscope then takes up the duties of lens L', and at the same time the objective of the microscope discharges the functions of the combination consisting of lens L'' together with lens C. The image of A produced at C then becomes that image of the hole (or slit) which may be seen in the "Concentration Image" of the microscope—*i. e.*, in the image which comes into view on removing the eyepiece and looking down the microscope tube. Furthermore, when the experiment is made with the microscope, any desired object can be put upon the stage of the microscope and becomes the object to be resolved. So far as the writer knows, far the best object to employ is one of the bands of Grayson's Rulings, supplemented by observations upon a single pair of lines such as may here and there be seen to project from one or other end of a band. The hole in the stop may, if desired, be made to behave as a self-luminous source of light, by focussing the light of the lamp-flame or other luminary upon the stop. This will be found in no degree to impair definition, whether the hole in the stop be large or small.

* The numerical aperture(*i. e.* the sine of half the angular aperture) of the objective of an achromatic telescope is seldom so large as 0.05; while the numerical apertures of dry microscope objectives and condensers range up to 0.95, and the numerical apertures of immersion objectives and immersion condensers range up to 1.4.

XV. *The Determination of the Moment of Inertia of the Magnets used in the Measurement of the Horizontal Component of the Earth's Field.* By W. WATSON, A.R.C.S., D.Sc., F.R.S., Assistant Professor of Physics at the Royal College of Science, London*.

ONE of the constants required when determining the horizontal component of the earth's magnetic field by the ordinary method, is the moment of inertia of the magnet which is used in the vibration experiment. Nearly all the magnetometers which are used in English-speaking lands are tested at the Kew Observatory; and the custom there has been, I believe, to determine the moment of inertia of the cylindrical brass bar supplied with each instrument by calculation from its dimensions, then by measuring the period of the magnet alone and when loaded with this bar to calculate the moment of inertia of the magnet. This method presupposes that the density of the inertia-bar is uniform throughout. Now it is by no means easy to secure a bar of which the density is uniform throughout, and further it is difficult to test whether such uniformity has been secured.

This question as to the uniformity of the inertia-bars supplied with magnetometers has been brought into some prominence lately; for the differences at first obtained when comparing the magnetometers intended for the Indian Magnetic Survey with the Kew standards were finally traced to want of uniformity in the inertia-bars.

It seems to me that more reliable and uniform results would be obtained if a somewhat different procedure were adopted. Namely, to determine once for all, with very great care, the moment of inertia of a standard bar, and then to determine the moment of inertia of the bars supplied with the different magnetometers sent to be tested by comparing them experimentally with the standard bar. Even if the standard adopted were slightly wrong, so that the values given for the bars belonging to the different instruments were uniformly either too high or too low, the confusion caused would be much less than where, as at present, the values obtained may, owing to want of uniformity in the bars, be sometimes too high and sometimes too low.

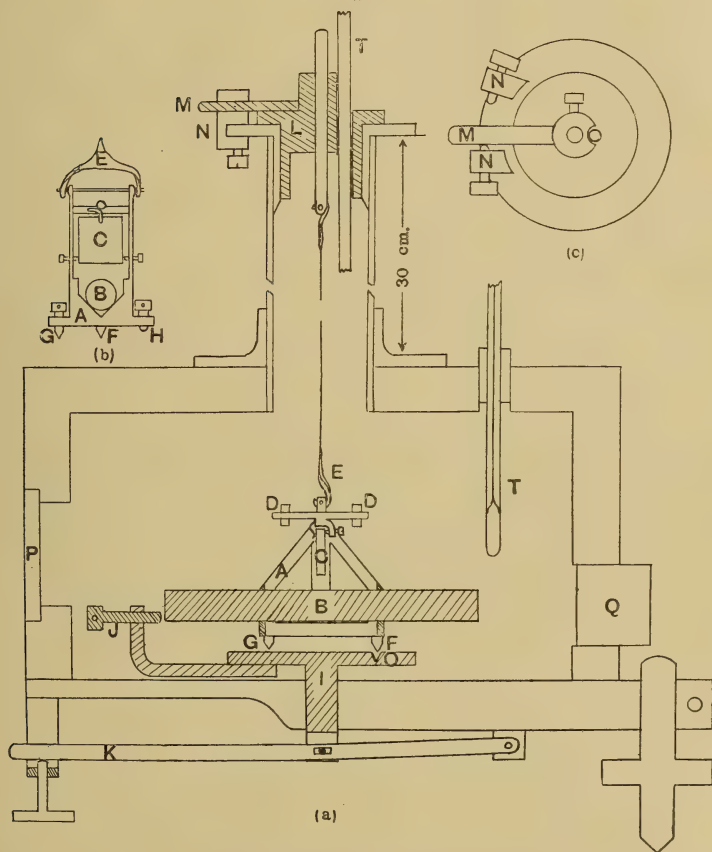
In the following paper is described an instrument suitable for comparing the moment of inertia of bars, together with some experiments made with a view to determining the moment of inertia of a standard bar, and an investigation on the influence of the air on the period.

* Communicated by the Physical Society: read April 14, 1905.

§ 1. *Determination of the Moment of Inertia of a Standard Bar.*

The instrument is shown in fig. 1. It consists of a cradle, A, suspended by a stout quartz fibre (length 30 cms., diameter 0.37 mm.). The bar, B, to be tested rests on this

Fig. 1.



cradle while the position of the cradle is read by reflexion of a scale in the mirror C. A table, I, which can be raised or lowered by a lever, K, has a hole-slot-and-plane on its upper surface, and the three feet, F, G, H, of the cradle rest in these when the table is raised. The conical hole, O, in which the leg F rests is so placed that the cradle when supported on the table lies about half a millimetre to the left of its position when hanging free. The object of this arrangement is that

the position of the bar B, with reference to the cradle, can be adjusted by means of the screw J, and yet, when the cradle is released, the bar may swing clear of the point of this screw. Two small weights D serve to adjust the balance and period of the cradle.

The quartz fibre is soldered to a clip E which hooks on to the cradle and another clip which hooks on to the end of a rod carried by the torsion-head L. An arm, M, attached to the torsion-head can be moved between two adjustable stops N. This arm serves to give the torsion-head a small to-and-fro motion of adjustable and fixed amount, and thus allows of the bar being set swinging.

The bar, &c., is enclosed in a wooden box the sides of which are removable. A window P allows of the mirror being seen; while a hole Q, which can be closed by a cork, serves for the removal of the bar from the cradle. The torsion-head is supported by a brass tube 30 cms. long attached to the top of the box. This brass tube is wound round with a layer of felt to reduce the temperature changes.

The method adopted for adjusting the instrument is as follows. The base of the instrument having been levelled, the cradle is supported on the table with the bar in position, and a striding level is placed on the bar. By adjusting the screws G and H the bar is brought into a horizontal position, and a telescope and scale are set up at a distance of about 70 cms. from the instrument, and so adjusted that the horizontal cross wire coincides with the ends of the division-lines of the scale as seen reflected in the mirror C. The bar is then removed and the cradle released. The weights D are adjusted till the horizontal wire of the telescope again coincides with the images of the ends of the division-lines of the scale. When this adjustment is complete the line joining the V's of the cradle will be horizontal. The bar having been placed in position, the screw J is adjusted till on releasing the cradle the horizontal wire of the telescope again coincides with the ends of the division-lines of the scale, and therefore the axis of the bar is horizontal.

The vertical cross wire having been brought into coincidence with some well-marked division of the scale, the arm M is moved from its position against one of the stops N up to the other stop and back again, thus starting the bar swinging. The two thermometers T are read, and the period is determined by means of a chronometer in the usual manner. The following is an example of such a determination of period, though in some of the observations, particularly those in which the air effect was being investigated, the vibrations were allowed to continue for over 30 minutes.

Amplitude { at commencement 4°
 { at end 1° 30'

Temperature { 14°·9 14°·9
 { 14°·7 14°·7

Times of Transit. Scale moving to right.			Difference.		Times of Transit. Scale moving to left.			Difference.							
h	m	s	h	m	s	m	s	h	m	s	h	m	s	m	s
11	46	13·6	12	6	15·1	20	1·5	11	46	34·2	12	6	35·7	20	1·5
	47	13·7		7	15·2		1·5		47	34·4		7	36·0		1·6
	48	13·8		8	15·3		1·5		48	34·5		8	36·0		1·5
	49	14·0		9	15·4		1·4		49	34·6		9	36·0		1·4
	50	14·0		10	15·6		1·6		50	34·6		10	36·1		1·5
4 0·4			Mean ...		20	1·50		4 0·4			Mean ...		20	1·50	
16 1·6								16 1·6							
12 6 15·6								12 6 36·2							

Approximate period determined with stop-watch = 3·75.
Hence in 60 secs. there are 16 vibrations, and in 4 m. 0·4 s.
there are 64. Thus in 16 m. 1·6 s. there are 256. Hence
the number of vibrations corresponding to each of the above
differences is 320.

Mean time for 320 vibrations . . . 20 m. 1·50 s.

„ „ one vibration . . . 3·7547 sec.

The period of the cradle alone has also to be determined,
and it will be found convenient to adjust the period by the
weights D to nearly one second, so that the method of co-
incidence may be conveniently employed.

The following table gives particulars of the different inertia-
bars which have been compared.

Distinguishing number.	Material, &c.
1.	Rolled brass.
2.	Cast silver.
3.	Rolled copper.
4.	Copper with 0·5 per cent. zinc. Cast.
5.	Copper with 0·5 per cent. phosphide of tin. Cast.
6.	Gun-metal. Cast.
7.	Rolled copper.
8.	Rolled copper.
9.	Rolled copper.
10.	A rolled brass bar with slightly rounded corners and gilt.

Note.—Bars 2, 4, and 5 were made from castings prepared with very
great care at the Royal Mint under the direction of Dr. Rose; and I am
much indebted to him for the trouble he has taken in the matter.

The bars were turned and then ground true, while special
care was taken to have the ends plane and perpendicular to
the axis of the cylinder. In the case of bars numbers 7, 8,
and 9 the ends were ground and polished to an optical surface,

which was tested by obtaining Newton's rings between the metal surface and a piece of plane glass. The ends were slightly rounded; but as this did not amount to more than about five half wave-lengths of sodium light, and this only near the edge, the accuracy of the measurements did not necessitate any correction being applied*.

To measure the lengths of the bars two cylindrical palpers having the same diameter as the bars were prepared, one end of each being turned to a spherical surface. A fiducial line was scratched on one cylinder. These cylinders rested in a V-groove in the iron bed of a comparator fitted with micrometer-microscopes, one palper bedding against a stop. The two palpers being placed in contact, by means of a force of 70 grams weight, the right microscope was adjusted to the fiducial mark on the left-hand palper. The bar to be measured having been introduced between the palpers, which were kept pressed together by the same force as before, the left-hand microscope was adjusted to the fiducial mark on the left-hand palper. The micrometers having been read, the bar was turned through 90° and the adjustment repeated. In this way four measurements were taken, each measurement consisting of four readings of the micrometers, the bar being turned through a right angle between each measurement. To obtain the distance between the micrometers a nickel steel (Invar) metre which had been calibrated at the International Bureau at Sèvres was substituted for the bars, and the micrometer-readings taken for two divisions 10 centimetres apart, care being taken not to displace either micrometer more than necessary from the position it occupied when setting on the bars.

In the case of the bars with optically worked ends, the length was also measured by setting the micrometers halfway between the point of a needle and its image as seen reflected in the end surface of the bar. At first this method gave values which were consistently 0.001 centimetre lower than those obtained by the contact method. This difference was found to be due to the objectives of the microscopes not being aplanatic, for in this method only half of the objective of the microscope is used. On replacing the objectives with others which were optically much better, and taking care to focus accurately, the measurements obtained by the two methods were in exact agreement.

* This question has also been tested at the National Physical Laboratory, where they measured the lengths of the generators of two of these cylinders across two diameters, and found that the lengths were constant except at the very edge, and that there the "rounding" did not amount to more than .0006 cm.

The diameters were measured at three places along each bar by means of a Brown and Sharp micrometer-screw gauge which had been tested against the standard metre, measurements being taken along different diameters at each place.

The masses were obtained by means of a Bunge balance, which reads easily to a tenth of a milligram. The method of double weighing was employed, the weights used having been calibrated and compared with a standard 100 gram weight the error of which has been determined at Sèvres. Except in the case of the silver bar no correction has been applied for buoyancy, as all the weights, except the fractions of a gram, were of brass.

In the following table are given the dimensions of the bars and the calculated values of the moment of inertia :—

No. of Bar.	Length at 15°. cm.	Diameter at 15°. cm.	Mass. grams.	Logarithm of the Moment of Inertia at 15°.
1	9·9755	·9909	65·230	2·73633
2	9·9811	·9974	81·814	2·83525
3	9·9998	·9975	69·204	2·76416
4	10·0044	·9979	69·437	2·76602
5	10·0017	·9985	69·391	2·76549
6	9·9878	·9972	68·516	2·75878
7	9·9964	·9994	68·745	2·76099
8	9·9837	·9973	68·414	2·75779
9	9·9856	·9976	68·481	2·75837

All the observations of period used in the comparisons of the moments of inertia of the bars were made at temperatures not greatly differing from 15° C. In order to be able to allow for the small departures from this temperature, a series of observations were made with the whole instrument heated in a well-stirred air-bath to a temperature of 30°. The change of period with change of temperature is very small, owing to the fact that the rigidity of quartz increases with rise of temperature, and thus produces the opposite effect to that due to the increase in the dimensions of the bars owing to expansion. In the case of the copper bars the coefficient of increase of period with temperature is 0·00004, and in the case of brass 0·000035. By means of the coefficients the observations of period recorded in the following table have been reduced to what they would have been at 15°, the actual temperatures at which they were made being included between 14° and 18°.

In the case of bar 1, since the observations with this bar

range over a very considerable time, the dates of the various measurements of period are given to indicate the constancy which can be obtained with such a quartz-fibre suspension.

No. 1.		No. 5.		No. 9.	
Date.	Period.				
13. x. 03.	3.6615	3.7726		3.7446	
14. x. 03.	11	23		47	
26. x. 03.	10	22		49	
27. x. 03.	11			48	
19. xi. 03.	09	Mean... 3.7724		Mean... 3.7448	
25. xi. 03.	12				
26. xi. 03.	13				
2. xii. 03.	14	No. 6.		No. 10.	
21. i. 04.	11	3.7467		3.6682	
5. ii. 04.	11	71		81	
5. ii. 04.	11			83	
		Mean... 3.7469		86	
Mean... 3.6612				84	
				84	
				81	
				81	
No. 2.		No. 7.			
4.0543		3.7545			
43		47			
		45		Mean... 3.6683	
Mean... 4.0543		49			
		Mean... 3.7547			
No. 3.				Cradle only.	
3.7672				1.2427	
70				9	
69				6	
		No. 8.		5	
Mean... 3.7670		3.7421		2	
		22		2	
		24		5	
		24		6	
No. 4.					
3.7751		Mean ... 3.7423		Mean... 1.2425	
48					
48					
Mean... 3.7749					

If t_1 is the period with a bar of moment of inertia K_1 in the cradle, while t_0 is the period of the cradle alone, K_0 being its moment of inertia, we have

$$t_1 = 2\pi \sqrt{\frac{K_1 + K_0}{c}},$$

$$t_0 = 2\pi \sqrt{\frac{K_0}{c}},$$

where c is a constant depending on the suspension fibre. Hence

$$\frac{K_1}{t_1^2 - t_0^2} = \frac{c}{4\pi^2}.$$

Thus if the bars are uniform in density and diameter we

have, subject to variations due to errors of experiment, that the quotient $K_1/(t_1^2 - t_0^2)$ ought to be the same for all the bars. The following table gives the value of the quotient for the different bars :—

Number of Bar.	$\frac{K_1}{t_1^2 - t_0^2}$	Difference from mean.
1	45·944	+·008
2	47	+·011
3	40	+·004
4	21	—·015
5	31	—·005
6	23	—·013
7	41	+·005
8	44	+·008
9	37	+·001
	Mean 45·936 ₄	±·0078

The mean value for $\frac{K_1}{t_1^2 - t_0^2}$ is 45·936₄. I do not, however, consider that this mean is the best value to take, as owing to the extra care taken in making the three bars numbers 7, 8, and 9, I consider it advisable to give the results with these bars treble weight. Doing this the mean becomes

$$45·938.$$

By means of this mean value we may calculate the moment of inertia of the standard bar No. 10. The value obtained in this way at 15° C. is

$$547·24.$$

Owing to the fact that the rigidity of fused silica increases as the temperature rises, the period changes very little with temperature. Thus for the cradle alone the percentage increase of the period for a fall of temperature of one degree centigrade is ·0036. With a brass bar of the dimensions used in these experiments, the percentage increase of period for a fall of temperature of one degree is ·0035. As long, therefore, as care is taken to have the temperature uniform throughout the instrument, the correction for changes of temperature can be made without difficulty.

In order to obtain some idea as to the accuracy with which the moments of inertia of bars may be compared by the method employed, we may take the differences from the respective means of the determinations of period given on page 136, taken irrespective of sign. The number obtained is 0·00014. This corresponds to a mean difference from the mean of 0·0037 per cent. This value, if we neglect the error

produced by an erroneous value obtained for the period of the cradle, which error can easily be made negligible, corresponds to a mean error of .018 per cent. in the ratio of the moments of inertia.

To test the question as to whether the vibrations were strictly isochronous, that is whether the period was independent of the amplitude, a series of measurements were made with different initial amplitudes, and the results are shown in the following table :—

Amplitude.		Period.
At commencement.	At end.	
14° 24	7° 29	3.6616
7 52	4 15	3.6613
4 55	2 25	3.6611
2 44	1 20	3.6611

It would thus appear that with an initial amplitude less than 5° the period is not, within the limits of the accuracy of the observations, affected by the variation in the amplitude. This point was also brought out because in many of the observations of period starting with an initial amplitude of between 4° and 5° one set of period observations were made, and then, without altering the amplitude of the vibrations, another set was made. The values obtained for the periods were, however, as often larger with the smaller amplitude as smaller. As a further precaution the initial amplitude was in all cases taken as about the same.

When obtaining the expression for $\frac{K_1}{t_1^2 - t_0^2}$ above it has been assumed that the torsional rigidity of the suspension fibre is the same whether the cradle is empty or loaded with one of the bars. To test whether this assumption is allowable, a set of measurements were made in which the period of the cradle alone was measured. Then its period when loaded with each of two bars separately, and finally when loaded with the two bars*. The results obtained in the case of two fibres are shown in the following table :—

* The cradle used differed slightly from that shown in fig. 1, in that a second pair of V's were provided to allow of two bars being suspended simultaneously.

	Weight of Suspended System.	Period.	Value of C.
Cradle only	27 grms.	1.2426	
Cradle+No. 1	92 "	3.6612	1813.5
Cradle+No. 4	96 "	3.7749	1813.6
Cradle+No. 1+No. 4 ...	161 "	5.1094	1813.9
Cradle only	27 "	0.8713	
Cradle+No. 10	92 "	2.5713	3691.3
Cradle+No. 7	95 "	2.6318	3691.5
Cradle+No. 7+No. 10 ...	160 "	3.5744	3692.2

Assuming in each case that the change in the rigidity of the suspending fibre is proportional to the load, we obtain the following values for the ratio of the change in c for an increase in load from 27 grams to 95 grams to the value of c ,

that is the value of $\frac{\delta c}{c}$.

From the observations with the first fibre . . . 00017

From observations with the second fibre . . . 00022

The effect of this increase in the rigidity of the suspension fibre with increase in load on the ratio deduced for the moment of inertia of the bars, will increase as the difference in the moment of inertia (and mass) of the bars increases. To obtain an idea of the magnitude of the effect we may calculate the correction in the case of the bars numbers 1 and 2, which are the lightest and heaviest bars respectively. The numbers obtained are :—

Ratio of moments of inertia obtained when c is
assumed constant 125574

Ratio of the moments of inertia obtained when
corrected for the change in c produced by
change in load 125568

It will be seen that the only effect of applying the correction is to alter the value of the ratio by one part in 20000, so that it may be neglected.

§ 2. *The Effect of the Inertia of the Air on the Period of a Vibrating Magnet.*

In order to test the influence of the air carried along by the bars when swinging on their period, a vacuum-chamber was prepared which was capable of enclosing the whole instrument. This chamber consisted of a wooden box, the sides having a thickness of two inches, which was coated outside with thin tinplate, the joints being soldered up. Two windows were fitted, one of considerable size and easily removable, through which the bars could be changed, and the

other for the observation of the mirror. An arrangement was also fitted by means of which the fibre could be removed and the top of the torsion-head turned so as to start the oscillations. A side tube was connected to a manometer and a Fleuss pump, which was driven by an electric motor and was capable of maintaining a pressure of about 5 centimetres of mercury. No attempt was made to stop whatever small leaks there were, which prevented a lower pressure being attained, as the range of pressure obtained was amply sufficient for the purpose. The thermometer, which projects into the box of the instrument, was read through the large window. A special experiment showed that the change of pressure from 76 cm. to 5 cm. did not affect the reading of this thermometer by $0^{\circ}05$ C.

The procedure adopted was to observe the period at atmospheric pressure, then exhaust and observe the period, and finally, the air having been readmitted, to again observe at atmospheric pressure. In every case at least an hour was allowed after the exhaustion or admission of the air for the temperature to become uniform.

The quartz fibre used in the comparison of the inertia-bars having been broken when fitting up the vacuum-chamber a new fibre had to be fitted. The following table gives the observed periods.

Cradle and Bar No. 1.

Period at 15°.	Pressure.	Period at 15°.	Pressure.
2·5651	75·3 cm.	2·5646	4·0 cm.
2·5650	75·3	2·5645	4·0
2·5648	76·8	2·5644	4·0
2·5647	76·8	2·5641	4·6
2·5647	72·0	2·5641	4·5
2·5647	72·0	2·5641	4·3
		2·5641	4·3
		2·5641	4·2
		2·5641	4·2
Means ... 2·5648	74·7	2·5642	4·2
<i>Cradle only.</i>			
·87049	76·0	·87001	4·7
·87046	75·5	·87003	4·7
Means ... ·87047	75·7	·87002	4·7

If t_1 and t_2 are the periods at the lower and higher pressures respectively, and K is the moment of inertia of the suspended system, while k_1 and k_2 are the moments of inertia of the air

carried along by the system at the two pressures, then

$$t_1 = 2\pi \sqrt{\frac{K + k_1}{c}} \quad \text{and} \quad t_2 = 2\pi \sqrt{\frac{K + k_2}{c}},$$

or since $t_2 - t_1$ is small compared to either t_1 or t_2 ,

$$k_2 - k_1 = \frac{2(t_2 - t_1)K}{t_1}.$$

From this expression and the numbers contained in the table above, we get the following results:—

	Difference of Pressure.	$k_2 - k_1$.	Moment of Inertia of Vibrating Body.	Increase of Moment of Inertia for 76 cm. Air-pressure.	Percentage increase of Moment of Inertia for 76 cm. Air-pressure.
Cradle	71·0 cm.	·073	70·88	·078	·11
Cradle + Bar No. 1	70·5	·288	615·68	·311	·05
Bar No. 1	...	...	544·80	·233	·04

As far as I am aware the only experiments on the effect of the inertia of the air carried by a swinging magnet on its period previously made are those of Lamont (*Pogg. Ann.* lxxi. p. 124, 1847). He found that the period of a cylindrical magnet 85 cm. long and weighing 8 grams* is given by

$$T_p = T \left(1 + \frac{p}{76} \times \cdot 00033 \right),$$

where T_p is the period in air at a pressure of p cm. of mercury, and T is the period in a vacuum.

The above corresponds to a percentage increase of moment of inertia for 76 cm. of air-pressure of 0·066, which agrees in magnitude with the numbers I have obtained.

It is thus evident that the inertia of the air appreciably increases the period. This result will not influence the results obtained in the preceding part of the paper, since the bars were all of the same size and did not differ much in moment of inertia. The magnitude of the effect, however, was such as to make it worth while to investigate the effect of the air in the case of the magnets used in the Kew pattern unifilar magnetometer. Since the moment of inertia of the magnet is determined by means of a bar such as those which are being tested, when the effect of the air is neglected the moment of inertia deduced will be too small. The moment of inertia required is that of the magnet together with any air it may

* This corresponds to a diameter of about 0·4 cm.

carry with it during its motion. Since, however, the inertia-bar itself will carry some additional air, if we assume that the inertia of the bar is that deduced from its dimensions, we shall underestimate the inertia of the magnet and the air which it carries.

To investigate this subject an exact copy of the magnet belonging to magnetometer No. 61 (Elliott Bros.) was made in brass, the moment of inertia of this dummy being almost exactly the same as that of the magnet, and this dummy magnet was swung in the instrument used for comparing the moments of inertia of the bars.

Three suspension-fibres of different diameters were used, that of intermediate thickness being chosen so that the period of the dummy was nearly the same as that of the magnet when swinging in the earth's field in England. The period of the dummy was determined both with and without the inertia-bar No. 1, and the results are collected together in the following table.

DUMMY ALONE.

	Period.	Temperature.	Pressure.	Logarithmic
		°	cms.	Decrement.
	2.5085	15.6	77.3	.0014
	2.5084	16.3	75.1	.0012
	2.5085	14.4	74.7	.0012
Means...	2.5085	15.4	75.7	.0013
	2.5074	15.4	5.8	.0006
	2.5071	16.4	5.7	.0006
	2.5072	16.5	5.5	
Means...	2.5072	16.1	5.7	.0006

DUMMY + BAR No. 1.

	4.3527	16.5	75.8	.0012
	4.3528	16.5	75.8	
	4.3528	15.8	75.9	.0011
	4.3527	15.9	75.9	
Means...	4.3527 ₅	16.2	75.8 ₅	.0012
	4.3511	15.7	6.0	
	4.3510	15.7	5.8	.0007
	4.3512	15.7	5.9	
	4.3510	15.4	6.3	.0007
	4.3510	15.4	6.0	
Means...	4.3511	15.6	6.0	.0007

DUMMY ALONE.

	9.4418	18.2	75.6	.0028
	9.4419	18.2	75.6	
	9.4419	17.2	75.4	.0029
	9.4419	17.3	75.4	
Means...	9.4419	17.7	75.5	.0028

DUMMY ALONE (*con.*).

Period.	Temperature. °	Pressure. cms.	Logarithmic Decrement.
9.4361	16.9	6.4	.0021
9.4356	16.9	6.3	
9.4358	16.9	6.2	.0020
9.4361	17.0	6.1	
Means... 9.4359	16.9	6.2	.0020

DUMMY + BAR No. 1.

16.3777	17.7	74.9	.0023
16.3779	17.7	74.9	
16.3795	16.0	75.7	
16.3795	16.0	75.7	.0023
16.3792	16.0	75.7	
16.3777	15.8	76.4	.0024
16.3777	16.0	76.4	
16.3778	16.1	76.4	.0023
Means... 16.3780	16.4	75.8	.0023
16.3722	15.8	6.8	
16.3715	15.8	6.8	.0020
16.3723	15.8	6.9	
16.3730	15.8	6.9	.0020
16.3723	15.9	7.0	
16.3722	15.8	6.9	.0020

DUMMY + BAR No. 1 (Box open at sides).

16.3814	16.8	76.7	.0026
16.3808	15.1	76.8	
16.3817	15.2	76.8	.0023
16.3817	15.3	76.5	
16.3820	15.5	76.5	.0023
Means... 16.3815 *	15.6	76.7	.0024
16.3765	15.6	8.5	.0014
16.3759	15.6	8.2	
16.3763	15.6	7.9	.0014
16.3763	15.6	7.7	
Means... 16.3762	15.6	8.1	.0014

DUMMY BAR No. 1 (in narrow box).

16.3784	15.9	76.6	.0093
16.3786	15.9	76.6	.0090
16.3778	16.1	76.6	.0090
Means... 16.3783 *	16.0	76.6	.0091

* The inertia-bar was only adjusted in a symmetrical position by eye. This accounts for the variation in period and not the change in the box, as will be shown by some experiments mentioned later.

DUMMY BAR No. 1 (in narrow box) *con.*

Period.	Temperature. °	Pressure. cms.	Logarithmic Decrement.
16·3743	15·9	7·4	·0088
16·3739	15·9	7·4	
16·3740	15·9	7·4	·0087
16·3734	15·5	8·2	·0087
16·3732	15·5	8·1	·0089
16·3734	15·5	8·1	
Means ... 16·3737	15·7	7·8	·0088

DUMMY ONLY.

14·6275	16·9	75·8	·0039
14·6278	17·3	75·8	
14·6273	17·7	75·8	·0038
14·6275	17·9	75·8	
14·6292	17·7	75·6	·0038
14·6286	17·7	75·6	
14·6292	17·8	75·6	·0038
14·6290	17·8	75·6	
Means... 14·6281	17·6	75·7	·0038
14·6215	17·3	6·6	·0028
14·6192	17·3	6·4	
14·6205	17·2	6·3	·0031
14·6195	17·2	6·2	
14·6196	17·2	6·2	·0028
14·6194	17·2	6·2	
Means... 14·6198	17·2	6·3	·0029

Increase of Moment of Inertia due to the Air.

Dummy alone.

Period.	Diff. of Pressure.	$k_2 - k_1$.	Increase of Moment of Inertia for 76 cm. Air-pressure.	Condition of Box in which the System was swung.
2·51	70·0	·281	·305	Shut.
9·44	69·3	·362	·397	Shut.
14·62	69·4	·315	·345	Shut.
			Mean... ·349	
<i>Dummy with Inertia Bar No. 1.</i>				
4·35	69·8	·637	·693	Shut.
16·38	68·9	·618	·682	Shut.
16·38	68·6	·528	·594	Open.
16·38	68·8	·511	·511	Narrowed.
			Mean... ·620	

From these results it would appear that if the correction for the air effect varies with the period, the experiments are not of sufficient accuracy to detect it, at any rate over the range employed. Neither do they indicate clearly that there is any change with the size of the box containing the vibrating system, at any rate nothing of the same order as the change in the logarithmic decrement. Hence the mean of all the observations will be taken when calculating the correction to be applied to the moment of inertia of the dummy as deduced from observations of period with and without the inertia-bar made at ordinary atmospheric pressure.

Since it is the apparent moment of inertia of the magnet swinging in air which is required when determining H , no correction is required to eliminate the effect of the air carried by the magnet itself. When, however, the inertia-bar is used with the magnet in determining the moment of inertia of the magnet, the inertia of the bar is really greater than that calculated from its dimensions, on account of the air which it carries along with it as it swings. To obtain the moment of inertia of the air carried by the bar, we have to take the difference between the corrections for the dummy together with the bar and that for the dummy alone. Hence the moment of inertia of the bar must be increased by 0.271 gm.-cm.^2 . In the case of a brass bar 10 centimetres long and 1 centimetre in diameter, which will have a moment of inertia of about 550 gm.-cm.^2 , the correction amounts to 0.049 per cent. That is, the calculated value of the moment of inertia of the magnet determined with such a bar, without taking any account of the air effect, will be 0.049 per cent. too small. If the horizontal component of the earth's field has the value 0.18 , this corresponds to an error in H given by

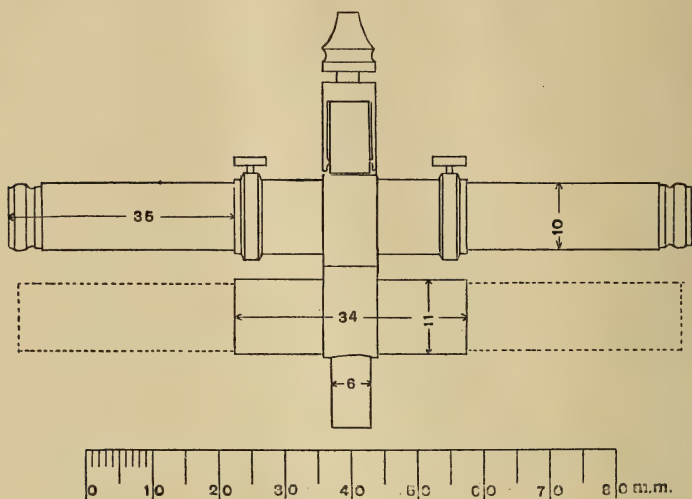
$$H = 0.000044 \text{ c.g.s. or } 4.4\gamma.$$

The above correction only applies to the form of magnet used in the particular pattern of unifilar, and of which the dimensions are given in fig. 2.

It will be noticed that when used in the instrument employed for comparing the moments of inertia of the bars, the correction to the moment of inertia of the bar 1 due to the air effect was 0.233 gm.-cm.^2 ; while when this bar is used with the dummy magnet, the correction comes out as 0.271 gm.-cm.^2 . A difference in the two cases is to be expected owing to the great difference in the shapes of the carriages which support the bar in the two cases, which will certainly

modify the amount of the additional air carried when the bar is inserted.

Fig. 2.



Some experiments were made to see whether the period of the dummy magnet is the same when swinging in the box as when swinging in the open air. These were performed by taking the period with the dummy in the closed box, and also with the sides of the box removed. The mean of four determinations of period with the box closed was 9.4472, while the mean of four determinations of period with the box open gave 9.4474. It would thus appear that at any rate with a box of the size of that shown in fig. 1 the confinement has no appreciable effect on the period of the magnet.

§ 3. Comparison of the South Kensington Bars with some belonging to the National Physical Laboratory.

The National Physical Laboratory possesses a series of bars by different makers obtained for the purposes of comparison, and with a view of determining the degree of accuracy it was reasonable to expect in bars supplied with magnetic apparatus sent for test. I was enabled, through the kindness of the Director and Dr. Chree, to swing these bars in my instrument, and thus compare their moments of inertia with those of my bars. I had at first hoped by determining the dimensions of these bars, and hence calculating their moments of inertia, to obtain additional data for determining the moment of inertia of bar No. 10. On

measuring the bars, however, I discovered that their form was so far from uniform as to make any argument based on the assumption that they were regular cylinders of relatively slight value. Thus in the case of bars 13, 14, and 15, the ends were inclined to the axis by at least 40'. In the case of bar No. 11 the diameter varied between .9937 and .9946. Bar No. 15 is the old bar belonging to the Kew Observatory standard instrument. Its diameter is very variable, being .9962 cm. at the middle and near one end, and .9934 cm. near the other. Although, on account of these irregularities, the numbers are of little value for determining the value of the moment of inertia of my standard bar, yet as the bars had been compared by the Observers at Kew with the standard magnet of their unifilar, all the bars were swung in the comparison apparatus, and the observed periods are given in the following table:—

No. 11.	No. 12.	No. 13.	No. 14.	No. 15.
3.6841	3.6777	3.6998	3.6577	3.7925
3.6841	3.6777	3.6995	3.6577	3.7927
3.6841	3.6781	3.6998		3.7929
3.6838	3.6777			
3.6837				
3.6840	3.6778	3.6997	3.6577	3.7927

From these values and the moments of inertia calculated from the mean dimensions of the bars, the following values for $\frac{K_1}{t_1^2 - t_0^2}$ were obtained.

Number of Bar...	11.	12.	13.	14.	15.
Value of $\frac{K_1}{t_1^2 - t_0^2}$	45.966	45.934	45.937	45.955	45.971
Difference from mean (excluding bar 15)...	-18	+14	+11	-7	

Although, for the reasons given, these values are of no use as far as determining the value of the constant $c/4\pi^2$ to be used in determining the moment of inertia of my standard bar is concerned, yet they are of very considerable interest in that they show the kind of variation to be expected in the bars such as they have been turned out by makers hitherto, and incidentally the magnitude of the errors likely to arise if the moment of inertia of such bars is calculated from their dimensions, and such calculated moment used to determine the moment of inertia of the magnet.

Dr. Chree having been kind enough to supply me with the mean values of the moment of inertia of their standard magnet, as deduced by comparisons with these bars, also with the dimensions which they had used in calculating the moments of inertia, I have been able, taking $45\cdot938$ as the mean value of $c/4\pi^2$, to calculate from my comparisons of the Kew bars the moment of inertia of the Kew standard magnet. The results obtained for the moment of inertia of the magnet at a temperature of 0° C. by the intermediary of the different bars are shown below, no correction having been applied to allow for the effect of the air.

Number of Inertia Bar used. }	11.	12.	13.	14.	15.
Moment of inertia of the Kew magnet at 0° C. }	268·61	268·47	268·61	268·61	268·54

In the case of the bar 15 the results are very doubtful, as the bar was damaged between the observations at Kew and those at South Kensington. Dr. Chree writes "... the bar suffered some abrasion subsequent to the swings whose results I send you, but prior to the time when it was sent to you." As a result of the abrasion the bar changed in weight from 68·770 grams to 68·756 grams. If, as is likely, the abrasion did not take place uniformly throughout the length of the bar, then it is impossible to allow for the effect it would produce on the comparison. If, for this reason, we omit the results with this bar, the mean value obtained for the moment of inertia of the Kew magnet at a temperature of 0° C. is

268·58.

Four of my own bars have been used by the Observatory for deducing the moment of inertia of the Kew magnet. The results obtained are as follows:—

Number of Inertia Bar used ...	1.	3.	8.	9.
Moment of inertia of the Kew magnet at 0° C. }	268·48	268·65	268·42	268·41
Mean.....	268·49			

Taking the two sets of results together, the mean value of the moment of inertia at 0° uncorrected for the air effect is

268·54.

Or, applying a correction of 0·049 per cent., which has to be added on account of the air carried by the inertia-bar, the final value is

268·67.

XVI. *Vibration Curves simultaneously obtained from a Monochord Sound-Box and String.* By E. H. BARTON, D.Sc., F.R.S.E., and C. A. B. GARRETT, B.Sc.*

[Plate I.]

THE vibrations of a bowed string have been treated both experimentally and mathematically by Helmholtz. It was thus shown that the displacement-time curve at any point of a well-bowed string is a zigzag line, the up and down slopes of which are both straight but in general at different angles. Hence the Fourier components of the string's motion have frequencies in the relation 1, 2, 3, 4, &c., and respective amplitudes $1, \frac{1}{2^2}, \frac{1}{3^2}, \frac{1}{4^2}, \&c.$, each to infinity.

Thus the main features of the motion of a well-bowed string are known. It is, however, a matter of common knowledge that the sound received by the ear from a string does not come chiefly from the string direct. Indeed, if a string is mounted on very rigid and massive supports scarcely any sound can be obtained from it, although the amplitude of its vibrations may be considerable. Under the usual conditions the string moves the bridges over which it is stretched, they in turn move the belly, sides, and back of the sound-box, and the air within the box pulsates in response. Probably, therefore, the chief part of the sound received by the ear comes from the belly and other parts of the sound-box, as it has a larger surface and is therefore better able to set the external air in vibratory motion.

Now the question naturally arises, are these vibrations of the same quality as those executed by the string itself? That is, may they be compounded of the same Fourier terms? Probably not; for the worth of a violin does not lie in the strings but in the sound-box.

It appears, therefore, to be a matter of some importance to attempt to trace the changes in the character of the vibrations which occur as we pass through the series string, bridges, sound-box, air within the sound-box, air outside the sound-box. As this, so far as we know, has not hitherto been done, we attacked the problem, commencing with a monochord, and hoping afterwards to pass on to a violin.

Preliminary trials were made with a Nernst lamp, optical levers, and a rotating mirror. These showed that the motions of the bridge, belly, and air (in and out of the holes of the sound-box) were all detectable. The corresponding vibration

* Communicated by the Authors.

curves were projected on to a screen and made visible to a small audience.

At this point the desirability appeared of making *simultaneous* photographs of the motion of the string together with that of one of the resonant bodies. This would show whether the variations of the latter's motion were traceable to bad bowing of the string. It would also exhibit the relations of amplitudes and phases of the two motions.

On surveying the possible ground to be covered in experiments of this kind, it was seen that there was too much for a single article of moderate dimensions. To exhaust the field the following variables occurred to us as needing successive treatment:—Pitch of string, its material and dimensions, place of bowing, place of touching (if any) to elicit an "harmonic," place of observing string, place of observing belly in length and width, motion of bridge vertically and horizontally, motion of membrane covering one of the holes of the sound-box. All these would need repeating if the string were excited by plucking, and again if it were struck. Then for the violin there would be eight such sets, one for each of the four strings if bowed and one for each if plucked. This would entail in all considerably over a hundred sets of observations.

It was accordingly decided to confine our attention at the outset to a metal string on a monochord excited by bowing, and to deal with simultaneous photographic records of the motion of string and belly. These form the subject of the present paper. It might seem more logical to observe the bridge before the belly. But the importance of obtaining, as early as possible, simultaneous records, led us to prefer the other alternative. For it seemed desirable to observe both belly and string near their middle points; because the belly might naturally be expected to have the greatest amplitude there, and the string near, but not quite at, its middle point shows all the Fourier components of its motion. And these places of observation of belly and string being near together, facilitated the focussing of the two corresponding images on the same plate. Whereas to do the same for the bridge and the middle of the string would entail much labour for which at the outset there appeared no compensating advantage.

Experimental Arrangements.

The monochord used bears no name, but is a fine well-seasoned instrument which has been at this College for twenty years. It was securely mounted horizontally on one table

with its legs over the table legs. The optical arrangements were placed on a second table at right angles to the first. The space between the bridges is 100 cm. and the "string" used throughout was a steel wire 0.92 mm. in diameter.

Helmholtz analysed the motion of a bowed string by means of the vibration microscope, thence deriving the displacement-time curve. Krigar-Menzel and Raps focussed upon the string the real image of an illuminated slit. This image was at right angles to the string, and was used to form a second real image upon the moving photographic film carried by a rotating drum. This method gave a direct photograph of the displacement-time curve, for the shadow of the string moved up and down along the bright image of the slit as the drum carried the film along horizontally.

For the string's motion we adopted a modification of the method of Krigar-Menzel and Raps; their film on a drum being replaced in our experiments by an ordinary glass negative. This was shot horizontally along rails like the smoked plate in the Electric Chronograph due to the Rev. F. J. Smith of Oxford.

But the motion of the belly needed providing for also. This was detected by a small three-legged optical lever made of aluminium. Two of its feet formed its axis of rotation and were carried by a hole and slot in a plate mounted on a massive stand. The third leg, to detect the motion of the belly, rested upon a plane, thus completing the geometrical arrangement of "hole, slot, and plane." The lever was held down by indiarubber bands. The "plane" consisted of the head of a drawing-pin, bare or covered with glass, or a glass plate cemented on to the belly direct. These separate cases will be dealt with in connexion with the experiments themselves.

The source of light was an arc lantern with a condenser five inches in diameter. This converged the beam upon a vertical slit and pin-hole in a sheet of tinfoil mounted on a glass plate. Let us first trace the course of the light passing through the slit. It is focussed by a lens upon the string near, but not quite at, its middle. It next passes on to a plane mirror behind the monochord. The beam then returns close by the side of the lantern, through a hole in the wall into another room where it reaches the photographic plate. In this part of its path a second lens serves to focus upon the plate the bright image of the slit crossed at its middle by the black shadow of the string. The second room containing the rails and photographic plate is, of course, kept pitch dark during an exposure. The carriage containing the plate is shot

by indiarubber cords, the place of exposure being that of practically zero acceleration.

Consider now the second portion of light which passes through the pin-hole. It next falls upon a lens, then upon the concave mirror of the optical lever. This sends it to the photographic plate just below the image of slit and string. The adjustment of the single lens is in this case sufficient for focussing.

Hence, if all were still, we should have upon the upper half of the plate a bright image of the vertical slit crossed by the string's shadow, while below would be the bright spot from the optical lever. When the string is excited, its shadow moves up and down the slit, and the belly moving in response the bright spot also oscillates vertically. When, however, the plate is moving horizontally, we have both oscillations drawn out into displacement-time curves. The upper one, for the string, is, in the positive print, a black line on a white ground; the lower one, for the belly, being a white one on a dark ground. Lumière X-ray plates were found very suitable and were used almost throughout. The plates travelled in the rails at about eleven feet per second, the length (four inches) passing in about $\frac{1}{33}$ of a second. Consequently the exposure of the plate to the spot (if at rest) bore the same ratio to $\frac{1}{33}$ of a second that the spot's diameter bore to the plate's length. For a rather large spot, of 1 mm. diameter, the exposure was therefore of the order $\frac{1}{3,300}$ of a second. When, however, the spot was tracing an oblique line at an angle ϕ with the horizontal, the exposure was only $(\cos \phi)$ -times the above. In some cases where the spot was rather small, 0.8 mm. diameter, and the obliquity considerable, the exposure was only $\frac{1}{13,000}$ of a second.

The magnifications of the motions upon the actual negatives (4 inches $\times$ 3) are for the string 2.9 and for the belly 760. Thus in the prints, to any scale, the motion of the belly is magnified about 260 times in comparison with that of the string. In some of the negatives, motions of the belly of the order $\frac{1}{1,000}$ mm. and accomplished in the $\frac{1}{2,000}$ second, are clearly traceable and occur regularly in each wave (see figure 8, Pl. I.).

As to sense and direction of the motion, on the prints the beginning of the time is at the left side and the ordinates are all inverted, *i. e.*, an upward motion of either shadow or spot means a downward motion of string or belly.

Tests as to Avoidance of Spurious Effects.

To test whether the floor was sufficiently rigid and to find the amplitude and period of its vibration, if any, a plate was exposed, the operator meanwhile jumping upon the floor. This gave a simple harmonic curve of very small amplitude and of about double the period of the slowest vibrations of the string. Hence any disturbance due to motion of the experimenters would, if present, cause a slight displacement of successive waves. But as this never appears in the photographs, it may be assumed absent throughout. It need scarcely be added that the experimenters, the one bowing the string and the other operating the plate, each assumed a set position which was carefully preserved till the plate had been exposed. Further, on this head, it made no essential difference to the curves obtained, whether the stand carrying the two legs of the optical lever was on the monochord table or on the lantern table.

A second possible source of disturbance was a general tremor of the tables bodily, which might be expected to give a motion of a spot of light reflected from a mirror fixed on the monochord. To test for this, the optical lever was inverted and fixed on the monochord belly so that it now ceased to be a lever. On bowing the string and shooting a plate, the spot reflected from the mirror gave a line practically straight.

Thirdly, the suspicion arose as to whether the leg of the optical lever might quit contact with the belly at the upper limit of the motion or might press in to the metal pin to various extents. To test this, the contact between the leg and the metal pin was included in an electric circuit with a cell and telephone. The telephone was taken to a distance of about 70 feet in an adjoining room, where all sound produced by vigorously bowing the string was inaudible. With a very weak constraint on the optical lever, the telephone gave scraping sounds corresponding to the making and breaking of contact. But with the constraint employed in the experiments no sound was heard from the telephone.

Apart, however, from these direct tests, we have indirect and confirmatory evidence of an equally convincing nature that the curves obtained correspond to actual motions of the belly, and are not illusory. Thus the immediate repetition of an experiment under like conditions gave like results. (Compare figures 4 and 5, also 6 and 7.) Again, the variation of pitch of the string always gave a change in the character of the curve obtained for the motion of the belly (see column 2). And this change could be obtained in its main

features after the lapse of days (see column 3). The nature of this indirect evidence will be more fully appreciated in a detailed examination of the various figures and the circumstances under which they were obtained.

Results.

The chief results are exhibited in the thirty-nine accompanying figures selected from 54 photographs, each showing the motions of both string and belly. On the margin near each figure a few words of explanation are given affording a clue to its object or significance, fuller details are given below in order. The length of the string on the monochord is 100 cm., and positions of bowing, &c. are given in terms of this coordinate.

Figures 1, 2, & 3 show early photographs in which the string was bowed at 90 cm., 93 cm., and 80 cm. respectively. Its frequency was about 120 per second. It was observed at 45.5 cm. The leg of the optical lever was at 50 cm., *i. e.*, at the middle of the belly as regards its length, but near the edge as regards width. It rested upon the bare head of a metal drawing-pin stuck in the belly. The string's motion is shown by a two-step zigzag, which is a Fourier series practically to infinity, and that is the case all through for the string when bowed near one end. The belly's motion as shown by the bright line in the lower half of figure 3 has components of frequencies 1 and 3, their amplitudes being respectively 1 and $\frac{1}{4}$.

Figures 4 & 5 show a pair of photographs taken after a short interval, but under like conditions, to test the regularity of working of the apparatus. The frequency of the string was about 120 per second. It was bowed at 90 cm. and observed at 44 cm. The leg of the optical lever was at 49.8 cm. and rested on a glass plate cemented on the belly. It is seen that the characters of the motions are identical in the two cases. In figure 4 both string and belly have small amplitudes, but in figure 5 both have large amplitudes. The components of the belly's vibration are here of relative frequencies 1, 2, and 3, the respective amplitudes being about 1, $\frac{1}{2}$, and 1.

Figures 6 & 7 show two photographs taken in quick succession under the same conditions. They afford a striking proof of the regularity of response of string, belly, and recording apparatus, when the impressed forces and conditions are the same. The belly's motion here consists almost solely of components of frequencies 1 and 3. It is also noteworthy that the phase lag of the belly's vibration relative to that of

the string is here distinctly less than half a wave. In the previous figures it was slightly more than half a wave. The frequency of the string was of the order 120 per second. It was bowed at 90 cm. and observed at 44.5 cm. The leg of the optical lever rested on a glass plate cemented on the head of a pin stuck in the belly at 50.5 cm.

Figures 8 & 9 give two photographs taken in quick succession, and quite undesignedly afford an interesting comparison between the analysis by the ear and that by the present graphic method. The circumstances are as follows:—It was desired to obtain curves for the bowing of the string at one-third of its length. But at the instant of shooting the first plate, both experimenters noticed that the bowing was bad and the tone produced of a “whistling” character. This was entered in the notebook, and a second plate exposed under similar conditions without stopping to develop the first. The second plate was shot while the tone was recognized by both observers to be of fair quality, considering the place at which the string was bowed, the best musical tone being of course impossible under those conditions. The difference thus heard and noted is clearly exhibited in the two negatives. The earlier one, figure 8, shows crinkles in the string’s curve which are almost entirely absent from the second, figure 9. The curves for the belly also show a like difference. The curve for the belly’s motion in figure 9 consists almost solely of components of frequencies 1 and 3. Now if the bowing had been exactly at one-third of the length, the third partial should have been absent. But, as is known, a slight deviation in the place of bowing would bring out the partial in question very strongly. The frequency of the string in these figures was 114 per second. It was bowed at $66\frac{2}{3}$ cm. and observed at 44.5 cm. The leg of the optical lever was on the glass-headed pin at 50.5 cm.

Figure 10 illustrates the entirely different motions produced in string and belly by plucking. The frequency of the string here was 128 per second. It was plucked by finger and thumb at 85.7 cm., *i. e.*, at one-seventh of its length, and was observed at 44.5 cm. The leg of the optical lever was on the glass-headed pin at 50.5 cm.

Figure 11 illustrates a strikingly simple motion of the belly when the string was pitched higher than before. It had in this example a frequency of 153 per second. It was bowed at 90 cm. and observed at 44.5 cm. The leg of the optical lever was resting on a glass-headed pin at 50.5 cm. The curve for the motion of the belly is easily found to consist chiefly of components of relative frequencies 1 and 2, whose

amplitudes are equal and their phase-difference about $\frac{3}{8}$. Hence the actual frequency of the higher component to which the belly responded so well was about 306 per second. This led us to believe that the sound-box had a special resonance at or near this pitch. To test this the next experiment was made.

Figure 12 shows the result of exciting the vibrations of the belly, not by the string, but by a trombone with its bell near the middle of the sound-box. The note sounded was $e'\flat$, of frequency about 320 per second. Before exposing the plate, this pitch was chosen as the most effective by watching the motion of the spot of light. It moved a little for d' (say 306 per second), but much better for $e'\flat$. The string was of course unmoved by the powerful blast which awakened such marked resonance in the belly. The belly's motion is also seen to be practically a pure sine graph.

Figure 13 shows the same vibrations elicited from the belly as in *figure 12*, but this time by means of the string. This note could not be obtained direct as the fundamental tone of the string, since the bridges would not bear the pressure due to the necessary tension. Neither did it seem advisable to produce the note as the second partial tone of the string, as this would have a node in the middle near our place of observation. The string was accordingly tuned down to a frequency of about 107 per second, and the required note (320 per second) produced as the third partial tone. The string was lightly touched at $33\frac{1}{3}$ cm., bowed at 7 cm., and observed at 44.5 cm. The leg of the optical lever was on the glass-headed pin at 50.5 cm. The photograph shows a very small motion of the string which is almost simple harmonic. The belly's motion is relatively large, and strictly simple harmonic as in the previous plate. Thus figures 11, 12, and 13 all agree in establishing a distinct resonance of the belly for a frequency of the order 320 per second.

Figures 14 to 26, occupying the second column of the Plate, illustrate the series of effects obtained by tuning the string to the successive notes of the equally-tempered chromatic scale through an octave. Throughout this set, the string was bowed at 90 cm. and observed at 44.8 cm. The leg of the optical lever was at 50 cm. and rested on glass cemented direct on the belly. The thirteen photographs were taken in one day, the negatives being stored in numbered light-tight bags for subsequent development. The frequencies ranged from 92 to 184 per second, and are shown in the space between the second and third columns of figures. These pitches were chosen so that the third partial of the lowest

note would be under 320 per second, and the second partial of the highest would exceed that frequency. It was thus hoped that near the beginning of the series the third partial would be specially exaggerated by the belly's resonance, and near the end of the series the second partial might receive a similar preference. But these anticipated effects, if realized at all, are almost entirely masked by a number of other resonances of which the belly seems capable. In fact, it appears difficult to obtain a sought resonance from an upper partial of any note, although such a resonance may be elicited accidentally.

Figures 27 to 39, forming the third and last column of the Plate, show the curves obtained by a repetition of experiments at pitches the same as those in the second column, the string being again bowed at 90 cm. and observed at 44.8 cm. But this time the leg of the optical lever was on the glass-headed pin at 50 cm. The general agreement of photographs forming a pair in any one line in the second and third columns testifies to the satisfactory working of the method. The results are, however, too complicated to be readily analysed at a glance. The last figure has for the belly's motion a curve whose components have frequencies 1 and 2 and relative amplitudes of 1 and $\frac{1}{4}$. This would give for the sound-box a resonance of the order 368 per second.

University College, Nottingham,
April 7, 1905.

XVII. *A Correction.* By HARRY C. JONES*.

IN a recent paper in this Journal† on "Recent Investigations bearing on the Theory of Electrolytic Dissociation," L. Kahlenberg attempts to overthrow this most important generalization in the field of Physical and Inorganic Chemistry. His method of reasoning on the basis of his supposed facts will be passed over without comment. It should be said, however, that in my opinion we have now a very satisfactory explanation of the fact that the gas-laws apparently do not hold for the osmotic pressures of concentrated solution,—there is combination between the solvent and the dissolved substance, so that such solutions are really much more concentrated than we should think them to be from the amount of dissolved substance contained in them.

The object of this note is to correct a statement made by L. Kahlenberg, concerning the results of some of my own

* Communicated by the Author.

† *Phil. Mag.* (6) ix. p. 214 (1905).

work. On page 215 L. Kahlenberg states that, "In 1901 I published a list of results of cryoscopic and ebulloscopic determinations made with typical aqueous solutions of electrolytes and non-electrolytes, and also a list of molecular weight determinations of the same electrolytes at 0° and at 95° . It is unnecessary to discuss again the details of these results which are rather voluminous. Suffice it to state that a comparison of the freezing-point values with the molecular conductivity at 0° , and also of the boiling-point values with the molecular conductivity at 95° , revealed the fact that there is no such connexion between freezing-points and boiling-points of solutions on the one hand, and their conductivity on the other, as is claimed by the theory of Arrhenius. In numerous cases not even a qualitative agreement exists. The facts presented in the paper cited have since been corroborated by Smits in his careful vapour-tension measurements, and by H. C. Jones and co-workers, in their molecular weight determinations on solutions."

Referring to the paper cited by L. Kahlenberg*, and turning to page 342, we find the following:—Under the heading "Behaviour of non-aqueous electrolytic solutions" the following supposed "facts are presented." "Again, many solutions have been found in which the solute according to molecular weight determinations is undissociated, and which nevertheless possess excellent power of conducting electricity." "According to Dutoit and Friderich, CdI_2 , LiCl , NaI , HgCl_2 , and NH_4CnS have normal molecular weights in acetone, and yet these solutions are conductors of electricity." If it could be shown that any substance in any solvent has a normal molecular weight, over any wide range of concentration, and at the same time gives large electrical conductivity, it would be a fact well worthy of careful consideration by all physical chemists. The essential point is that the *molecular weight determinations should be made over a considerable range of dilution*, since at any one dilution the polymerization of a part of the molecules and the dissociation of others, might just counterbalance one another, and the result would then appear to be a normal molecular weight. As the dilution is increased the number of the polymerized molecules would be diminished and the molecular weight would gradually become smaller and smaller. Recognizing the importance of this fact as cited by L. Kahlenberg, I took up this problem and published the results of my work in January of 1902†. The results obtained were as follows:—

* *Journ. Phys. Chem.* v. p. 339 (1901).

† *Amer. Chem. Journ.* xxvii. p. 16 (1902).

Cadmium iodide, ammonium sulphocyanate, and sodium iodide *did not give normal molecular weights at any of the dilutions used*. The molecular weights found for cadmium iodide and ammonium sulphocyanate were larger than the normal molecular weights for all the dilutions that were employed; while the molecular weights found for sodium iodide were smaller than the normal for all the dilutions studied.

Another important point is *that the molecular weights of all three substances changed with the dilution of the solutions, becoming less and less as the dilution increased*.

On account of the small solubility of lithium chloride in acetone, it could not be successfully studied. All three of the above substances give very considerable conductivity in acetone, showing that a large number of the molecules are broken down into ions.

Mercuric chloride in acetone gives molecular weights that are very nearly normal, and these do not change appreciably with change in the dilution of the solutions. But *mercuric chloride in acetone shows practically no conductivity*. At a dilution of 36 litres the molecular conductivity is less than unity, being only 0.734 according to the work of Laszczyński*.

I concluded my paper with the following statement†:—
“These substances instead of presenting any exception to the theory of electrolytic dissociation fall directly in line with the theory, as so many similar cases have done. Indeed, it is extremely interesting to see how many apparent exceptions to the theory of electrolytic dissociation disappear as experimental methods become more refined and experimental work more accurately carried out. The amount of evidence for the general correctness of this most fruitful generalization is at present so large that any apparent exception will be accepted only after it has been very thoroughly substantiated by repeated experiments.”

Notwithstanding these facts and the conclusions drawn from them, L. Kahlenberg states that “The facts presented in the paper cited have since been corroborated by . . . H. C. Jones and co-workers in their molecular weight determinations on solutions.”

The sole object of these lines is to correct the above statement made by L. Kahlenberg.

Johns Hopkins University,
March 1905.

* *Ztschr. Electro-Chemie*, ii. p. 56.

† *Amer. Chem. Journ.* xxvii. p. 22 (1902).

XVIII. *Note on the Voltage Ratios of an Inverted Rotary Converter.* By W. C. CLINTON, B.Sc., A.M.Inst.E.E.; Demonstrator in the Pender Electrical Laboratory, University College, London*.

IF a separately excited rotary converter be driven at a constant speed with the rings and brushes on open circuit, the ratios of the maximum alternating E.M.F. between the rings to the E.M.F. over the brushes is given by a well-known expression on the assumption of a sinusoid distribution of the air-gap magnetic flux over the surface of the armature.

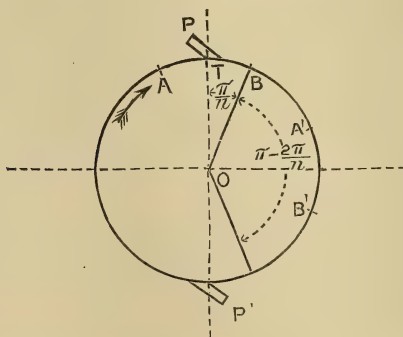
Putting E_a and E_c for the alternating and continuous E.M.F.'s respectively, and n for the number of rings, then

$$\frac{E_a}{E_c} = \sin \frac{\pi}{n}$$

where $\frac{2\pi}{n}$ is the angular width in degrees of phase between the contacts for two successive rings.

If, however, the converter be driven by current supplied to either side, the other side being on open circuit, then a correction on the above value becomes necessary owing to ohmic drop in the armature conductors.

Let direct current be supplied to the commutator, the rings being on open circuit. Let the resistance of a single path



through the armature from brush to brush be R , and let the current flowing in that path be C . Then the ohmic drop from brush to brush is CR .

* Communicated by the Physical Society : read March 24, 1905.

Let AB be points of connexion of a pair of rings to the winding.

The P.D. between the rings due to drop is zero at AB, increases uniformly till A is under the brush at P, and then remains constant until B comes under the brush P'. It decreases to zero again with A 180 phase degrees from its first position.

Let $\frac{2\pi}{n}$ = angular distance in phase degrees from A to B, and let θ = phase displacement of the centre T of the section AB from the line OP.

As θ changes from 0 to $\frac{\pi}{n}$ the P.D. between A and B due to ohmic drop increases from 0 to $\frac{2CR}{n}$ as AB is the $\frac{2}{n}$ th part of a single path between the brushes.

The value of this P.D. for an intermediate value of θ may be written as

$$\frac{2CR}{n} \cdot \frac{\theta}{\frac{\pi}{n}} = \frac{2CR\theta}{\pi}.$$

For further increase of θ up to $\pi - \frac{\pi}{n}$ this drop is constant and equal to $\frac{2CR}{n}$.

From $\pi - \frac{\pi}{n}$ to π the ohmic drop diminishes to zero again, in the same manner as it rose in the section 0 to $\frac{\pi}{n}$.

The other half of the cycle is similar with the polarity of the rings reversed.

Let V = voltage on brushes, then the total instantaneous voltage on the rings is given by:—

$$(V - CR) \sin \frac{\pi}{n} \sin \theta + \frac{2CR\theta}{\pi} \quad \text{when } \theta \text{ is between 0 and } \frac{\pi}{n} \quad . \quad . \quad (1)$$

$$(V - CR) \sin \frac{\pi}{n} \sin \theta + \frac{2CR}{n} \quad , \quad , \quad \frac{\pi}{n} \text{ and } \pi - \frac{\pi}{n} \quad . \quad (2)$$

$$(V - CR) \sin \frac{\pi}{n} \sin \theta + \frac{2CR(\pi - \theta)}{\pi} \quad , \quad , \quad \pi - \frac{\pi}{n} \text{ and } \pi \quad . \quad (3)$$

The R.M.S. value of the voltage on the rings can be
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obtained by proceeding in the usual way with the above expressions.

$$\begin{aligned}
 & \int_0^{\pi/n} \left\{ (V - CR) \sin \frac{\pi}{n} \sin \theta + \frac{2CR\theta}{\pi} \right\}^2 d\theta \\
 &= \left\{ (V - CR)^2 \sin^2 \frac{\pi}{n} \left(\frac{\pi}{2n} - \frac{\sin \frac{2\pi}{n}}{4} \right) + \frac{4CR(V - CR) \sin \frac{\pi}{n}}{\pi} \left(\sin \frac{\pi}{n} - \frac{\pi}{n} \cos \frac{\pi}{n} \right) \right. \\
 &\quad \left. + \frac{4C^2R^2\pi^2}{n^3} \right\}. \quad (4) \\
 & \int_{\pi n}^{\pi - \pi/n} \left\{ (V - CR) \sin \frac{\pi}{n} \sin \theta + \frac{2CR}{n} \right\}^2 d\theta \\
 &= (V - CR)^2 \sin^2 \frac{\pi}{n} \left\{ \frac{\pi}{2} - \frac{\pi}{n} + \frac{\sin \frac{2\pi}{n}}{2} \right\} + \frac{8CR(V - CR) \sin \frac{\pi}{n} \cos \frac{\pi}{n}}{n} \\
 &\quad + \frac{4C^2R^2}{n^2} \left(\pi - \frac{2\pi}{n} \right). \quad (5)
 \end{aligned}$$

The sum of the squares for the period 0 to π is equal to twice expression (4) + expression (5).

The value of this is

$$\begin{aligned}
 & (V - CR)^2 \sin^2 \frac{\pi}{n} \cdot \frac{\pi}{2} + \frac{4CR(V - CR)}{n} \sin \frac{\pi}{n} \left\{ 2 \sin \frac{\pi}{n} - \frac{2\pi}{n} \cos \frac{\pi}{n} + 2 \cos \frac{\pi}{n} \right\} \\
 & \quad + \frac{8C^2R^2\pi^2 + 4C^2R^2(n - 2)\pi}{n^3}.
 \end{aligned}$$

Dividing through by π , taking the square root, and dividing this again by V we get as the value of the ratio of the alternating P.D. to the direct current P.D.

$$\begin{aligned}
 \frac{1}{V} \sqrt{ \frac{(V - CR)^2 \sin^2 \frac{\pi}{n}}{2} + \frac{8CR(V - CR) \sin \frac{\pi}{n}}{n\pi} \left\{ \sin \frac{\pi}{n} + \cos \frac{\pi}{n} - \frac{\pi}{n} \cos \frac{\pi}{n} \right\} } \\
 + \frac{4C^2R^2}{n^3} (2\pi + n - 2)
 \end{aligned}$$

Let $CR = \frac{V}{m}$. Then the expression for the ratio becomes :—

$$\sqrt{ \frac{(m - 1)^2 \sin^2 \frac{\pi}{n}}{2m^2} + \frac{(m - 1) \sin \frac{\pi}{n}}{m^2 n \pi} \left\{ \sin \frac{\pi}{n} + \cos \frac{\pi}{n} - \frac{\pi}{n} \cos \frac{\pi}{n} \right\} + \frac{4}{m^2 n^3} (2\pi + n - 2) }.$$

The corrected ratios of conversion are compared with the

uncorrected values for different values of m in the following table :—

No. of Phases.	Corrected Ratios (as percentages).						Uncorrected Ratio as percentages.
	$m=10.$	$m=20.$	$m=50.$	$m=100.$	$m=200.$	$m=500.$	
Single phase ...	67·13	68·32	69·61	70·14	70·42	70·58	70·72
Three phase ...	56·70	58·71	60·18	60·69	60·95	61·10	61·24
Four phase ...	46·04	47·87	49·11	49·55	49·75	49·905	50·00
Six phase	32·46	33·82	34·73	35·03	35·19	35·29	35·35
Twelve phase...	15·60	17·50	17·97	18·13	18·21	18·26	18·30

It is of interest to construct a table showing the differences between the above ratios and the corresponding uncorrected ratio, as a percentage of the latter :—

No. of Phases.	Percentage difference from uncorrected ratio.					
	$m=10.$	$m=20.$	$m=50.$	$m=100.$	$m=200.$	$m=500.$
Single phase	5·076	3·39	1·57	·82	·42	·20
Three phase	7·413	4·13	1·73	·90	·47	·23
Four phase	7·920	4·26	1·78	·90	·50	·19
Six phase	8·176	4·33	1·75	·83	·45	·17
Twelve phase ...	14·75	4·37	1·80	·93	·49	·22

This table brings out the fact that the percentage correction due to voltage drop on the unloaded machine is practically independent of the number of phases, if the value of m is of the order usually found in good design.

XIX. *Some Properties of the α Rays from Radium.* By E. RUTHERFORD, F.R.S., Macdonald Professor of Physics, McGill University, Montreal*.

THE present paper contains the preliminary results of an investigation made for the purpose of accurately determining the velocity and ratio e/m of the α particle expelled from radium. The usual method has been adopted of measuring the deflexion of a pencil of homogeneous rays when passed through a magnetic and electric field of known

* Communicated by the Author.

intensity. The amount of the magnetic deviation has been measured, but the electric deviation has not yet been determined with the accuracy required.

During the course of these investigations some facts of interest and importance have been observed, of which a brief account will be given at this stage.

In the experiments previously made by the writer* and Des Coudres† for determining the velocity and ratio e/m of the α particle, a thick layer of radium in radioactive equilibrium was used as a source of rays. It was recognized that the α rays from radium were complex, and consisted of α particles projected at different velocities. This fact, coupled with the difficulty of obtaining a large electric deviation of the pencil of rays, prevented the determination of the velocity and ratio e/m of the particle with the accuracy required to settle definitely whether or not it is an atom of helium.

It has long been known that the α rays emitted from radium and its products differed in their power of penetrating matter. Two valuable papers have been recently published by Bragg and Kleeman‡ which have considerably extended our knowledge of the mechanism of absorption of the α rays by matter. A brief account will be given of their experimental results, as they are of importance in connexion with the work described in this paper. By using a very thin film of radium bromide, in which the absorption of the α rays was negligible, and by using narrow cones of rays, they showed that the rays from radium consisted of four distinct sets, each of which passed through a different but definite distance in air before the rays ceased to ionize the gas. The ionization produced per cm. of path of the gas by each α particle was approximately the same over the whole range, and was found to end fairly abruptly. Each of these sets of rays of definite range in air was found to correspond to the rays from one of the four α ray products present in radium in radioactive equilibrium. For example, the rays from radium C passed through about 6.7 cms. of air before ionization ceased. The corresponding distance of the rays from radium itself was about 3 cms. These results indicated that each product of radium emitted α particles of one definite velocity, but that this velocity varied considerably for the rays from the different products. As Bragg has pointed out, these results confirm in a novel and interesting way the theory of

* Phil. Mag. Feb. 1903.

† *Physik. Zeit.* iv. p. 483 (1903).

‡ Phil. Mag. Dec. 1904.

successive changes, which has been advanced from data of quite another character.

By passing the rays through an absorbing screen of thickness d , the range in air was found to be reduced by the distance ρd , when ρ was the density of the screen compared with air. This is an expression of the fact that the absorption of the rays is proportional to the density of matter traversed. In a thick layer of radioactive material of one kind, the rays emitted into the gas come from different depths, and consequently pass through a distance in air varying from zero to the maximum range, corresponding to the rays from a thin film of that material. It is thus obvious that the α particles escaping into the gas will have different velocities, that is, the rays will be complex. In radium in radioactive equilibrium, which contains four distinct products emitting α particles, the rays are still more complex in character, as each set of rays has all ranges in air between zero and its maximum.

In order to obtain results of a definite character, it is thus advisable not to use radium itself as a source of rays, but a thin film of radioactive matter of one kind, so that the rays all escape into the gas with the same velocity.

This condition is fulfilled by using as a source of rays a wire made active by exposure to the radium emanation. The active deposit on the wire after removal contains the three products radium A, B, and C. Since radium A is half transformed in three minutes, it has practically disappeared in the course of fifteen minutes. The rays then are only emitted from the one product radium C, since radium B does not emit rays at all. The deposit on the wire is so thin that no absorption takes place in the active matter itself, so that all the rays escape without change in their velocity. The α particles projected into the wire are absorbed completely by the wire itself. The activity of radium C dies down with the time, falling to half value in the first 28 minutes after removal.

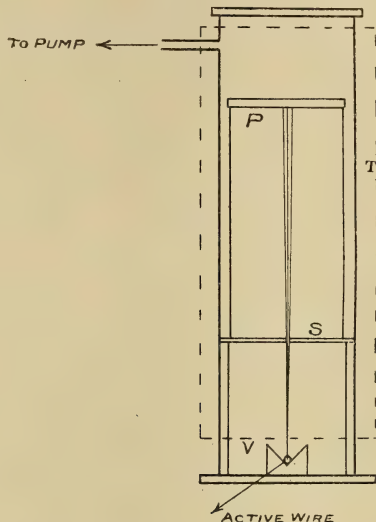
In order to produce a well-marked photographic action, using a narrow pencil of α rays at the distance of about 7 cms. required in the experiments, it was necessary to employ an intensely active wire as a source of radiation. For this purpose, a thin wire, about 1 cm. long, was charged negatively to -800 volts in the presence of the accumulated emanation from about 20 milligrams of radium bromide in solution. The wire to be made active was the only negatively charged body exposed in the presence of the emanation, and consequently the active deposit was concentrated upon

it. In this way the fine wire was made extremely active, and produced strong luminosity on a screen of zinc sulphide or willemite brought near it.

Magnetic Deflexion of the α Rays.

The method of determining the amount of deflexion of the α rays in a magnetic field is shown in fig. 1.

Fig. 1.



The active wire was placed in a slot V, at a distance of 2 cms. below a narrow slit S. The photographic plate P was supported at a definite distance above the slit. The whole was enclosed in a brass tube T, which could be rapidly exhausted to a pressure of a fraction of a millimetre by means of a Fleuss pump. The whole apparatus was placed between the rectangular pole-pieces of a large electromagnet, so that the uniform field extended from a distance of 1 cm. below the slit to the top of the tube T.

In practice, the photographic plate was placed in position, and the active wire then placed in the slot. The outer tube was then rapidly waxed down to a base-plate and exhausted. The whole operation took less than five minutes, and the magnetic field was then applied parallel to the plane of the wire and slit. The field was reversed every ten minutes for a period of about one hour. On developing the plate, two narrow bands were observed, and the distance between the centres of these bands represented twice the distance of

deflexion from the normal of the pencil of rays by the magnetic field. The strength of the magnetic field was about 9470 c.g.s. units, and this produced a well-marked separation of the bands. For example, when the photographic plate was 4 cms. from the slit, the distance between the bands on the plate was 4.7 mms.

The β and γ rays from the active wire were found to produce only a small photographic effect compared with the α rays themselves. The width of the band on the plate was found to be the same whether the magnetic field was on or not. This shows that the rays were all bent to the same extent, that is, that the α particles were all projected at the same speed. This result has been observed in a number of experiments, and confirms the conclusion arrived at by Bragg in a different way, viz., that all the α particles from one product are projected with the same velocity. We may conclude from this that each α particle, at the moment before its expulsion from the atom, is moving at a definite velocity within the atom which is the same for all the atoms of that particular substance.

The radius of curvature of the path of the rays is readily calculated.

Let $2d$ = distance between centre of bands.

d_1 = „ of plate above slit.

d_2 = „ of slit above the beginning of the magnetic field.

ρ = radius of curvature of path of the rays.

Then $2\rho d = d_1(d_1 + d_2)$.

The following table shows the results obtained for values of d_1 of 2, 3, and 4 cms.; $d_2 = 1.0$ cm. in all cases:—

Distance d_1 of plate from slit.	Value of $2d$.	Radius of curvature ρ of path of rays.
2	1.465 mms.	41.0 cms.
3	2.78 „	43.2 „
4	4.79 „	41.7 „
		Mean value...42.0 cms.

The values of ρ are thus in as good agreement as could be expected, since the magnet was excited by a 110 volt power circuit, whose E.M.F. occasionally fluctuated. These results show that over the whole range examined the path of the rays is a circle of mean radius of curvature of 42.0 cms.

The strength of the magnetic field H was constant in all the experiments, and was equal to 9470 c.g.s. units. Thus the value of $H\rho = 3.98 \times 10^5$.

The α rays from radium C are the most penetrating of all those emitted from the various products of radium, and presumably, therefore, are expelled with the greatest velocity. In a previous paper (*loc. cit.*) I deduced that the maximum value of $H\rho$ for radium rays was 390000. This result was obtained by means of the electric method, using radium in radioactive equilibrium as a source of rays. Becquerel has shown that the value of $H\rho$ is not a constant, if measured by means of the trace obtained on a photographic plate when the rays from radium passed through air at atmospheric pressure. The value of $H\rho$ varied between 291000 and 341000. This result is clear in connexion with the experiments discussed later. The pencil of rays was not homogeneous, as Becquerel thought, but included α particles projected with different velocities, which consequently were deflected to different extents. An explanation of the results of Becquerel along these lines has been given by Bragg (*loc. cit.*).

We know from the action of a magnetic field in a moving charged body that $H\rho = \frac{mV}{e}$, where m is the mass of the particle, V its velocity, and e its charge. Thus

$$\frac{mV}{e} = 3.98 \times 10^5.$$

The experiments of the deflexion of the α rays in passing through an electric field are not yet completed; they are complicated by the fact that a vacuum, sufficiently high to allow of a large difference of potential between the plates without sparking, has to be obtained in a very short time after the active wire is introduced.

We can, however, obtain an approximate estimate of the value of e/m and of V on the assumption that the heating effect of radium C is due entirely to the kinetic energy of the α particles. From the experimental results given by Rutherford and Barnes, it can be deduced that about 31 per cent. of the total heating effect of radium is due to the product radium C. Since one gram of radium emits heat at the rate of about 100 gram-calories per hour, the heating effect of radium C present in one gram of radium is 31 gram-calories per hour.

Since radium C is a direct product of radium, the number of particles expelled per second from radium C, present in

one gram of radium in radioactive equilibrium, is equal to the total number n of α particles expelled per second from one gram of radium at its minimum activity. I have recently determined this number by measuring the charge carried by the α rays emitted from a thin film of radium bromide at its minimum activity. Assuming that each α particle carries the usual ionic charge of 1.13×10^{-20} electromagnetic units, it was deduced that 6.2×10^{10} particles are expelled per second from radium itself.

Thus $n = 6.2 \times 10^{10}$.

Now $\frac{1}{2}mnV^2 =$ heating effect of α particles expelled per second from radium C.
 $= 3.6 \times 10^5$ ergs.

Substituting again the value of the ionic charge e and the value of n , $\frac{mV^2}{e} = 1.03 \times 10^{16}$.

In this result the value of e has not been assumed, since $n = \frac{i}{e}$, where i was the measured current due to the charge carried by the α rays. We have previously seen that

$$\frac{mV}{e} = 4.0 \times 10^5.$$

From these two equations, it is seen that

$$V = 2.6 \times 10^9 \text{ cms. per second}$$

and $e/m = 6.5 \times 10^3$ electromagnetic units.

These values are in surprisingly good agreement with those previously deduced by Des Coudres, and myself, from the amount of deviation of the rays in passing through a magnetic and electric field. Des Coudres found that $V = 1.6 \times 10^9$ and $e/m = 6.3 \times 10^3$, while I found that $V = 2.5 \times 10^9$, and $e/m = 6 \times 10^3$.

While the application of the heating effect of radium to determine the values of the constants of the α rays is of interest, I do not think that at present much weight can be given to the results, on account of the uncertainty attaching to the value of n , which is very difficult to measure with accuracy.

When the experiments at present in progress on the electric deflexion of the α rays from radium C are completed, it is hoped that the value of e/m will be obtained with sufficient accuracy to settle definitely the important question, whether the α particle is a projected helium atom.

Decrease of Velocity of the α particle in passing through matter.

Some experiments were made to determine the relative velocity of the α particles from radium C after passing through known thicknesses of aluminium. The apparatus shown in fig. 1 was employed, and successive layers of aluminium-foil of thickness $\cdot 00031$ cm. were placed over the active wire. The amount of deviation of the rays is inversely proportional to their velocity after their passage through the screen. The impressions obtained on the plate were all clear and distinct, and the breadth of the band was very nearly the same in all cases. This shows that the rays after traversing a metal screen were still homogeneous, although their velocity had been reduced.

A clear photographic impression was obtained with twelve layers of foil over the wire, but it was not found possible to obtain any effect through 13 layers. This result shows that the photographic action, like the ionizing action of the α rays, ceases very abruptly. The photographic effect of the α rays ceases after they have passed through a thickness of aluminium-foil greater than $\cdot 0037$ cm. and less than $\cdot 0040$ cm.

The results obtained are shown in the following table. Assuming that the value of e/m is constant, the third column gives the velocity of the α particles after passing through the aluminium. This is expressed in terms of V_0 , the velocity of the α particles when the screens are removed.

Number of layers of aluminium-foil.	Distance $2d$ between bands.	Velocity of α particle.
0	1.46 mms.	1.00 V_0
5	1.71 "	.85 "
8	1.91 "	.76 "
10	2.01 "	.73 "
12	2.29 "	.64 "

The velocity of the α particle is thus reduced only 36 per cent. after passing through 12 layers of aluminium.

Since the α particle produces approximately the same number of ions per cm. of path in air over its whole range, the simplest assumption to make is that the energy of the α particle is diminished by a constant amount in passing through each layer of aluminium-foil. After passing through 12 layers, the kinetic energy of the α particle is reduced to 41 per cent. of the maximum. Each layer of foil thus absorbs about 4.9 per cent. of the maximum energy. The observed kinetic energy of the α particle after passing through different thicknesses of aluminium-foil, and the value

calculated on the above assumptions, are shown in the following table.

Number of layers of aluminium foil.	Observed energy.	Calculated energy.
0	100	100
5	73	75
8	58	61
10	53	51
12	41	41

The experimental and calculated values agree within the limit of experimental error. We may thus conclude, as a first approximation, that the same proportion of the total energy is abstracted from the α particles in passing through successive layers of aluminium-foil.

Velocity of the α Rays from other products.

Knowing the velocity of the α particle expelled from radium C, the velocity of the α particle emitted from the other radioactive products can at once be deduced, provided the maximum range of its ionization in air is known.

This velocity can be determined from the data already given of the decrease of the velocity of the α particle in passing through screens of aluminium-foil. The ionizing action of the α rays ceases after passing through 12.5 layers of foil, which is equivalent to a thickness of 6.7 cms. of air. Each layer of foil corresponds to a depth of 0.54 cm. of air. The velocity, for example, of the rays from radium itself which have a range of 3 cms. in air, that is, which would be stopped by 5.5 thickness of foil, corresponds to the velocity of the rays from radium C which have traversed $12.5 - 5.5 = 7$ layers of foil. For convenience, the velocity of the α particle from different products which have ranges in air from 1 to 6.7 cms. are given in terms of V_0 , the velocity of the α particle from radium C.

Maximum range of the α particles in air.	Maximum velocity of α particle.
0	$\cdot 64 V_0$
1	$\cdot 71$ "
2	$\cdot 765$ "
3	$\cdot 82$ "
4	$\cdot 88$ "
5	$\cdot 92$ "
6	$\cdot 96$ "
6.7	1.00 "
7	1.01 "
8	1.06 "

The following table shows the maximum ranges of the α particle for the various products of radium, experimentally observed by Bragg. The corresponding initial velocity of projection is also shown.

Product.	Maximum range of α particles in air.	Velocity of α particles.
Radium.....	3 cms.	$\cdot 82 V_0$
Emanation	3·8 or 4·4 cms.	$\cdot 87$ or $\cdot 90 V_0$
Radium A.....	4·4 or 3·8 cms.	$\cdot 90$ or $\cdot 87 V_0$
Radium C.....	6·7 cms.	$1\cdot 00 V_0$

It is difficult to determine from the experiments whether the range 3·8 cms. belongs to the rays from the emanation or radium A. The mean velocity of the four sets of α particles is $\cdot 90 V_0$. The velocity of the α particles from the different products thus does not vary more than 10 per cent. from the mean value.

The velocity of the α particles from the products of the other radio-elements can be determined in a similar way when their range in air is known.

Range of Ionization and Photographic Action in Air.

The abrupt falling off of the photographic impression on the plate, after the rays had passed through 12 layers of aluminium-foil, suggested that it might be directly connected with the corresponding abrupt falling off of the ionization in air, so clearly brought out by Bragg. This was found to be the case. It will be shown in the next section that the amount of absorption of the α rays in each thickness of foil corresponded to 0·54 cm. of air. Twelve layers of foil thus corresponded to 6·5 cms. of air. Now Bragg has shown that the α rays from radium C ionize the air for a distance 6·7 cms., and that the ionization then falls off very rapidly. We may then conclude that the α rays cease to produce an effect on the photographic plate at the same velocity as that at which they cease to ionize the gas. This is a very important result, and, as we shall see later, suggests that the action of the photographic plate is due to the ionization of the photographic salts.

Assuming that the absorption of the α rays in aluminium

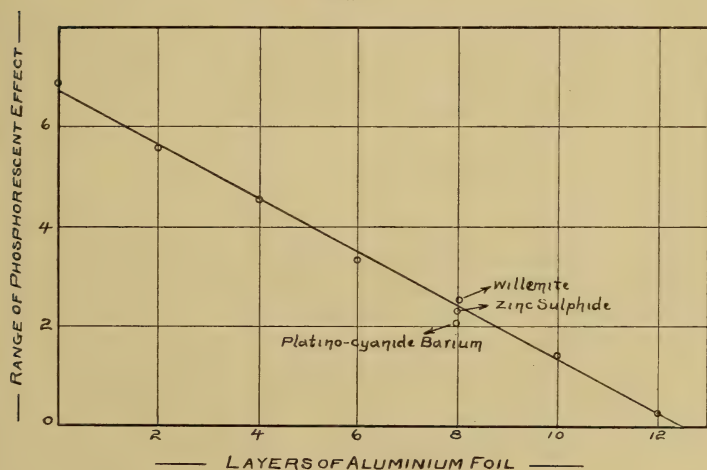
is directly proportional to its density compared with air, 12 layers of aluminium, each of thickness $\cdot 00031$ cm., correspond to 8 cms. of air. The equivalent distance in air found by experiment is 6.5 cms., so that the absorption in aluminium is somewhat less than the density law would lead us to expect.

Range of Phosphorescent Action in Air.

Some experiments were also made to see whether the action of the α rays in producing luminosity in substances like zinc sulphide, barium platinocyanide, and willemite, ceased at the same distance as the ionization property.

A very active wire was taken and placed on a movable plate, the distance of which from a fixed screen of phosphorescent substance could be quickly varied. The distance at which the phosphorescent action ceased could be fairly accurately determined. Different thicknesses of aluminium-foil were then placed over the active wire, and the corresponding distance at which the luminosity disappeared was measured. The results are shown graphically in fig. 2, where

Fig. 2



the ordinates represent the distance of the phosphorescent screen from the active wire, and the abscissæ the number of layers of aluminium-foil, each $\cdot 00031$ cm. thick.

It is seen that the curve joining the points is a straight

line. 12.5 thicknesses of foil absorbed the rays to the same extent as 6.8 cms. of air, so that each thickness of aluminium corresponded in absorbing power to .54 cm. of air. For a screen of zinc sulphide, the phosphorescent action ceased at a distance of air of 6.8 cms., showing that the photographic and phosphorescent ranges of the α rays in air were practically identical.

The experiments with barium platinocyanide and willemitite were more difficult, as the β and γ rays from the active wire produced a luminosity comparable with that produced by the α rays. Fairly concordant results, however, were obtained by introducing a thin sheet of black paper between the active wire and the screen. If the luminosity was sensibly changed, it was concluded that the rays still produced an effect, and in this way the point of cessation of phosphorescent action could be approximately determined. For example, with eight thicknesses of foil over the active wire, the additional thickness of air required to cut off the phosphorescent effect of the α rays was 2.5 cms. for willemitite, and 2.1 cms. for barium platinocyanide. The corresponding distance for zinc sulphide was 2.40 cms.; a value intermediate between the other two.

Since eight layers of foil are equivalent to 4.3 cms. of air, the range in air of the phosphorescent effects for zinc sulphide, barium platinocyanide, and willemitite correspond to 6.7, 6.8, and 6.4 cms. respectively. The differences in these values are quite likely due to experimental error.

Discussion of Results.

We have seen that the ionizing, phosphorescent, and photographic actions of the α rays emitted from radium C cease after traversing very nearly the same distance of air. This is a surprising result when it is remembered that the α particle, after passing through this thickness of air, still possesses a velocity at least 60 per cent. of its initial value. Taking the probable value of the initial velocity of the α particle from radium C as 2.5×10^9 cms. per second, the ionizing, phosphorescent, and photographic actions cease when the velocity of the α particle falls below 1.5×10^9 cms. per second, that is, a velocity of about 1/20 of that of light. The particle still possesses nearly 40 per cent. of its initial energy of projection at this stage.

These results show that the property of the α rays of

producing ionization in gases, of producing luminosity in some substances, and of affecting a photographic plate, ceases when the velocity of the α particle falls below a certain fixed value which is the same in each case. It seems reasonable, therefore, to suppose that these three properties of the α rays must be ascribed to a common cause. Now the absorption of the α rays in gases is mainly a consequence of the energy absorbed in the production of ions in the gas. When the α particles are completely absorbed in any gas, the same total amount of ionization is produced, showing that the energy required to produce an ion is the same for all gases. On the other hand, for a constant source of radiation, the ionization per unit volume of the gas is approximately proportional to its density. Since the absorption of the α rays in solid matter is approximately proportional to the density compared with air, it is probable that this absorption is also a result of the energy used up in producing ions in the solid matter traversed, and that about the same amount of energy is required to produce an ion in matter, whether solid, liquid, or gaseous.

It is probable, therefore, that the production of ions in the phosphorescent material, and in the photographic film, would cease at about the same velocity for which the α particle is unable to ionize the gas. On this view, therefore, the experimental results receive a simple explanation. The action of the α rays in producing photographic and phosphorescent action is primarily a result of ionization. This ionization may possibly give rise to secondary actions which influence the effects observed.

This point of view is of interest in connexion with the origin of the "scintillations" observed in zinc sulphide and other substances when exposed to the action of the α rays. This effect is ascribed by Becquerel to the cleavage of the crystals under the bombardment of the α particles. These results, however, show that we must look deeper for the explanation of this phenomenon. The effect is primarily due to the production of ions in the phosphorescent material, and not to direct bombardment, for we have seen that the α particle produces no scintillations when it still possesses a large amount of kinetic energy. It seems not unlikely that the scintillations produced by the α rays must be ascribed to the recombination of the ions which are produced by the α particle in the crystalline mass. It is difficult to see how this ionization could result in a cleavage of the crystals.

This close connexion of the photographic and phosphorescent

actions of the α rays with the property of producing ions raises the question whether photographic and phosphorescent effects in general may not, in the first place, be due to a production of ions in the substance.

Rayless Changes.

The results already discussed show that the α particles from the radioactive substances are projected with an average velocity not more than 30 per cent. greater than the critical velocity, below which the α particles are unable to produce any ionizing, photographic, or phosphorescent action. Such a conclusion suggests that the property of the radioactive substances of emitting α rays has been primarily detected in these substances because the α particles were projected slightly above this critical velocity. A similar disintegration of matter may be taking place in other substances at a rate much greater than in uranium, without producing much electrical effect, provided the α particles are projected below the critical velocity.

The α particle, on an average, produces about 100,000 ions in the gas before it is absorbed, so that the electrical effects observed are about 100,000 times as great as those due to the charge carried by the α particles alone, when the velocity falls below the critical value.

It is not unlikely that the numerous rayless products which have been observed may undergo disintegration of a similar character to the products which obviously emit α rays. In the rayless product, the α particle is expelled with a velocity less than 1.5×10^9 cms. per second, and so fails to produce much electrical effect.

These considerations have an important bearing on the question of whether matter in general is radioactive. The property of emitting α rays above the minimum velocity required to produce ionization in the gas may well be a property only of a special class of substances, and need not be exhibited by matter in general. At the same time, the results suggest that ordinary matter may be undergoing transformation accompanied by the expulsion of α particles at a rate much greater than uranium, without producing appreciable electrical or photographic action.

XX. *The Structure of Ions formed in Gases at High Pressures*
By O. W. RICHARDSON, M.A., D.Sc.*

M. LANGEVIN has recently pointed out† that the slowness of diffusion of ions in gases cannot be explained simply by the increase of collision-frequency caused by the attraction of neutral molecules polarized by the electric field surrounding the ions. He finds that, even when this effect is taken into account, it is necessary to assign to the positive and negative ions diameters equal respectively to thrice and twice that of an uncharged molecule, in order to obtain the values of the coefficient of diffusion actually found. The object of the present note is to indicate some further considerations which seem to shed light on the structure of ions in gases.

As is well known, by making use of the kinetic theory of gases we can obtain an expression for the drift velocity v' of an ion, whose charge is e and mass m , under an electric field of intensity X , in the form

$$v' = \frac{1}{2} \frac{e}{m} \frac{\lambda}{c} X,$$

where λ is the mean free path and c the mean speed. In deducing this formula, the statistical assumption is made that the effect of a collision is to wipe out the previous history of the molecules concerned; it is also assumed that $\frac{1}{4} \frac{Xe\lambda}{mc^2}$ is small compared with unity. This condition is satisfied if the ratio of the electric intensity to the pressure is small enough.

If we assume that the ions at pressures near atmospheric consist of two or three molecules held together by an electric charge, and substitute the probable values of e , m , and c in the above formula, we obtain values of v somewhat less than that found experimentally but of the same order of magnitude. Several reasons might be assigned for the discrepancy, but we shall not enter into them here. The fact that the correct order of magnitude is attained indicates that the theory is not altogether wide of the mark.

Admitting this, we see that if the nature of the ions remains unaltered, v'/X , the velocity under unit field, will be directly proportional to λ , that is, inversely proportional to the pressure p . This result has been established experimentally by Langevin ‡, who found that at pressures sufficiently

* Communicated by the Author.

† *Comptes Rendus*, tom. cxi. p. 35 (1905).

‡ *Thèses de l'Université de Paris*, A. 431, p. 190. Gauthier Villars, Paris, 1902.

high the value of $pv'/76$, when $X=1$ electrostatic unit of potential per centimetre, was equal to 425 for positive and 505 for negative ions. At low pressures, however, the value of the product for both kinds of ions increased, and this increase commenced at a higher pressure with negative than with positive ions. This increase can be accounted for by supposing that the ions at high pressures consist of a primitive ion combined with a number n of gas molecules. This comparatively complex ion dissociates as the pressure is reduced, and we get a dynamic equilibrium between the two kinds of ions and the neutral gas molecules. The observed conductivity will thus be due to a mixture of two kinds of ions possessing different ionic velocities. The equilibrium postulated by this hypothesis will be attained long before the ions recombine with those of opposite sign, owing to the fact that the number of ions of either sign present vanishes in comparison with the number of molecules. We shall confine our attention to the case of the negative ions, as the data for the positive are not sufficiently complete.

Suppose that at any assigned pressure p the gas contains two kinds of negative ions, viz., those which occur alone at high pressures and for which $pv/76=504$, together with those which occur alone at the very lowest pressures and for which $pv/76$ has a considerably higher value (670). Let C_1 be the concentration of, and $[pv]_1$ the value of pv for, the first kind of ion, and C_2 and $[pv]_2$ the corresponding quantities for the second kind. The case is in all respects analogous to that of chemical dissociation; and since the first kind give rise to the second as the pressure is reduced, it is evident that the second must be produced with increase of volume. We shall therefore suppose that each ion of the first kind, when it dissociates, gives rise to one ion of the second kind together with n molecules of gas. Since the concentration of the gas is proportional to its pressure, we have when equilibrium is attained,

$$C_1 = kC_2p^n,$$

where k is a constant.

Now the value of pv which is measured experimentally is the average for the whole of the ions present, and is given by

$$\begin{aligned} pv &= \frac{C_1[pv]_1 + C_2[pv]_2}{C_1 + C_2} \\ &= \frac{[pv]_2 + kp^n[pv]_1}{1 + kp^n}, \end{aligned}$$

so that

$$pv - [pv]_1 = \frac{[pv]_2 - [pv]_1}{1 + kp^n} = \frac{A}{1 + kp^n}.$$

If we put $n=2$, $A=166$, $[pv]_1=504$, and $k=.00316$ (p being expressed in cms. of mercury), we obtain numbers for pv which agree very well with those found experimentally by Langevin. The greatest difference amounts to a half per cent., and is therefore well within the experimental error. Langevin's numbers for pv are tabulated alongside those calculated from the above formula in the following table.

p (in cms.).	$pv/76$ (found).	$pv/76$ (calculated).
7.5	647	645
20	580	580
41.5	530	530.5
76	510	513
142	505	506.5

Since there are two constants beside n in the above formula, which has only to fit five observations, it might be thought that some other number would do equally well. It was found, however, that both $n=1$ and $n=3$ gave a far worse agreement with the numbers, whilst higher values of n are readily seen to be impossible.

These results lead us to interpret Langevin's numbers in the following manner:—The negative ions, which occur at high pressures and which have a velocity of 504 cms. per sec. under a field of one electrostatic unit per cm. at atmospheric pressure, dissociate as the pressure is reduced. They then give rise to two molecules of gas and a smaller negative ion which would have a velocity of 670 cms. per sec. under a field of one electrostatic unit per cm., if it could exist in air at atmospheric pressure. The actual velocity measured at any pressure is the average for the actual numbers of the two kinds which occur.

Since the velocity of the positive ions at high pressures is smaller than that of the negative, the theory requires that they should dissociate similarly at low pressures. The experiments show that there is no evidence of dissociation until pressures of about 7 cms. are reached, when the velocity of the positive ions experiences an increase.

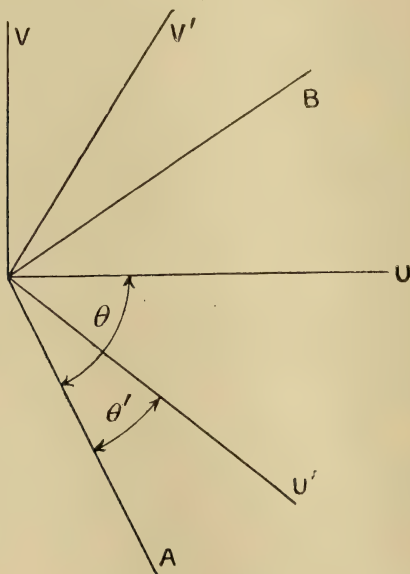
The negative ions for which $pv/76=670$ do not necessarily represent the ultimate stage in the dissociation. It is probable that these dissociate further at lower pressures into corpuscles, which would have a much higher value for pv .

XXI. *On Ellipsoidal Lenses.*By R. J. SOWTER, *B.Sc., A.R.C.Sc.**

THIS note extends the treatment of thin ellipsoidal or astigmatic lenses by the author's method †, and gives a simple solution for complex problems of the following types:—"To determine the astigmatic pencil, after refraction of an astigmatic pencil by an ellipsoidal lens." And "to find the ellipsoidal lens equivalent to two cylindrical lenses placed a definite distance apart, with their axes inclined at any angle." The method of treatment can be applied to crossed ellipsoidal lenses, in contact, or separated, and is applicable in general to astigmatic pencils.

It has been shown by Prof. S. P. Thompson ‡, and by the author independently, that for obliquely-crossed cylindrical lenses an important double-angled parallelogram of powers can be constructed. This parallelogram is associated with the resolution of a lens-power P into the two powers $P \cos^2 \theta$, $P \sin^2 \theta$, at right angles. It is here "generalized."

Fig. 1.



If an astigmatic pencil having two focal lines at distances uv from an ellipsoidal lens of focal powers A, B is refracted by the lens, which is further defined in position with respect

* Communicated by the Physical Society: read April 14, 1905.

† "On Astigmatic Lenses," *Proc. Phys. Soc.* vol. xvii.

‡ *Proc. Phys. Soc.* vol. xvi., and *Phil. Mag.* March 1900.

to the focal lines by θ , then by resolving along two directions at right angles and adopting the curvature notation, we get :—

$$-(U \cos^2 \theta + V \sin^2 \theta) + U' \cos^2 \theta' + V' \sin^2 \theta' = A \quad (i.)$$

$$-(U \sin^2 \theta + V \cos^2 \theta) + U' \sin^2 \theta' + V' \cos^2 \theta' = B \quad (ii.)$$

where

$$U = \frac{1}{u}, \quad V = \frac{1}{v},$$

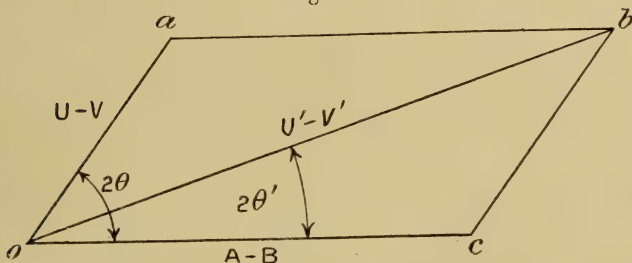
and U', V', θ' define the focal lines in the refracted pencil. Fig. 1 shows the orientation of the powers, or principal curvatures, with respect to each other. Equations (i.) and (ii.) give :—

$$-(U + V) + U' + V' = A + B \quad (I.)$$

$$-(U - V) \cos 2\theta + (U' - V') \cos 2\theta' = A - B \quad (II.)$$

This last equation shows that the power parallelogram $oabc$, shown in fig. 2, has its sides equal to $(U - V)$, $(A - B)$, its

Fig. 2.



angle 2θ , the diagonal $(U' - V')$, and the inclination of the diagonal $2\theta'$. The parallelogram affords the equation .—

$$\frac{U' - V'}{U - V} = \frac{\sin 2\theta}{\sin 2\theta'} \quad (III.)$$

Equations I., II., III., or the parallelogram $oabc$ together with the simple equation (I.), give a complete solution. The signs to be adopted are those that are usual, and such as to make

$$-(U + V) + U' + V' = A + B.$$

The usual *lengthy* solution by the use of the characteristic function resolves itself into that given by Herman* employing Malus' theorem.

If a parallel pencil fall upon the first lens of a crossed and separated cylindrical combination, the equations (i.) (ii.) are

$$-U \cos^2 \theta + U' \cos^2 \theta' + V' \sin^2 \theta' = A,$$

$$-U \sin^2 \theta + U' \sin^2 \theta' + V' \cos^2 \theta' = 0,$$

* Geom. Optics, § 174.

where U is the equatorial curvature of the cylindrical wave at the second lens, power A , θ is the angle of crossing of the lenses, and $U' V' \theta'$ define the refracted pencil, and consequently the equivalent ellipsoidal lens.

The sides of the parallelogram are A and U .

The following experiments were performed :—

Experiment I. Light from a small aperture having vertical and horizontal cross-wires was brought to a parallel beam by a convex lens, and the beam fell on a convex cylindrical lens of focal length 18·8 cms. supported with its axis vertical. At 8 cms. from this lens another convex cylindrical lens of focal length 25·3 cms. was supported with its axis inclined at 60° to the horizontal. The focal lines were at 8·1 cms., and 140 cms. from the second lens, and the near line was inclined at 21° to the axis of the inclined lens. Equation (I.) gives :—

$$U' + V' = -\cdot 132,$$

and equations II. and III., or the parallelogram, give :—

$$(U' - V') \cos 2\theta' = A + U \cos 2\theta = -\cdot 0858$$

$$(U' - V') \sin 2\theta' = U \sin 2\theta = -\cdot 0802$$

from which $U' - V' = -\cdot 118$

$$\tan 2\theta' = \cdot 935$$

that is,

$$U' = -\cdot 125 \quad (u' = -8 \text{ cms.})$$

$$V' = -\cdot 007 \quad (v' = -143 \text{ cms.})$$

$$\theta' = 21\frac{1}{2}^\circ,$$

values in agreement with those observed.

Experiment II. A beam diverging from a circular aperture having vertical and horizontal cross-wires was made parallel, and fell upon a concave cylindrical lens ($f=18\cdot 8$ cms.) supported with its axis vertical. At 10 cms. from this lens, two convex cylindrical lenses were placed in contact and crossed at right angles. Their axes were at 60° and 30° respectively to the horizontal, and their focal lengths were 18·8 cms. and 25·3 cms. The focal lines were at 23 cms. and 70 cms. from the crossed cylinders, and the inclination of one focal line to one of the cylinder axes was 41° .

Equations (i.) and (ii.) give at once :—

$$-U \cos^2 \theta + U' \cos^2 \theta' + V' \sin^2 \theta' = A$$

$$-U \sin^2 \theta + U' \sin^2 \theta' + V' \cos^2 \theta' = B,$$

and the parallelogram, the sides being $A - B$ and U , gives at once :—

$$(U' - V') \sin 2\theta' = U \sin 2\theta,$$

from which we get:—

$$U' + V' = -\cdot 058$$

$$U' - V' = \cdot 0302$$

$$\tan 2\theta' = 8\cdot 11$$

or

$$U' = -\cdot 0139 \quad (u' = -72 \text{ cms.})$$

$$V' = -\cdot 0441 \quad (v' = -23 \text{ cms.})$$

$$\theta' = 41\frac{1}{2}^\circ.$$

This method of power resolutions, as also the associated “generalized” parallelogram, can only be applied to astigmatic problems in which the wave-fronts after refraction are paraboloidal, or are ellipsoidal and can be assumed paraboloidal in the neighbourhood of some point.

Sturm appears to have been the first to show that all the normals to a paraboloid in the neighbourhood of a point converge to or diverge from two lines at right angles to one another.

XXII. *The Properties of Radium in Minute Quantities.*

To the Editors of the Philosophical Magazine.

GENTLEMEN,—

IN connexion with Mr. Eve’s paper on the above subject, the following fact may be of interest.

In December 1903 I prepared a solution by dissolving 1 milligram of a preparation of radium—stated to contain $\frac{1}{1000}$ of its weight of radium barium bromide—in a few c.c. of water. Some of the solution was poured upon a glass plate about 60 sq. cms. in area and the water evaporated. A film was left behind which gradually increased in activity, and at the present time, seventeen months after preparation, is quite as active as it was a week after preparation, and will still excite luminescence on a screen. No sort of care has been taken of the plate. A piece of glass $\cdot 25$ sq. cm. in area was cut from the plate today and its radioactive power tested and compared with that of 25 grams of fairly good pitchblende. The quantity of active salt upon this area could never have been more than $\frac{1}{200,000}$ milligram, and yet after seventeen months this small amount is at least 20 times as active as the 25 grams of pitchblende.

Yours faithfully,

W. A. DOUGLAS RUDGE.

Woodbridge School, Suffolk,
May 17, 1905.

XXIII. *Notices respecting New Books.*

The Study of Chemical Composition. An Account of its Method and Historical Development. By IDA FREUND, *Staff Lecturer and Associate of Newnham College, Cambridge.* Cambridge: At the University Press. 1904. Pp. xvi + 650.

WE can heartily commend this book, not only to students of chemistry and physics, but to all who take an interest in the development of that method of study to which the stately edifice of modern science owes its existence. In an elementary course of chemistry, there is but little time or opportunity to refer to the historical development of the subject; and even many advanced students have but a very imperfect notion of the struggles by which our present spoils of knowledge have been won. The book before us should do much to remedy this defect, and to supply that connecting link with the past without which a correct sense of historical perspective is impossible.

After a clearly-written introduction in which the author deals with the method of the inductive sciences, observation, generalization and law, hypothesis and theory, we are introduced in Chapter I. to the phlogistic theory, whose rise and decay are traced in detail. Lavoisier's important investigations in connexion with the law of the conservation of mass are next dealt with in Chapter II., and this is followed by a discussion of exact and approximate laws in Chapter III. The next few chapters deal with Berthollet and the law of mass action, Proust and the law of fixed ratios, Dalton and the law of multiple ratios, Richter and the law of equivalent ratios, combining or equivalent weights, hypotheses prior to 1800 regarding the ultimate constitution of matter, Dalton and the atomic hypothesis, Gay-Lussac and the law of the combining volumes of gases, Avogadro and the molecular hypothesis, Cannizzaro and the application of Avogadro's hypothesis to the determination of molecular and atomic weights, and Petit and Dulong and the law of atomic heat. Chapter XV., "Mitscherlich and the Connection between Crystalline Form and Chemical Composition," is deserving of special notice as furnishing a most excellent introduction to crystallography—a much neglected subject. Chapter XVI. deals with Mendeleef and the Periodic Law, Chapter XVII. with Kekulé and the doctrine of valency, and Chapter XVIII. with Berzelius and isomerism. Lastly, the important speculations regarding the probable structure of the atom which are the outcome of recent researches on the discharge of electricity through gases and on the nature and properties of radio-active bodies, form the subject-matter of the concluding chapter.

A copious index is provided, and—like all volumes forming the *Cambridge Physical Series*, to which it is the latest addition—the book is beautifully printed.

Die Grundlagen der Bewegungslehre. Von einem modernen Standpunkte aus dargestellt von Dr. G. JAUMANN, Professor der Physik an der Deutschen Technischen Hochschule in Brunn. Mit 124 Abbildungen. Leipzig: Johann Ambrosius Barth. 1904. Pp. vi+421.

IN almost every respect, this book differs markedly from the usual type of text-book on dynamics. It is an attempt to present the fundamental laws and principles of the subject in a thoroughly modern form. The author freely uses the vector calculus, following Gibbs in his method of treatment. The account which he gives of this subject is extremely condensed, and we fear that the beginner would be quite unable to follow it. To the advanced student the book may be recommended as a most interesting experiment in the modernization of one of the oldest branches of science.

It is divided into three parts, the first dealing with the motion of rigid bodies, the second with sound-waves, and the third with the motion of deformable media. The treatment is everywhere characterized by extreme compactness and generality: thus, Part I. includes brief accounts of such subjects as planetary motion; the tides, &c. There is a certain amount of incongruity in many portions of the book: any reader whose mental equipment is of a sufficiently high order to enable him to study the book with profit, will not be in any need of the surprisingly elementary information which is sandwiched in between more or less advanced analysis, involving applications of the higher parts of the vector calculus and differential equations. Such readers may surely be supposed to know the general construction of a barometer, cathetometer, hydrometer, air-pump, &c., instruments not only referred to by the author, but actually illustrated; but perhaps the author was of opinion that a number of illustrations (without adequate description) would serve to relieve the somewhat forbidding aspect of the notation of the vector calculus.

High-Temperature Measurements. By H. LE CHATELIER and O. BOUDOUARD. Authorized Translation and Additions by G. K. BURGESS, D.Sc. Second Edition, Revised and Enlarged. New York: John Wiley & Sons. London: Chapman & Hall. 1904. Pp. xv+341.

THE increasing importance of exact methods in pyrometry, not only for purposes of scientific research, but for purely industrial use, renders the appearance of this second edition of a well-known and deservedly popular standard work doubly welcome. The revision of the new edition has, at Prof. Le Chatelier's request, been carried out by Dr. Burgess, and the work has been expanded and enriched by numerous additions containing the results of important recent researches—especially in the domain of optical pyrometry. From a purely literary point of view, no doubt, improvements here and there would be desirable, as the translation is in some instances too literal; but one can afford to overlook trifling defects in view of the very substantial service which the translator has rendered in placing within easy reach of English-speaking students so admirable a treatise on a subject of growing importance.

Bulletin of the Bureau of Standards. Vol. I. No. 1. Issued Nov. 1, 1904. Washington: Government Printing Office. 1904. Pp. 124.

THE results of the researches "and other work of importance to the scientific, technical and manufacturing interests of the country," conducted by the *Bureau of Standards* at Washington, are to be in future published at intervals, as frequently as the number of papers ready for publication may require. The best idea of the contents of the opening number of the Bureau's *Bulletin* will be obtained from the titles of the papers: "Recomparison of the United States Prototype Meter," by L. A. Fischer; "A Study of the Silver Voltameter," by K. E. Guthe; "The So-called International Electrical Units," by F. A. Wolff; "The Spectra of Mixed Gases," by P. G. Nutting; "On Secondary Spectra," by P. G. Nutting; "Some New Rectifying Effects in Conducting Gases," by P. G. Nutting; "On Fibres resembling Quartz in their Elastic Properties," by K. E. Guthe; and "On the Temperature of the Arc," by C. W. Waidner and G. K. Burgess. The systematic publication of this *Bulletin*, which is well printed and illustrated, is an important step, as it will render easily accessible to the scientific public the important work done by the *Bureau*.

Avogadro and Dalton. The Standing in Chemistry of their Hypotheses. By ANDREW N. MELDRUM, D.Sc. Edinburgh: W. F. Clay. 1904. Pp. 113.

THIS is a most interesting and stimulating essay, in which the author critically examines the relations of Avogadro's hypothesis and Dalton's atomic theory to the fundamental principles of chemical science. It is no uncommon thing for highly-trained specialists to so far lose sight of the foundations on which their own subject is built as to retain only the haziest perception of the exact relation of the law and hypothesis, and of their logical connexion with the great principles which, once accepted, by dint of familiarity only too frequently come to be regarded in the light of self-evident axioms of the science. The author is to be congratulated on having, by his cautious and masterly treatment of a difficult subject, shed a flood of light on a region which up to the present has been shrouded in much obscurity.

Practical Methods of Electro-Chemistry. By F. MOLLWO PERKIN, Ph.D. London: Longmans, Green & Co. 1905. Pp. xii + 322.

For some reason or other, electro-chemistry has, until comparatively recently, attracted but little attention in England, though both on the Continent and in the United States it has been making rapid progress. The neglect of this important branch of science in England is evidenced by the poverty of English electro-chemical literature, most of the important works dealing with the subject having, until quite recently, been represented by translations from foreign languages. We are glad to note that this defect is gradually being made good. By far the best elementary introduction to the subject is Dr. Lehfeldt's little treatise, which we had occasion

to notice in these columns recently. We now have another work, dealing with a different aspect of the subject, from the pen of Dr. Mollwo Perkin, whose activity as a worker in the field of electro-chemistry is well known to all interested in the subject.

The book is divided into three Parts. Part I. is headed "General," and may be said to form a short treatise on the more purely electrical aspect of the subject. Part II. deals with electro-chemical analysis, and Part III. with preparations by electrolytic means.

We prefer to deal with the last two parts first. The preparation of these must have involved a considerable amount of trouble, for, as the author states in the preface, he decided not to include any experimental work which he had not tested personally. The directions given are very explicit, and the author's wide personal experience enables him to supply many useful hints. The bibliographical references at the ends of the various sections form a useful guide to the student who wishes to consult the original papers on the subjects dealt with. On the whole, we have nothing but praise for this part of the work, although the author is at times extremely lax in his scientific terminology. We would remind him, for instance, that C.D. (current-density) is not measured in amperes, but in amperes per unit of area. The unit of area adopted by the author is the square decimetre; so that when he speaks of a C.D. of 0.2 *ampere*, he really means 0.2 *ampere per sq. decimetre*. We are quite well aware of the fact that the author drops the "per sq. decimetre" for the sake of brevity. But there is no excuse for scientific inaccuracy in a scientific work. If the author desired to secure brevity, why did he not simply say "a C.D. of 0.2," without mention of the particular unit, it having been stated once for all that the unit is the ampere per sq. decimetre.

We wish we could be excused the task of reviewing Part I. of this book. It fairly bristles with inaccuracies which can only be described as truly appalling. On p. 9, one of the columns containing electro-chemical equivalents is headed "per coulomb in mg. *per sec.*" (the italics are ours). On p. 24, the author refers to branched circuits, and considers the case of two conductors, A and B, joined in parallel; "suppose"—we read—"the resistance of A to be 1 ohm, and that of B to be 10 ohms, *then only $\frac{1}{10}$ of the current will pass along B, the remaining $\frac{9}{10}$ passing through A.*" On p. 27 we read "electrical energy is the product of the current and potential," and immediately afterwards, "the unit of electrical energy . . . is the volt-coulomb." On p. 57, we glean the valuable piece of information that "by connecting the cells up in parallel, the internal resistance of the system is lowered, being very little *more* (!) than that of only one cell." The author is hopelessly at sea regarding the use of the terms "potential" and "E.M.F."—to him they are merely a case of "tweedledum and "tweedledee."

We feel that the best advice which we can give to the author is that in a future edition of the book he should either omit entirely Part I., which is an unpardonable blot on the work, or else have it revised by a friend competent to deal with the subject

XXIV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from Vol. ix. p. 818.]

April 5th, 1905.—J. E. Marr, Sc.D., F.R.S., President,
in the Chair.

THE following communications were read:—

1. 'On the Divisions and Correlation of the Upper Portion of the Coal-Measures, with special reference to their Development in the Midland Counties of England.' By Robert Kidston, F.R.S.L. & E., F.G.S.

2. 'On the Age and Relations of the Phosphatic Chalk of Taplow.' By Harold J. Osborne White, F.G.S., and Llewellyn Treacher, F.G.S.

The paper opens with an account of the work hitherto accomplished in the Phosphatic Chalk of Taplow, and especially of the work of Mr. Strahan. The rocks at this locality are then described in detail, and the following classification is adopted:—

	<i>Feet.</i>
E. Upper White Chalk	(visible) 16
D. Upper Brown Chalk, or rich phosphatic band	about 8
C. Middle White Chalk	„ 16
B. Lower Brown Chalk, or rich phosphatic band	„ 4
A. Lower White Chalk	(visible) 17

The Lower White Chalk includes a thin layer of tabular flint and one of elongated nodular flints, and the first signs of phosphatic material were observed a few inches below the tabular seam. Attention is drawn to the presence of phosphatic nodules and concretions at certain horizons. Fossil-lists are given from each of the above divisions; and the authors conclude that the Lower White Chalk belongs to the zone of *Micraster cor-anguinum*, and the succeeding beds to that of *Marsupites testudinarius*; while the lower phosphate-band represents the lower part of the *Urtacrinus*-band, and the upper one that of the *Marsupites*-band of that zone. In each phosphate-band the base is quite sharp, being defined by a rock-bed in the Chalk; but the upper limit is very ill-marked. The Middle White Chalk is in part divided into lenticles with slickensided surfaces. The authors find *Actinocamax verus* in B, and *A. granulatus* in D and E, but not *A. quadratus* in any bed. Phosphatization is not confined to the foraminifera, and other microscopic remains, but occurs in all shells and structures which are readily penetrable, although not so markedly in those of a more homogeneous character. *Scaloria* occurs in division E, the upper part of which may possibly just include the base of the zone of *Actinocamax quadratus*, or at any rate may not be many feet below that base. The distribution,

numerical proportion, and, to some extent also, the morphological character of the macroscopic fossils of the Phosphatic Chalk are exceptional. The majority of the French deposits of Senonian age appear to belong to a distinctly-higher horizon. The authors give evidence to show that a part, at least, of the phosphatized material has acquired its distinctive mineralogical character on the spot. So far as can be ascertained from existing data, the Phosphatic Chalk is confined to a small tract of country measuring less than $3\frac{1}{2}$ miles from north-east to south-west by less than 1 mile from north-west to south-east. It occurs as an intercalation between the normal Chalk and the Lower Eocene (Reading) Beds, and occupies a structural trough which coincides with, and is probably due to, a synclinal flexure.

April 19th.—Horace B. Woodward, Esq., F.R.S.,
Vice-President, in the Chair.

The following communications were read:—

1. 'The Blea-Wyke Beds and the Dogger in North-East Yorkshire.' By Robert Heron Rastall, B.A., F.G.S.

The author describes the type-section at Blea Wyke in detail, dividing the rocks into the following divisions, enumerated in descending order:—

	<i>Feet</i>	<i>Inches.</i>
5. Dogger	11	6
4. Yellow Beds	53	0
3. <i>Serpula</i> -Beds	10	6
2. <i>Lingula</i> -Beds	32	0
1. <i>Striatulus</i> -Shales.		

Descriptions and fossil-lists from these divisions are given, and then the succession is compared with that shown at Peak Alum-Works, Saltwick & Whitby, Sandsend & Kettleness, Boulby Cliff, Falling Foss & Littlebeck, Eskdaleside & Grosmont, Glaisdale, and along the north-western escarpment of the Cleveland Hills. A complete series from the Lias to the Lower Estuarine Series is seen only at Blea Wyke; at other sections the transition from Alum-Shale to Dogger is abrupt, and the characteristic passage-beds are absent. In all these sections there are signs of unconformity, and in most cases there is a well-marked pebble-bed of peculiar character. In many localities the upper surface of the Lias is worn into distinct hollows, in which lenticular masses of sandstone have been deposited. Probably shallow-water conditions and strong currents prevailed over a wide area. This was most likely brought about by a slow elevation which was more prominent in the northern part of the area, as though the Peak Fault were partly of pre-Oolitic date. This fault probably formed a submarine cliff, and in the still waters at its foot the Blea-Wyke passage-beds were formed. North of the fault erosion was active, planing down the Lias to a more or less level surface, with occasional deep hollows due to strong

currents. Eventually the upward movement ceased, and the hollow below the fault was filled up, so that the Dogger was laid down as a continuous bed over the whole area. The uplift to the north was probably much greater; consequently the whole of the Upper Lias was removed, and the Middle Lias reached and denuded. Pebbles derived from the Middle Liassic Ironstone occur in the basement-conglomerate of the Wainstones, according to Prof. P. F. Kendall.

2. 'Notes on the Geological Aspect of some of the North-Eastern Territories of the Congo Free State.' By Gaston Félix Joseph Preumont.

This paper is a brief sketch of the geological structure of the northern part of the Congo State, from Buta on the River Rubi and Bima on the Uelle in the west, to Lado and Dufile on the Nile.

In the whole of this region, the only post-Primary rocks met with, other than those of comparatively-modern alluvial origin, were chocolate-coloured shales (Buta Shales) and sandstone, and an oolitic limestone, on the extreme west.

From the Lipodongu Falls on the Rubi, and thence through Poko to Rungu, on the Bomokandi River, none but granitic rocks (gneisses) were observed. Along the Uelle, from Bima to Bomokandi, the same rocks were seen.

In the centre of the region mica-schists, quartzites, and similar metamorphic rocks replace the granite wholly or in part. A noticeable feature here is the presence of a range of isolated hills, composed almost completely of great beds of magnetite and hæmatite occurring in the schistose series.

In the south-eastern portion of the region visited, between the Uelle-Kimbali and Bomokandi rivers, a great plutonic massif is laid bare in the mountainous district of Arebi.

From the presence of iron-ore and schists at Mount Tena on the south-west and analogous rocks at Mount Gaïma on the north-west side of the massif, and from the fact that the dip is in each case strongly away from the intervening country, it is suggested that these rocks have been uplifted and tilted by the plutonic complex, and that the beds of Mount Tena and Mount Gaïma are identical in age.

The plutonic massif itself contains microclitic gneiss, and abundant diabasic rocks, and the same rocks in all stages of dynamo-metamorphism.

On the boundary between the Congo State and the Bahr-el-Ghazel, several hills made up of rocks of coarse gneissose and schistose character are described; some of these rocks are rich in tourmaline, kyanite, and garnet in large crystals.

From the region of the Enclave de Lado and the western side of the Nile between Lado and Dufile, mica-schists, quartzites, and microcline-gneisses are described.

The alluvium of a large part of the Uelle is covered, on the higher ground, by a deposit of limonitic conglomerate; in places

this may be due to the decomposition *in situ* of the alluvium, but in the neighbourhood of the iron-mountains a sort of passage may be seen between a conglomerate of fresh iron-ores and the more general type of limonitic conglomerate (laterite?).

May 10th.—R. S. Herries, M.A., Vice-President,
in the Chair.

The following communications were read:—

1. 'The Geology of Dunedin (New Zealand).' By Patrick Marshall, M.A., D.Sc., F.G.S.

The paper opens with an account of the physiography of Otago Peninsula and the adjacent mainland, in which the origin of the land-forms is briefly discussed. The author then passes on to a detailed account of the petrography of the district. The age of the oldest rocks seen, mica-schists, is not definitely known; they have been referred to the Archæan and to the Silurian systems. They are followed by about 1000 feet of Tertiary sandstones and limestones, which contain sufficient fossils to class them with the Oamaru system of Oligocene age (Hutton) and with the Cretaceous-Tertiary of the Geological Survey. Fine, plant-bearing shales succeed unconformably; and upon these again rests a pumice- or light scoria-bed. These rocks are in their turn covered by the igneous rocks next described.

The rocks described include an ill-exposed, gold-bearing syenite; a diorite, with an uneven and weathered surface, underneath the lavas; rhomb-porphry containing anorthoclase, and partly intrusive, partly interbedded; tinguaita, in the form of dykes; hypabyssal trachydolerite, containing nepheline and ægirine, and sometimes sodalite and analcite; a teschenite-dyke; and trachyte, notably lacking in ferromagnesian minerals, occurring in dykes and lava-flows. A large number of trachytoid phonolites occur in the form of interbedded sheets; they are divided by the author into five principal varieties, one of which bears leucite. Nephelinitoid phonolite is rare. A considerable number of rocks are classified as trachydolerites apparently occurring as lavas. The andesites are characterized by hornblende and augite, with little or no olivine; basanite is restricted in its occurrence. Dolerites of two principal types occur in dykes, one type being the commonest of all the rocks in the area. There are many lava-flows and some dykes of basalt; and one melilite-basalt is found in the extreme north of the district. A note is appended on the economic application of these rocks.

A considerable series of chemical analyses follows, showing that the silica-percentage varies from 66 in the Portobello trachyte to 44.84 in one of the dolerites. The petrographical differences are coincident with the chemical differences, and the rocks are generally similar to rocks of the same classes in other parts of the world, and more particularly to those of East Africa. A classification is attempted, according to the methods proposed by Pirsson,

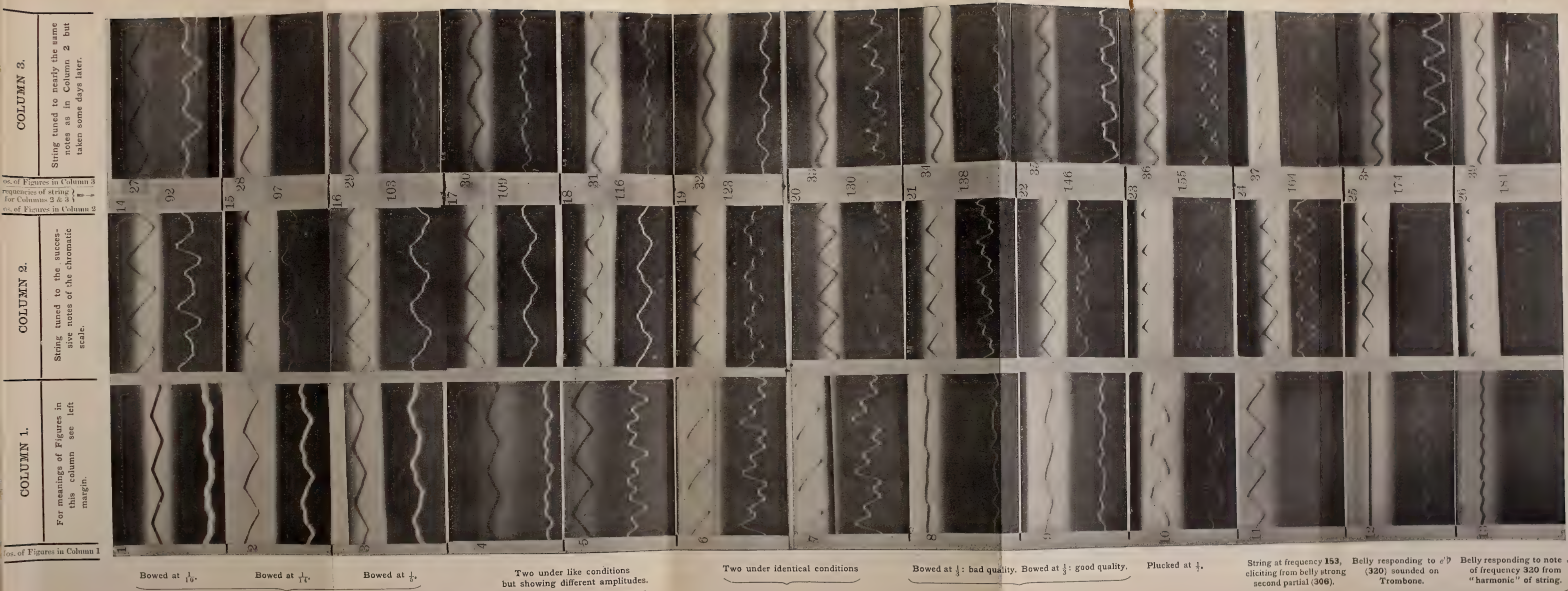
Washington, and Iddings; but the author concludes that, in his case, this classification, while grouping the rocks according to their chemical relationship, is unsatisfactory from a geological standpoint. The chemical composition of the rocks is further represented in diagrams constructed according to the method employed by Hobbs. These diagrams show clearly the chemical differences between the alkaline and the basic series, and the extent to which this hiatus is bridged over by the trachydolerite and basanite. The series, however, does not show a gradation similar to that described by Prof. Brögger in the Christiania rocks.

In the concluding section of the paper the relative ages of the volcanic rocks are worked out, so far as possible. The order does not appear to be explicable on any theory of magmatic differentiation, but it is suggested that it may be due to the mixing of magmas before and during eruption.

2. 'The Carboniferous Limestone of the Weston-super-Mare District.' By Thomas Franklin Sibby, B.Sc.

The Carboniferous Limestone of the Weston-Worle Ridge includes the greater part of the *Syringothyris*-Zone (C), extending from the 'laminosa-dolomites' upwards, and a large part of the *Seminula*-Zone (S). While the dip of the rocks of the ridge is towards the south, a reversed fault throws the *Syringothyris*-Beds on the south against the *Seminula*-Beds to the north, and the latter rocks are over-folded on the north side of the fault. To the south of the fault there is a sequence from C_1 to S_2 ; to the north, S only is represented. The lower part of C_1 , consisting of the 'laminosa-dolomites' and the 'Caninia-Oolite,' resembles the equivalent part of the Clevedon sequence, and indicates shallow-water conditions; the upper part of C, consisting of a thick mass of fossiliferous limestone, closely resembles the corresponding part of the Burrington section, and indicates the predominance of a Mendip-facies. Much dolomite occurs in the lower part of S, indicating shallow-water conditions during part of *Seminula*-time. There is a distinct development of the *semireticulatus*-subzone (S_1), which exhibits a faunal overlap between C and S; and the base of S_2 is remarkable for its rich and varied fauna.

The Woodspring Ridge shows a sequence from the Upper *Zaphrentis*-Zone (Z_2) to the top of C_1 , the whole sequence being exactly similar to that of Clevedon. In this ridge the contemporaneous igneous rocks occur in horizon γ . In the Weston-Worle Ridge they are put above the 'Caninia-Oolite,' that is, about 450 feet below the top of C. It is, therefore, evident that there were two periods of volcanic activity, one of which occurred at the close of *Zaphrentis*-time and the other early in *Syringothyris*-time. Notes are given on the faunal sequence.



Bowed at $\frac{1}{10}$.

Bowed at $\frac{1}{11}$.

Bowed at $\frac{1}{8}$.

Two under like conditions but showing different amplitudes.

Two under identical conditions

Bowed at $\frac{1}{3}$: bad quality. Bowed at $\frac{1}{3}$: good quality.

Plucked at $\frac{1}{7}$.

String at frequency 153, eliciting from belly strong second partial (306).

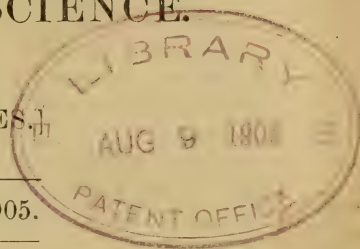
Belly responding to e'' (320) sounded on Trombone.

Belly responding to note of frequency 320 from "harmonic" of string.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[SIXTH SERIES.]

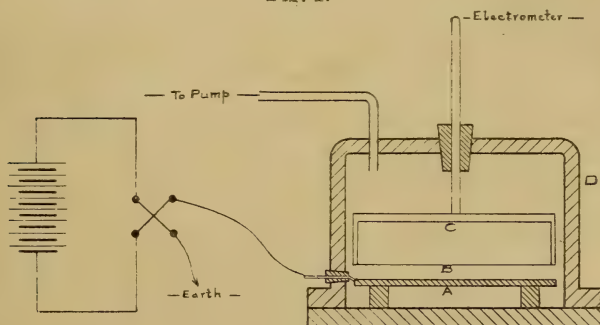
AUGUST 1905.



XXV. *Charge carried by the α and β Rays of Radium.* By
E. RUTHERFORD, F.R.S., *Macdonald Professor of Physics,*
McGill University, Montreal.*

IN the Bakerian Lecture (Phil. Trans. A 204. pp. 169-219, 1904), I gave an account of some experiments made in order to measure the charge carried by the α rays, with a view of determining the number of α particles shot out per second from a given mass of radium. The method employed is clearly shown in fig. 1. A known small quantity of pure

Fig. 1.



radium bromide was dissolved in water and the solution spread uniformly on a metal plate A, and then evaporated to dryness. The process of solution and evaporation liberates the radium emanation and about three hours afterwards

* Communicated by the Author. A brief account of the results included in this paper was published in a letter to 'Nature,' March 2, 1905.

the excited activity practically disappears, and the activity, measured by the α rays, is reduced to one quarter of its equilibrium value. Experiments were made with the radium as nearly as possible at this minimum activity in order to avoid possible complications due to the presence of β rays. The latter have almost completely disappeared about three hours after the emanation is removed. The β activity is gradually recovered, rising to half its maximum value in about four days.

A second insulated plate B was placed parallel to the lower plate and a few millimetres distant from it. The plates were insulated in a brass vessel which could be exhausted to a high vacuum by means of a mercury pump. The lower plate A was connected with one pole of a battery, the other pole of which was connected to earth. The current passing between the two plates was measured by a Dolezalek electrometer with a suitable capacity in parallel.

If the α rays carry with them a positive charge, this will be communicated to the upper plate in which they are absorbed. At ordinary pressures of the air, however, the amount of ionization produced by the α particles in their passage through the gas is so large that this charge is rapidly dissipated. It is necessary to work at a very low pressure of the gas, in order to reduce the ionization to a very small value. Under such conditions, it was to be expected that the upper plate would acquire a positive charge. A very different result, however, was observed : on diminishing the pressure of the gas, the current between the plates decreased directly as the pressure, but finally reached a limiting value, corresponding to about 1/1000 of the value at atmospheric pressure. This current was about the same whether the lower plate was charged positively or negatively, and, although an extremely high vacuum was produced, it was not found possible to further reduce its value. This minimum current was not much altered when hydrogen was present in the vessel instead of air.

No certain evidence that the α particles carried a charge could be obtained. It was suggested that the failure to detect this charge might be due to the presence of a number of slow moving electrons, liberated from the plates by the action of the rays.

The following explanation (*loc. cit.*) was given :—

“The apparent absence of charge on the α particles would be explained if an equal number of negatively charged particles, or electrons, were expelled at the same time with a slow velocity. If the electrons had about the same penetrating power as the α particles, it would be difficult to detect their

presence by the electric method, as the ionization produced by the α particles would probably mask that produced by the electrons. The electron should be readily deflected in a magnetic field, and experiments are at present in progress to examine whether the α rays show any trace of positive charge when the rays are exposed to a strong magnetic field."

If a strong magnetic field is applied parallel to the plane of the plates, any slow moving electrons, which escape from the plates, will describe curved paths and return to the plates from which they set out.

Owing to work in other directions, experiments of this character were not begun till November 1904. The apparatus of fig. 1 was placed between the pole-pieces of a large electromagnet, when a striking alteration of the values of the current between the two plates was observed. The currents in both directions were much reduced in value, and the upper plate was found to gain a positive charge, whether the lower plate was charged positively or negatively.

While these experiments were in progress, an abstract of a paper by Professor J. J. Thomson (Proc. Camb. Phil. Soc. Nov. 14, 1904) appeared in 'Nature' (Dec. 15, 1904). In this an account was given of some experiments made with a view of detecting the positive charge carried by the α rays. A plate of radio-tellurium, which emits only α rays, was used. It was found that a number of slow moving electrons were emitted from the active plate, which could readily be bent by a magnetic field. I did not receive a copy of the paper itself until my experiments were completed, and was not aware until after a publication of my results in 'Nature' (March 2, 1905) that Prof. Thomson had previously succeeded in detecting the positive charge carried by the α rays.

As the method employed by J. J. Thomson is especially well suited to show the presence of the electrons expelled with the α particles, a brief account will be given of his experiments. A plate of radio-tellurium was placed in a vacuum-tube 3 cms. away from a metal plate connected with a gold-leaf electroscope. When a very good vacuum was obtained, the electroscope was observed to leak very rapidly if positively charged, and very slowly if negatively. The positive leak was at least 100 times that of the negative. This result indicated that the plate of radio-tellurium expelled a number of slow moving electrons, which gave up their charge to the electroscope. This was confirmed by placing the apparatus in a strong magnetic field. The positive leak was almost stopped, showing that the electrons had been bent away by the action of the magnetic field. The small negative

leak without a magnetic field shows that the electrons are projected with such slight velocity that they cannot travel against the electric field. In a later experiment, where the plates were closer together, it was found that the electroscope gained a positive charge in a strong magnetic field, proving that the α rays carried a positive charge.

The experimental method used by the writer was not so much for the purpose of detecting the charge carried by the α rays, as to measure this charge and so deduce the number of α particles expelled from a known quantity of radium. The use of radio-tellurium as a source of α rays is very advantageous, as the β rays are altogether absent. It has been shown, however, that in the case of radium the disturbance due to the β rays can be avoided by using radium at its minimum activity.

An account will now be given of the details of the experiments. 0.484 milligram of radium bromide was taken, dissolved in water, and the solution evaporated uniformly on a polished aluminium plate, about 20 sq. cms. in area. This amount of radium bromide was not directly weighed, but determined by comparison of the γ ray effect in an electroscope with that produced by 23.7 milligrams of radium bromide. The latter had previously been used in experiments to determine the heating effect of radium, and gave out heat at a slightly greater rate than 100 gram-calories per gram per hour. The radium bromide was thus probably pure.

Assuming that the radium was uniformly distributed on the plate, the weight of radium bromide per sq. cm. of surface was 2.4×10^{-5} grams. This film of radium was so thin that only a very small fraction of the α particles shot out from the surface was absorbed in the radium itself. This is seen to be the case, when it is remembered that half of the α particles emitted by radium are stopped by about .0004 cm. of aluminium, that is, by a weight of aluminium per unit area of about .001 gram. Assuming that the absorption is proportional to density, only a few per cent. of the rays can have been absorbed by the radium itself.

The saturation current due to this deposit of radium at its minimum activity was measured between plates 3.5 cms. apart by means of a high-resistance galvanometer. The current observed was 8.4×10^{-8} amperes.

A second set of experiments were made with a glass instead of an aluminium plate. The deposit of radium was covered with a layer of aluminium-foil .00034 cm. thick. The saturation current, observed as in the case of the aluminium plate, corresponded to 3.3×10^{-8} amperes. From this, it was concluded that the number of α particles issuing through the

aluminium-foil corresponded to about 0.19 milligram of radium bromide. It was thought possible that some of the radium might be absorbed in the pores of the aluminium plate, and it was for this reason that a set of experiments was made with a glass plate. The agreement of the results, however, obtained with the two plates indicate that such an effect, if it exists, is small.

One of these radium-coated plates was placed in the vacuum-vessel D in the position of the plate A.

With the view of reducing as far as possible the number of electrons emitted by the impact of the α particles on the absorbing plate, the α particles were shot into a rectangular copper vessel BC, the bottom of which was covered with a layer of aluminium-foil .00034 cm. in thickness. Most of the α particles passed through this aluminium-foil, and were stopped by the metal walls of the vessel.

The brass vessel D was exhausted by means of a mercury pump to a very low pressure. In later experiments, it was found that a low vacuum could be more easily produced by Dewar's method of absorption of the residual gas by charcoal. A side tube, containing cocoanut charcoal immersed in liquid air, was connected to the vessel D. The air was first partially exhausted by means of a mercury pump, the cocoanut charcoal being heated to drive off the absorbed gases. This method was found to be very convenient to keep a good vacuum without the necessity of continuous pumping. The apparatus was placed between the pole-pieces of a large electro-magnet, so that the field was parallel to the plane of the plates AB. The following table gives the numbers obtained for the current, when the lower plate was charged positively or negatively, and when the magnetic field was on or off:—

Potential of lower plate.	Current in arbitrary units.	
	Without magnetic field.	With magnetic field.
0		+ 36
+ 2 volts.	+ 2.0	+ 46
- 2 "	- 2.5	+ 33
		} 39.5
+ 4 "	+ 2.8	+ 47
- 4 "	- 3.5	+ 35
		} 41
+ 8 "	+ 3.1	+ 56
- 8 "	- 4.0	+ 31
		} 43.5
+ 84	+ 3.5	+ 77
- 84	- 5.2	+ 24
		} 50.5

The above numbers refer to the radium-coated glass plate.

When once the magnetic field had attained a certain value a considerable increase in its strength did not affect the magnitude of the current observed. This showed that the field was strong enough to bend back all the electrons and to prevent them reaching the opposite plate. The field was not sufficiently strong to appreciably deflect the α particles themselves.

The magnitude of the charge carried by the α particles can readily be deduced. Let i_0 be the current due to the ionization of the residual gas between the plates. A very small E.M.F. is required to produce saturation at such a low pressure, and this current should be equal in magnitude but opposite in sign when the potential is reversed. Let i_1 be the charge per second communicated to the upper electrode when the lower plate is charged positively, and i_2 the value when charged negatively. Let n be the number of α particles, carrying a charge e , projected per second into the upper plate. Then

$$i_1 = i_0 + ne$$

$$i_2 = -i_0 + ne.$$

Adding

$$ne = \frac{i_1 + i_2}{2}.$$

Now in the third column of the table above, it is seen that the values of $\frac{i_1 + i_2}{2}$ are .395, .41, .435 for 2, 4, and 8 volts respectively. The numbers are thus in fairly good agreement. The charge communicated to the upper plate when the lower plate is earthed, is, as we should expect, slightly less than that calculated from the mean of the two currents, since the ionization of the residual gas tends to dissipate it as soon as the potential of the upper plates commences to rise.

It will be observed that the current between the plates before a magnetic field was applied was greater when the lower plate was negative, and for the potential difference of 4 volts was about 9 times the current due to the α particles alone.

A very different result, however, was observed when the radium-coated aluminium plate was used. In this case, the film of radium was not covered with aluminium-foil.

Voltage.	Without magnetic field.	With magnetic field.
+ 8 volts.	6.0	1.22
- 8 ,,	14.0	.03
		} .62

The negative current was about twice as great as the positive, and over twenty times as great as the current carried by the α particles alone.

A very similar result was observed when the upper electrode BC was replaced by a thick brass plate, fixed at a distance of about 2 mms. from the lower plate. We must thus conclude that the escape of electrons from the lower electrode was considerably reduced by placing a layer of aluminium-foil over the radium. The results obtained in the different experiments were fairly concordant.

The current due to the charge carried by the α particles was not small, and the electrometer-needle moved at a convenient rate when a capacity of $\cdot 0024$ microfarad was added to the system. For example, using the aluminium plate as a source of rays, the mean value of ne was found to be $9\cdot 8 \times 10^{-13}$ ampere.

Assuming that each α particle carries the same charge as an ion, viz. $3\cdot 4 \times 10^{-10}$ electrostatic units, or $1\cdot 13 \times 10^{-19}$ coulombs, the number of α particles projected per second into the upper plate was $8\cdot 7 \times 10^6$. This is the number from $\cdot 484$ milligram of radium bromide; and remembering that half of the α particles projected from the radium are absorbed in the lower plate, it can be readily deduced that the total number of α particles expelled per second from 1 gram of radium bromide at its minimum activity is $3\cdot 6 \times 10^{10}$.

The corresponding number obtained for the $\cdot 19$ milligram of radium bromide on a glass plate was $3\cdot 36 \times 10^{10}$, and for the $\cdot 484$ milligram, when the upper plate B was of brass, $3\cdot 96 \times 10^{10}$. The mean of these three determinations is $3\cdot 6 \times 10^{10}$, and this may be taken as the probable value.

Assuming the composition of the compound employed as RaBr_2 , it follows that the total number of α particles expelled per second from 1 gram of radium at its minimum activity is $6\cdot 2 \times 10^{10}$. Now the α ray activity of radium in radioactive equilibrium is four times this minimum value, and includes three products, viz. the emanation, radium A, and radium C, which emit α rays. We may thus conclude that the total number of α particles expelled per second from 1 gram of radium in radioactive equilibrium is four times the number at its minimum activity, and is equal to $2\cdot 5 \times 10^{11}$.

This number is in good agreement with the number 2×10^{11} , previously deduced from direct data, based on the heating effect of radium and the observed volume of the emanation*.

I think these experiments show conclusively that the α rays from radium do carry a positive charge. We have seen that

* Rutherford (*loc. cit.*).

the numbers obtained are in good agreement, although the magnitude of the current due to the electrons varied considerably in the different experiments. The fact that the current either in the positive or negative direction was independent over a wide range of the strength of the magnetic field, when once a certain small value of the magnetic field had been reached, shows that the strength of the field was sufficient to bend back all the slow-moving electrons emitted by the plates.

The failure of the earlier experiments to detect the charge carried by the α rays led to the suggestion that the α particle was uncharged at the moment of expulsion, but gained a positive charge in its passage through the gas. It is probable that the α particle, if initially uncharged, would lose a negative electron by collision with the gas molecules, and so retain a positive charge. This point came up in the discussion at the close of the Bakerian Lecture (*loc. cit.*) last year, and was referred to by me in the paper "Present Problems of Radio-activity," read before the International Congress of Arts and Sciences, St. Louis, 1904. The same suggestion was also made by Bragg*.

There does not, however, now seem any doubt that the α particle is charged at the moment of its expulsion. The pressure of the gas in the experiments was so low that only a small fraction of the α particles could come into collision with the gas molecules. This is brought out by the comparison of the values of the positive and negative currents in a strong magnetic field. The difference between the currents in the two directions is probably due to the small ionization produced in the residual gas by the passage of the α particles through it. In some cases the value of this ionization current was not more than one-tenth of the current due to the charge on the α particles alone. This indicates that only a small fraction of the α particles come into collision with gas molecules. On the other hand, the magnitude of the charge carried by the α rays was independent of the state of the vacuum over a considerable range, showing that the residual gas had no effect in altering the number of charged α particles absorbed in the upper plate.

We may thus conclude that the α particles are positively charged at the moment of their release from the radium plate, and, remembering that the film of radium used in the experiments was very thin, there is no obvious reason for supposing that they are not charged at the moment of their expulsion from the radium atoms.

* Phil. Mag. Dec. 1904.

Origin of the Slow-Speed Electrons.

We have seen that J. J. Thomson has shown that a number of slow-speed electrons are emitted with the α particles from a plate of radio-tellurium. The experiments described above show that this is equally the case with radium. The magnitude of the current due to these electrons is sufficient to completely mask the effect due to the charge carried by the α particles, and in some cases is twenty times as great. The question now arises, whether these electrons are projected from the radioactive matter itself, or are emitted from the plates on which the α particles impinge. The fact that the current without a magnetic field is greater when the lower plate is negative than when it is positive shows that a greater number of electrons are emitted from the lower plate. If the electrons were only emitted from the lower plate, the positive current should greatly exceed the negative. The results indicate that these electrons arise from both the upper and lower plate, but to a greater extent from the latter. This is borne out by the experiments when the radium film deposited on the glass plate is covered with thin aluminium-foil. The negative current is still the greater, but the difference is not nearly so marked. Since the electrons are probably not projected with sufficient velocity to pass through even one thickness of aluminium-foil, it seems probable that the electrons liberated from the radium itself and the glass plate would be absorbed in the aluminium-foil. The experiments as a whole indicate that the electrons escape both from the radium plate and from the surface in which they are absorbed. It seems probable that these electrons constitute a type of secondary radiation which results from the impact of the α particles on matter. Since half the α particles emitted from the radium are projected into the lower plate and the other half are projected upwards, it is to be expected that more would escape from the lower than from the upper plate. It is difficult to settle whether the radium itself emits any of these slow-speed electrons at the moment of expulsion of the α particle. There is no reason, however, to doubt that such electrons would be liberated by the bombardment of the radium by the α particles projected from its own mass.

The existence of this type of secondary radiation, set up when the α particles pass through matter, readily explains some results obtained by Mme. Curie in her experiments on the absorption of the α rays from polonium. The ionization current between two parallel plates was compared when two screens of different materials, placed over the

polonium, were interchanged. For example, with screens of aluminium and cardboard, the current was greater when the aluminium was uppermost. Similar results were obtained with other materials. These results indicate that different amounts of secondary radiations—probably in the form of the slow-speed electrons in question—were produced at the surface of the matter through which the rays passed.

Charge carried by the β Rays.

It is of importance to determine the total number of β particles emitted from one gram of radium in radioactive equilibrium, as, theoretically, it is to be expected that this number should bear a definite relation to the total number of α particles expelled. Now radium in radioactive equilibrium contains four substances, viz., radium itself, the emanation, radium A, and radium C, which emit α particles. On the other hand, β particles are only expelled from one product, radium C. These substances are successive products of radium, and, when equilibrium is reached, the same number of atoms of each break up per second. If the disintegration of the atom of each product is accompanied by the expulsion of one α particle and the case of radium C also of one β particle, the number of α particles emitted from radium in radioactive equilibrium will be four times the number of β particles. The number of β particles from radium in equilibrium will thus be equal to the number of α particles expelled at its minimum activity, when the emanation and its further products are absent.

Some experiments have been made by Wien* to determine the number of β particles expelled from a known quantity of radium. About 4 milligrams of radium bromide were enclosed in a platinum cylinder, which was insulated in a larger tube, and the air exhausted. The platinum cylinder was found to gain a positive charge, since the β particles, some of which were projected through the cylinder, carried with them a negative charge. The rate of escape of electricity from the platinum cylinder corresponded to 2.91×10^{-12} ampere. If each β particle carries the observed ionic charge of 1.13×10^{-19} , this corresponds to an escape of 2.66×10^7 β particles per second. From one gram of radium, the corresponding number would be 1.14×10^{10} .

It is known from experiments upon the absorption of the β rays from radium that some of the β particles are easily absorbed in passing through a small thickness of matter. The above estimate is for this reason far too small.

* Wien, *Physik. Zeit.* iv. p. 624 (1903).

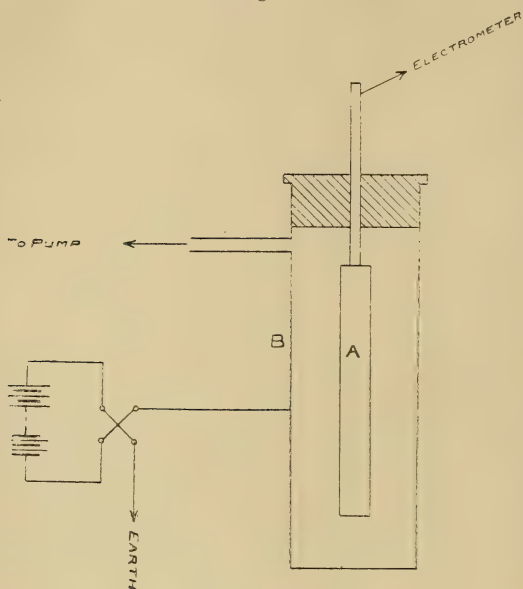
In order to eliminate as far as possible the error due to absorption of the β rays in the radium itself and the envelope containing it, I employed a different method. Instead of using the radium compound itself, a body made active by exposure in the presence of the radium emanation was used as a source of β rays. A lead cylinder 4 cms. long and 4 mms. in diameter was made the negative electrode in a vessel containing a large quantity of radium emanation. After about three hours exposure, the excited activity reaches a maximum. The lead rod was then removed, and the intensity of the γ rays from it was compared directly by means of an electroscope with that due to a known weight of pure radium bromide in radioactive equilibrium. The γ rays, rather than the β rays, were chosen as a means of comparison, as the absorption of the γ rays in the lead rod or the radium envelope is very small. Suppose, for example, that the γ ray effect from the active deposit in the lead rod was equivalent to m milligrams of radium bromide. Now the β and γ rays from the radium or the active deposit on the rod arise only from the one product radium C. Since the β and γ rays always occur together and in the same proportion, the total number of β particles emitted by the lead rod is equivalent to the number emitted by m milligrams of radium bromide. Since the active deposit on the rod is extremely thin, half of the β rays projected from it escape without absorption. The stoppage of the β particles by the active matter itself, such as would occur if radium bromide were directly used, is thus avoided.

Immediately after testing, the lead cylinder was wrapped with a thickness of aluminium-foil just sufficient to completely absorb the α rays. It was found experimentally that the α rays were completely stopped by 13 layers of foil, each of thickness .00031 cms. 17 layers of foil, that is a thickness of aluminium of .0053 cm. were added in all. This ensured that the absorption of the β particles in the aluminium screen was a minimum, consistent with complete absorption of the α rays. The lead rod was made the central electrode A in the apparatus of fig. 2. The outer cylinder B was connected with one pole of a battery, the other pole of which was earthed, and the central electrode with a Dolezalek electrometer, using a suitable capacity in parallel. The air was exhausted as rapidly as possible, and measurements were begun usually about 20 minutes after the removal of the active rod from the emanation.

The potential of the external cylinder was alternately reversed, and the currents measured. As we have seen in the

experiments on the charge carried by the α rays, half the algebraic sum of the currents observed with reversal of the

Fig. 2.



potential represents the value ne , where n is the number of β particles escaping from the central electrode per second, and e the charge on each particle.

The value of ne was found to decrease with the time at the same rate as the rod lost its β activity, measured in the usual way by an electroscope. Knowing the curve representing the variation of the β ray activity with time, the value of ne at any time can be expressed in terms of the value of ne at the moment of removal of the rod from the emanation.

The following numbers illustrate the experimental method. At the moment of removal, the γ ray activity of the rod corresponded to 4.20 milligrams of radium bromide. The value of ne was determined after the rod had been removed 33 minutes. From comparison with the known curve, the β ray activity had during that time fallen to 74 per cent. of the initial value. The value of ne , reduced to the initial value, was found to be 1.05×10^{-11} ampere. Taking e as 1.13×10^{-19} coulomb, the value of n is thus 9.3×10^7 .

This is the number corresponding to 4.2 milligrams of radium bromide. Remembering that half of the β particles are projected into the lead which absorbs most of them, it can

readily be calculated that one gram of radium in radioactive equilibrium emits 7.6×10^{10} β particles per second.

A second series of experiments made under different conditions gave a value of 7.0×10^{10} .

Taking the mean of these two values, we may thus conclude that the *total number of β particles expelled from one gram of radium per second is 7.3×10^{10} .*

The total number of α particles expelled per second from one gram of radium at its minimum activity has been shown to be 6.2×10^{10} . The approximate agreement between these numbers is a strong indication of the correctness of the theoretical views, previously discussed. It is to be expected that the number of β particles, deduced in this way, should be somewhat greater than the number of α particles, for the β particles give rise to a secondary radiation, consisting also of negatively charged particles moving at a high speed. These secondary β particles, arising from the impact of the β particles on the lead, will pass through the aluminium screen, and add their effect to those directly shot out. For this reason, probably, the experimental number is somewhat too large. The results, however, indicate that four α particles are expelled from radium in radioactive equilibrium for each β particle, and thus confirm the theory of successive changes.

Deductions from the Results.

The determination of the total number of α particles expelled per second from one gram of radium is of great value, for by its means we are enabled to directly deduce the magnitude of other important physical constants in radioactivity. The methods of calculation of some of these quantities will now be briefly given.

Life of Radium.—We have seen that one gram of radium at its minimum activity expels 6.2×10^{10} α particles per second. It seems probable that only one α particle is expelled during the disintegration of the radium atom, so that 6.2×10^{10} atoms of radium break up per second per gram. The number per gram per year is 1.95×10^{16} . It has been experimentally deduced that one cubic centimetre of hydrogen at standard pressure and temperature contains 3.6×10^{19} molecules. Taking the atomic weight of radium as 225, it follows that one gram of radium contains 3.6×10^{21} atoms of radium. The fraction λ of the radium atoms which break up per year is thus

$$\frac{1.95 \times 10^{16}}{3.6 \times 10^{21}} \quad \text{or} \quad 5.4 \times 10^{-4}.$$

Thus in a gram of radium about half a milligram disintegrates per year. Now it is probable that, as in every other radioactive product, the number of atoms of radium which break up is always proportional to the number present. Thus if n is the number present after an interval t , and n_0 the initial number, then $\frac{n}{n_0} = e^{-\lambda t}$. The value of λ is $5.4 \times 10 \text{ (year)}^{-1}$, so that the time required for the radium to be half transformed is about 1280 years. The average life of radium is thus 1850 years.

Volume of the Emanation.—Each atom of radium in breaking up is supposed to produce one atom of emanation. If q_0 is the number of atoms of emanation produced per second per gram, the total number of atoms N_0 present when radioactive equilibrium is reached is given by $N_0 = \frac{q_0}{\lambda}$, where λ is the radioactive constant of the emanation. Now $q_0 = 6.2 \times 10^{10}$, and $1/\lambda = 480000$, thus $N_0 = 3.0 \times 10^{16}$.

But one cubic centimetre of any gas contains 3.6×10^{19} molecules. Thus the maximum volume of the emanation to be obtained from one gram of radium in radioactive equilibrium is equal to $\frac{3.0 \times 10^{16}}{3.6 \times 10^{19}} \text{ c.c.} = 0.83 \text{ cubic millimetres.}$

Now Ramsay and Soddy found experimentally that the volume of emanation to be obtained from one gram of radium was about one cubic millimetre. The numbers are thus in good agreement.

Heating Effect of Radium.—Rutherford and Barnes have shown that the heating effect of radium and of its various products is due to the bombardment of the α particles expelled from them. From measurements of the constants of the α particle, I deduced that its kinetic energy was about $5.9 \times 10^{-6} \text{ erg.}$

Radium in radioactive equilibrium emits 2.5×10^{11} α particles per second per gram. The emission of energy in the form of kinetic energy of the α particles thus corresponds to 126 gram-calories per gram per hour. This number is in fairly good agreement with the value 100, first determined experimentally by Curie and Laborde.

If the heating effect of radium is assumed to be a measure of the kinetic energy of the α particles, we may conversely deduce that the average energy of the α particle emitted from radium and its products is $4.7 \times 10^{-6} \text{ erg.}$

Number of Ions produced by an α particle.—Knowing the number of α particles expelled per second from a thin film

of radium, and the saturation current produced when the radiation is all absorbed in the gas, we can at once deduce the number of ions produced in air at atmospheric pressure and temperature by the passage of a single α particle through it.

A weight of .484 milligram of radium bromide spread in the form of a thin film on an aluminium plate emitted 8.7×10^6 α particles into the gas per second. The saturation current observed between parallel plates at sufficient distance apart to absorb most of the rays was 8.4×10^{-8} ampere. Taking the charge on an ion as 1.13×10^{-19} coulomb, this current corresponds to a production of 7.5×10^{11} ions per second in the gas. But this number was produced by 8.7×10^6 α particles. The average number of ions produced by each α particle, expelled from radium itself, is thus 86000. Now Bragg has shown that an α particle passing through air produces nearly the same number of ions per unit length of its path, and the ionization ceases fairly abruptly. The range in air for the α particles from radium at its minimum activity is about 3.0 cms. The number of ions produced per cm. of path in air at normal pressure and temperature is thus 29000. The number per cm. of path at a pressure of one millimetre of mercury would be 38.

Townsend found that the maximum number of ions produced by an electron per cm. of path at a pressure of one mm. of mercury was 21. At this maximum it was deduced that each collision of the electron with the molecules in its path resulted in the production of ions. Since the α particle produces 38 ions under the same conditions, we may conclude that an α particle is nearly twice as efficient an ionizer as the electron at its maximum efficiency. Such a result indicates that the α particle has a somewhat larger sphere of action than the electron, and is able to ionize about two molecules for the electron's one. This is not unexpected, since the α particle is of atomic dimension, while that of the electron is small compared with an atom.

Energy required to produce an Ion.—A deduction of the average energy required to produce an ion by collision of the α particle with the gas molecules can be made, if the range of velocity, over which the α particles ionize the gas, is determined. I have shown in my paper* "*Some Properties of the α Rays from Radium,*" that the α rays, emitted by a thin film of radium at its minimum activity, are initially projected with a velocity .88 v_0 , where v_0 is the initial velocity of projection of the α particles from radium C. The α particles cease to ionize the gas when the velocity falls.

* Phil. Mag. July 1905.

to $\cdot 64 v_0$. From this it can at once be deduced that $\cdot 48$ of the total energy of the α particle, shot out by radium itself, is absorbed when it ceases to ionize the gas. Now it can be calculated from the heating effect of radium at its minimum activity—25 gram-calories per hour per gram—that the kinetic energy of the α particle is $4\cdot 7 \times 10^{-6}$ erg. The amount of energy absorbed when the α particles just cease to ionize the gas is $2\cdot 3 \times 10^{-6}$ erg. Assuming that this energy is used up in ionization, and remembering that the α particle from radium itself produces 86,000 ions, the average energy required to produce an ion is $2\cdot 7 \times 10^{-11}$ erg. This is equivalent to the energy acquired by an ion moving freely between two points differing in potential by 24 volts.

McGill University, Montreal,
May 1, 1905.

XXVI. *On the Ratio between the Mean Spherical and the Mean Horizontal Candle-Power of Incandescent Electric Lamps.*
By J. A. FLEMING, M.A., D.Sc., F.R.S., Professor of
Electrical Engineering in University College, London*.

A PAPER was read before the Physical Society on November 11th, 1904†, by Mr. G. B. Dyke, B.Sc., in which, amongst other matters, he gave the results of numerous measurements made in the Pender Electrical Laboratory of University College, London, of the ratio of the mean spherical candle-power of incandescent electric lamps to the mean horizontal candle-power, taken when the lamp was rotating round a vertical axis. In all cases the ratio of the mean spherical to mean horizontal candle-power was experimentally found to be a number near to 0·78 for about nine different types of electric glow-lamps. This constant ratio must depend upon definite optical facts, and cannot be a matter of accident. That this is the case can be shown by considering a typical instance.

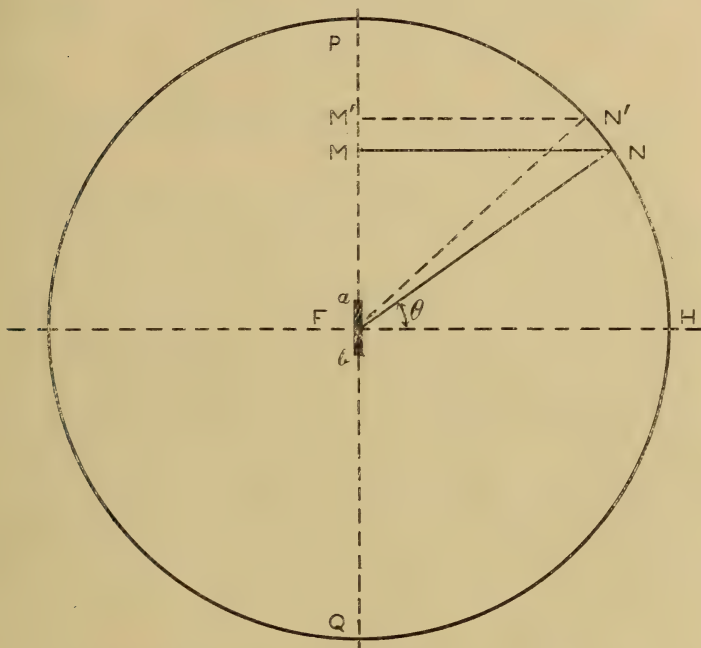
Let us suppose a short straight filament ab (fig. 1) rendered incandescent to be placed vertically in the centre of a sphere described by the revolution of a semicircle PHQ round a diameter PQ , coinciding with the direction of the filament. Let the length of the filament be small compared with the radius $FH=r$ of the sphere. Let I_H represent the

* Communicated by the Physical Society: read June 16, 1905.

† See Mr. G. B. Dyke, "On the Fractical Determination of the Mean Spherical Candle-Power of Incandescent and Arc Lamps," *Phil. Mag.* ser. 6, vol. ix. p. 136, Jan. 1905; also *Proc. Phys. Soc.* vol. xix. p. 399 (1905).

intensity of the light emitted or the candle-power of the filament in a horizontal direction FH , and let I be the intensity in any direction FN making an angle $NFH = \theta$ with the horizontal direction. Let I_s be the mean spherical candle-power.

Fig. 1.



Let us then start with the assumption that $I = I_H \cos \theta$. The justification for this is found in the fact that if a long straight incandescent filament is placed behind a small rectangular-shaped hole in a metallic plate, then inclining the filament to the plane of the plate within certain angular limits makes no sensible difference in the quantity of light which comes through the hole*. Consider then a zone of the sphere swept out by an elementary arc NN' of the semicircle. The surface of this zone is $2\pi r^2 \cos \theta d\theta$, where $d\theta$ denotes the angle $N'FN$.

Hence the quantity of light falling on the whole zone is $I_H 2\pi r^2 \cos^2 \theta d\theta$, and the total quantity of light incident on the

* For the limits within which the above statement is confirmed by experiment the reader is referred to the Appendix to this Paper.

210 Dr. Fleming: *Ratio between Mean Spherical and entire surface of the circumscribing sphere is*

$$4\pi r^2 I_H \int_0^{\frac{\pi}{2}} \cos^2 \theta d\theta$$

Hence by the definition of mean spherical candle-power I_s we have

$$4\pi r^2 I_s = 4\pi r^2 I_H \int_0^{\frac{\pi}{2}} \cos^2 \theta d\theta,$$

or

$$\frac{I_s}{I_H} = \int_0^{\pi} \cos^2 \theta d\theta = \int_0^{\frac{\pi}{2}} \left(\frac{1 + \cos 2\theta}{2} \right) d\theta = \frac{\pi}{4} = 0.785.$$

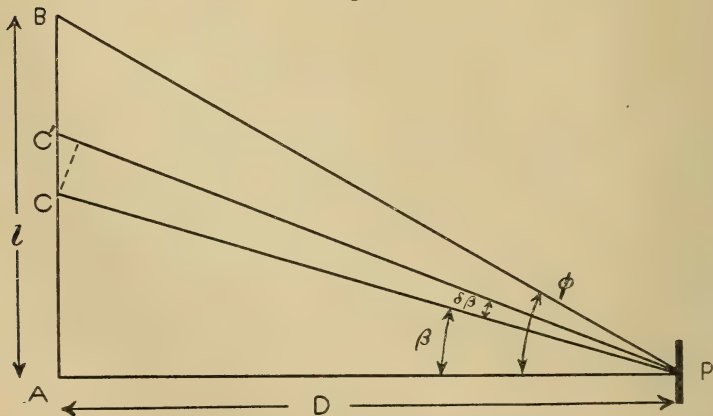
Accordingly for such a short straight filament the ratio experimentally found by Mr. Dyke, viz., $I_s/I_H = 0.78$, holds good.

Conversely, the fact that experiment shows that for any incandescent lamp $I_s/I_H = \pi/4$ nearly, is a proof that the cosine law of luminous radiation from the filaments in different direction holds good, with considerable exactness.

The departure from the above rule when the filament is not short compared with the radius of the sphere can be found as follows:—

Consider a straight incandescent filament of length $2l$ the half of which is represented by the line AB (see fig. 2) placed

Fig. 2.



at a distance $AP = D$ from a photometer-disk P , the filament being parallel to the photometer-plane. Let an element of length at C be denoted by dl . Let the angle APB subtended at

P by half the length of the filament be denoted by ϕ , and let the angle APC be denoted by β . Let the intensity of the light sent out or the candle-power *per unit of length* of the filament in a direction normal to itself be denoted by I_H , then the illumination on the photometer-disk, assuming it to follow the above mentioned cosine law, due to an element dl of the filament at C subtending an angle $d\beta$ at P will be given by the expression

$$\frac{I_H \cos^2 \beta dl}{(CP)^2} = \frac{I_H \cos^4 \beta dl}{D^2}.$$

Also from the geometry of the figure we have

$$dl = \frac{D \cdot d\beta}{\cos^2 \beta}.$$

Hence the illumination on the photometer-disk due to the element dl of the filament at C is given by the expression

$$\frac{I_H D \cdot \cos^2 \beta d\beta}{D^2}.$$

This is the same illumination as that which would be given by a vertical element of length $D d\beta$ placed at A, but having a horizontal intensity or horizontal candle-power per unit of length equal to $I_H \cos^2 \beta$.

To obtain the whole illumination on the photometer-disk due to the whole filament of length $2l$, we have to integrate the above expression for one element, between the limits $\beta=0$ and $\beta=\phi$, and then multiply by 2, where ϕ is half the angle subtended by the whole filament.

Since

$$\int_0^\phi \cos^2 \beta d\beta = \frac{\sin \phi \cos \phi}{2} + \frac{\phi}{2};$$

and since $l \cos \phi = D \sin \phi$,

we see that

$$2 \int_0^\phi \frac{I_H D \cos^2 \beta d\beta}{D^2} = \frac{I_H}{D^2} 2l \left(\frac{\cos^2 \phi}{2} + \frac{\phi \cot \phi}{2} \right).$$

Hence the correcting factor when photometering a filament, the apparent length $2l$ of which subtends a finite angle 2ϕ at the photometer-disk, is $\frac{1}{2}(\cos^2 \phi + \phi \cot \phi)$. Accordingly the ratio of mean spherical to mean horizontal candle-power in this last case is given by the expression

$$\frac{I_s}{I_H} = \frac{\pi}{4} \left\{ \frac{2}{\cos^2 \phi + \phi \cot \phi} \right\}.$$

If the filament is not a straight filament but a loop as in

an actual lamp, which may be so coiled that the different portions lie in different planes, we have an additional small correction to make. We may first notice the effect of the inclination of a straight filament in various directions.

Let three coordinate planes be drawn through the centre of the sphere of reference. Let one of these planes be horizontal. Let the photometer-disk be placed with its plane vertical. Let two other planes be described through the centre of the sphere which are respectively parallel and perpendicular to the photometer-disk plane.

Then consider a straight filament lying in the plane, perpendicular to the photometer-disk and having an inclination to it.

It is clear that if the cosine law of radiation holds good, the light sent out by each element of the inclined filament in a horizontal direction will be the same as that of its projection on a plane parallel to the photometric disk. Hence we may substitute for the inclined filament its projection on a vertical plane, and if we assume the filament and its projection have the same candle-power per unit of length, the horizontal illuminating power will be the same in the two cases. If, therefore, the filament lies in any plane we may consider it divided into equal elements of length, each of which makes its own contribution to the mean spherical and the mean horizontal candle-power. Disregarding for the moment the effect of distance from the horizontal plane on the illuminating effect of each element on the photometer-disk, we see that the ratio of the mean spherical to the mean horizontal candle-power must be greater for each element of length of the filament the more it is inclined to the vertical plane or the less its inclination to the line joining it to the photometer-disk.

Hence if the whole filament lies in one plane placed perpendicularly to the photometer axis the ratio I_s/I_H must be $\pi/4$. If it does not lie wholly in that plane the ratio will be somewhat greater.

In the case of filaments coiled in various ways a small correcting factor is also necessary, providing for the reduced illuminating effect of each equal element of the filament, into which the filament may be considered to be divided, the further the element is removed from the axis of the photometer, that is, from the horizontal plane. In the case of simple horseshoe-shaped filaments, so placed that the whole filament is visible, this correcting factor will be the factor

$$\frac{2}{\cos^2 \phi + \phi \cot \phi}$$

where ϕ is half the angle subtended by the whole filament at the centre of the photometer-disk.

If the filament has a double or multiple loop, then the precise correcting factor for angular magnitude of the whole loop is not quite so sharply defined. It will, however, always operate to increase the fraction or percentage which represents the ratio of mean spherical to mean horizontal candle-power. It will always be a small correction if the whole filament subtends an angle, say, of not more than 10° at the centre of the photometer-disk, as in the ordinary photometry of an incandescent electric lamp.

These considerations show that incandescent lamp manufacturers can without much difficulty furnish the figures required to give the true photometric efficiency of any type of lamp. Instead of stating, as at present, the value of the maximum candle-power taken in one direction, viz., the direction perpendicular to the axis of the lamp in which the whole of the filament is seen exposed, they should furnish the mean horizontal candle-power taken in the same direction when rotating the lamp around its vertical axis, using for this purpose well-known mechanical devices.

If this reading is taken at such a distance from the lamp that the whole filament does not subtend a greater angle than 5° to 10° at the photometer-disk, then the mean spherical candle-power of the incandescent lamp is for practical purposes very nearly obtained by multiplying this mean horizontal candle-power by the factor $\pi/4$. From the measurement of the electric power in watts supplied to the lamp, we have at once the means of calculating the total flux of light per watt which fixes scientifically the efficiency of the lamp as an energy-transforming device.

APPENDIX.

To enable a judgment to be made as to the validity of the assumption that the intensity of the light proceeding from a filament in any direction varies as the cosine of the angle between the ray and the normal to the element, the following experiments were tried by Mr. G. B. Dyke at the suggestion of the writer.

A tubular incandescent carbon-filament lamp was prepared with a straight filament about 10 inches in length. This was placed on a photometric gallery at a distance of about 40 inches from a Lummer-Brodhun photometer, and two baffle-screens interposed having circular holes in them about 1 inch in diameter. These screens were placed so as to divide the

space between the photometer and the lamp into three nearly equal parts.

The tubular lamp had a protractor attached to it, and could be tilted over at various angles to the vertical line, keeping it in one vertical plane normal to the photometer-disk. It is obvious that if the illumination sent out from unit of length of the straight filament obeys the cosine law above mentioned, then tilting the lamp as described to various angles should make no difference in the observed illumination on the photometer. As a matter of fact, the observed illumination decreased rapidly as the lamp was tilted over, in accordance with the figures in the following table.

Angle between the straight filament and the plumb-line through its centre.	Observed candle-power or illumination through the two baffle-screens.
0	2.33
15	2.27
30	2.23
45	2.15
60	1.96
75	1.06

It is clear, however, that some part of the decrease of light at high inclinations is due to the loss of light by reflexion at the inner surface of the tubular glass bulb. To obtain the correction for this reflexion a sheet of glass was interposed between an ordinary lamp and the photometer, the plane of the glass being tilted at various angles to the plane normal to the ray. The photometric readings were as follows :—

Angle at which sheet of glass was placed to the plane normal to the ray.	Observed intensity of the light.
0	26.1 c.p.
15	25.9 "
30	25.8 "
45	25.2 "
60	25.4 "
75	16.7 "

It follows therefore, that if the carbon filament in the tubular lamp could have been used without a glass envelope, the light sent by it through the baffle-screens would have been greater in the ratio of the first term in the right-hand column of the previous table to any succeeding term in the same column corresponding to the same angle of inclination of the filament.

If therefore we call the intensity of the light sent through the screens unity when the filament is vertical, then its true

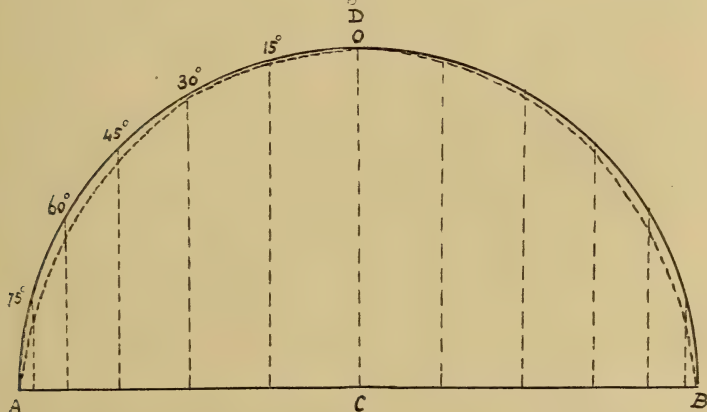
intensity corrected for reflexion when the lamp is tilted to various angles is as follows :—

Angle of Inclination.	Corrected Value of Intensity of Light emitted per unit of length.
0	$2.33 \times 26.1 \div 2.33 \times 26.1 = 1.00$
15	$2.27 \times 26.1 \div 2.33 \times 25.9 = .98$
30	$2.33 \times 26.1 \div 2.33 \times 25.8 = 1.00$
45	$2.15 \times 26.1 \div 2.33 \times 25.2 = .96$
60	$1.96 \times 26.1 \div 2.33 \times 25.4 = .86$
75	$1.06 \times 26.1 \div 2.33 \times 16.7 = .71$

The figures in the last column therefore represent the intensity of the light sent out by unit length of the incandescent filament at various angles of the normal. If the cosine law were strictly fulfilled these values should all be unity.

The easiest way to determine the effect of this departure from the true cosine law on the mean spherical candle-power, is to describe a semicircle (firm line) of unit radius and to set off on its diameter distances from the centre C (see fig. 3)

Fig. 3.



proportional to the sines of the angles 0° , 15° , 30° , &c., and then through these points to draw ordinates (dotted lines) to the diameter of the semicircle. Fractions of these ordinates equal to the fractions in the last column of the above table are then set off, and the upper ends of these define a curve (dotted line) which is the Rousseau diagram of the photometric curve of luminous radiation of the filament.

If the value of the mean ordinate of this last (dotted) curve is taken, it gives us the ratio of the mean spherical to the mean horizontal candle-power of the straight filament. For the (dotted) curve as drawn in fig. 3 delineating the observations made with the above-mentioned carbon filament, this

mean ordinate or corrected ratio comes out 0.77 instead of 0.785 as it should do if the cosine law had been strictly fulfilled. In this case the dotted line would have fallen on the semicircle and coincided with it.

Another similar set of measurements was made with a straight oxide filament taken from a Nernst lamp used without heating-coil, and with the supports of the filament all carefully painted dead-black to avoid errors due to reflected light. In this last case no correction due to loss of light by reflexion from a glass envelope was needed, as the Nernst filament was used in the open air. The ratio of the mean spherical to mean horizontal filament in this last case was experimentally found to be 0.785, or exactly the value it should have if the cosine law is obeyed.

We have therefore in these experiments a justification for the assumption made in this paper for the law of luminous radiation from a straight incandescent filament.

My thanks are due to Mr. Dyke for his assistance in the experimental portion of this work.

XXVII. *On the Flux of Light from the Electric Arc with Varying Power-Supply.* By G. B. DYKE, B.Sc.*

[Plate II.]

IN 1896 a paper was communicated to the Physical Society by Dr. J. A. Fleming, F.R.S., and Mr. J. E. Petavel †, recording the results of numerous observations on the Electric Arc, dealing, amongst other matters, with the question of the relation between the flux of light, or mean spherical candle-power, and the watts expended in the arc.

The time and labour necessary to obtain the mean spherical candle-power of any source of light from observations made on the point-by-point method then employed, prevented any very extended study of this relation.

The construction of the integrating photometer, described by the author in his paper "On the Practical Determination of the Mean Spherical Candle Power of Incandescent and Arc Lamps" ‡, having, to a very large extent, removed these difficulties, it was thought that a more extended series of observations in this direction might be undertaken, and facilities for this purpose were kindly granted in the Pender

* Communicated by the Physical Society: read March 24, 1905.

† "An Analytical Study of the Alternating-Current Arc," by J. A. Fleming, M.A., D.Sc., F.R.S., and G. E. Petavel, Phil. Mag. April 1896, p. 315.

‡ "On the Practical Determination of the Mean Spherical Candle-Power of Incandescent and Arc Lamps," by G. B. Dyke, B.Sc., Phil. Mag. Jan. 1905, p. 136, and Proc. Phys. Soc. vol. xix.

Electrical Laboratory of University College, London, by Prof. J. A. Fleming.

The objects in view may be thus stated :—

- (i.) To obtain a series of curves for both continuous and alternating current arcs, showing the relation between the mean spherical candle-power and the watts taken up in the arc.
- (ii.) To compare the “efficiencies” of the continuous and alternating arcs as illuminants when supplied at different powers, the “efficiency” being expressed in mean spherical candles per watt.

As the field covered by these statements was too broad to be covered in its entirety in the time at the author’s disposal, it was decided to impose the following restrictions :—

The make and diameters of the carbons used to remain unaltered throughout the experiments.

Alternating current of one frequency only to be employed, this frequency to be 80~

In continuous-current experiments the top carbon to be invariably positive.

Arc-lengths not to exceed 0·5 inch.

Power to be supplied at rates not greater than 1500 watts.

The carbons used may be specified thus :—

Upper Carbon—

Maker	C. Conradty, Nuernberg.
Description	Cored. Marke W.C.
Diameter	12 mm.
Diameter of core	2 „
Weight per cub. centim.	1·41 gramme,
Weight per c.c. of outer carbon	1·43 „
Weight per c.c. of core	0·93 „

Lower Carbon—

Maker	C. Conradty, Nuernberg.
Description	Solid. Marke C.
Diameter	10 mm.
Weight per cub. centim.	1·46 gramme.

The mean spherical candle-power was measured directly by means of an integrating photometer.

This instrument was described in detail in the paper referred to above, and it will be sufficient to say that it consists of a system of pairs of mirrors arranged in a semi-circle, about 6 feet in diameter, in such a manner that light emitted from a source placed at the centre of the system shall be incident upon a photometer-screen, also situated at the centre, at the same angle with the vertical as that which it made on emission from the source.

The intensity of illumination of the screen is then proportional to the mean spherical candle-power. As previously arranged for testing incandescent lamps, the photometer measured directly the ratio of the mean spherical candle-power to the mean horizontal candle-power. When dealing with arc-lamps, however, this ratio is not of much practical value, so a small alteration was made to adapt the instrument to read the absolute value of the mean spherical candle-power. This was effected by replacing the horizontal sliding pair of mirrors by a standard incandescent lamp on a sliding support.

This alteration will of course alter the constant of the instrument.

The formula given before was

$$\frac{\text{M.S.C.P.}}{\text{M.H.C.P.}} = \frac{5.09}{d^2}$$

where d is the distance in feet from source to screen by way of the sliding pair of mirrors.

Now the sliding pair of mirrors reflected 70.5 per cent. of the light incident on them; hence it is obvious that if these mirrors are replaced by a lamp of known candle-power C.P. we have

$$\text{M.S.C.P.} = \frac{5.09}{.705} \times \frac{\text{C.P.}}{d^2}.$$

Hence the formula becomes

$$\text{M.S.C.P.} = \frac{7.22}{d^2} \times \text{C.P. of balancing-lamp},$$

where d is the distance of balancing-lamp from photometer-screen in feet.

As this result is obtained somewhat indirectly the arc was replaced by an incandescent lamp, and the polar diagram constructed by opening the slides in front of the mirrors one at a time and making a photometric measurement for each.

The Rousseau diagram was then drawn, and the mean spherical candle-power deduced and found to be 22.49. All the mirror-slides were then opened and a balance obtained at 15.95 inches, the candle-power of the balancing-lamp being 5.51.

Hence, applying the formula given above,

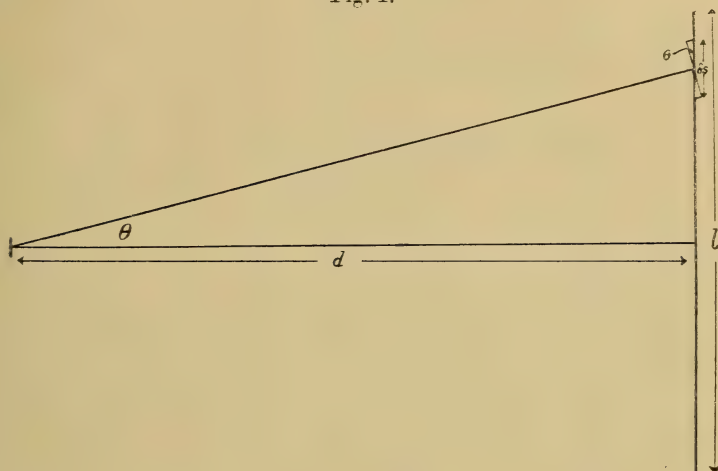
$$\begin{aligned} \text{constant of instrument} &= \frac{22.49}{5.51} \times \left(\frac{15.95}{12} \right)^2 \\ &= 7.21. \end{aligned}$$

This check-experiment being considered satisfactory, the value 7.22 was taken as the constant of the instrument.

When dealing with arcs of some 2000 mean spherical candle-power, it was necessary to bring the balancing-lamp very close to the photometer.

In this case it was necessary to apply a correction to the

Fig. 1.



scale-reading of the distance d , due to the angle subtended by the filament of the balancing-lamp becoming applicable.

If l is the length of the filament, and i the brightness per unit-length.

Then, considering a flat-loop lamp at a distance d from the photometer (see fig. 1), we have :—

Intensity of illumination due to 2 legs of filament

$$\begin{aligned}
 &= 2 \left\{ 2i \int_0^{\frac{l}{2}} (\delta s \cos \theta) \cos \theta / \left(\frac{d}{\cos \theta} \right)^2 \right\} \\
 &= 4i \int_0^{\tan^{-1} \frac{l}{2d}} \left(\frac{d}{\cos \theta} \times \delta \theta \right) \cos \theta \times \left(\frac{\cos \theta}{d} \right)^2 \\
 &= \frac{4i}{d} \int_0^{\tan^{-1} \frac{l}{2d}} \cos^2 \theta \delta \theta \\
 &= \frac{2i}{d} \left[\theta + \frac{1}{2} \sin 2\theta \right]_0^{\tan^{-1} \frac{l}{2d}} \\
 &= \frac{2i}{d} \left[\tan^{-1} \frac{l}{2d} + \frac{\frac{1}{2}ld}{d^2 + (\frac{1}{2}l)^2} \right]
 \end{aligned}$$

or approximately

$$\begin{aligned}
 &= \frac{2i}{d} \left[\frac{\frac{1}{2}d}{\sqrt{d^2 + (\frac{1}{2}l)^2}} + \frac{\frac{1}{2}ld}{d^2 + (\frac{1}{2}l)^2} \right] \\
 &= \frac{il}{d} \left[\frac{d + \sqrt{d^2 + (\frac{1}{2}l)^2}}{d^2 + (\frac{1}{2}l)^2} \right] \\
 &= \frac{2il}{d^2} \left(\frac{d^2 + d\sqrt{d^2 + (\frac{1}{2}l)^2}}{2(d^2 + (\frac{1}{2}l)^2)} \right)
 \end{aligned}$$

Hence

d^2 must be multiplied by the factor

$$\frac{2\{d^2 + (\frac{1}{2}l)^2\}}{d^2 + d\sqrt{d^2 + (\frac{1}{2}l)^2}}$$

The author is indebted to Mr. W. C. Clinton, B.Sc., for bringing this correction to his notice.

A hand-regulated arc was used in the experiments, the length of arc being kept constant by observing the image of the arc thrown on to a screen by means of a lens.

During each series of experiments the arc was maintained at a constant length, whilst the power supplied was varied by means of an adjustable resistance.

In order to obtain a mean result ten photometric readings were taken at each setting.

Altogether eight lengths of arc were employed, viz.

$\frac{1}{16}$ in., $\frac{3}{32}$ in., $\frac{1}{8}$ in., and hence by 16ths to $\frac{7}{16}$ in.

The table which follows gives the mean results of these experiments, involving nearly four thousand observations.

If now these results are plotted, taking watts as abscissæ and mean spherical candle-power as ordinates, we obtain the series of curves shown in Pl. II., the full-line curves referring to continuous-current arcs and the dotted lines to alternating-current arcs.

It will be seen that, within the limits of experimental error, the relation between mean spherical candle-power and watts follows a straight-line law. This straight line, however, does not pass through the origin, but for zero candle-power there is still an outstanding amount of power amounting to some 200 to 400 watts.

The amount of this outstanding effect increases, in general, with the arc-length, and is probably due to the energy dissipated as heat by radiation, and as chemical energy in evaporation of the carbon at the crater.

The author hopes, at some future date, to investigate this matter experimentally.

It is found from these curves that for each arc-length the point in which the efficiency curve cuts the axis of watts is

Arc length. Inches.	Continuous Current Arc.				Alternating Current Arc.			
	Volts.	Amperes.	Watts.	M.S.C.P.	Volts.	Amperes.	Watts.	M.S.C.P.
$\frac{1}{16}$	50.0	6.4	318	207	39.8	5.6	221	155
	46.3	7.7	355	267	37.8	7.1	268	236
	44.7	9.3	417	370	37.4	9.2	344	359
	45.1	12.1	544	542	(violent hissing if current increased)			
	45.4	16.3	741	777				
$\frac{3}{32}$	44.2	20.1	890	1078				
	50.5	6.7	336	257	47.5	6.7	317	156
	48.9	7.9	384	354	40.5	9.3	378	293
	48.0	9.6	459	490	39.7	11.2	442	445
	47.0	12.4	583	738	39.8	13.4	533	624
$\frac{1}{8}$	46.4	16.9	786	1119	40.1	16.0	643	855
	45.3	20.9	947	1476	40.7	19.8	807	1104
	53.5	6.0	323	290	43.8	6.8	299	124
	51.0	7.5	382	390	42.1	8.6	362	241
	52.2	9.2	481	609	41.6	10.3	429	297
$\frac{3}{16}$	49.5	12.3	609	901	41.7	12.5	520	477
	47.5	16.8	796	1295	42.1	14.6	616	579
	47.0	26.4	961	1761	42.2	18.8	793	846
					40.3	20.1	811	977
	58.0	5.7	333	237	51.0	4.9	250	80
$\frac{1}{4}$	54.6	7.1	386	353	48.2	6.3	304	132
	52.1	8.9	462	524	45.4	8.5	384	244
	51.2	11.7	599	767	43.5	12.6	547	543
	48.8	16.3	796	1219	42.1	15.8	655	712
	48.8	20.2	985	1669	42.2	19.9	837	990
$\frac{5}{16}$	64.6	6.8	441	410	56.1	4.6	259	87
	60.9	8.7	527	683	53.9	5.4	291	154
	59.4	12.2	727	1161	50.6	7.1	360	196
	55.9	15.4	863	1532	46.0	10.7	490	432
	57.0	19.4	1104	2210	45.6	15.9	727	863
$\frac{3}{8}$	63.0	8.7	548	549	57.9	8.3	478	270
	60.8	10.4	632	727	56.3	10.5	617	382
	59.0	12.5	728	921	53.6	13.7	735	596
	58.5	14.9	871	1284	49.2	17.5	861	797
	61.1	18.8	1150	1900	48.9	21.7	1062	1024
$\frac{7}{16}$	67.2	6.8	459	350	57.7	8.1	470	238
	62.5	9.0	565	557	53.2	11.3	602	446
	62.1	12.2	760	1130	54.1	14.1	763	720
	65.8	10.0	655	621	56.8	10.5	593	407
	65.9	11.6	762	810	56.6	13.8	783	660
	64.9	14.3	927	1202	55.6	18.1	1003	1070
	62.4	18.9	1182	1703				

the same whether the arc is supplied with continuous or alternating currents, pointing to the fact that the power expended in forms of energy other than light, *e. g.* volatilization of carbon, radiation of heat, sound, &c., is exactly the same for similar alternating and continuous arcs.

For lengths of arc greater than $\frac{1}{8}$ in. the ratio of the efficiencies of the continuous and alternating arcs is about 3 to 2, agreeing very well with that obtained by Dr. Fleming and Mr. Petavel.

If, however, the arc be made shorter than $\frac{1}{8}$ in. the efficiency line for the alternating-current arc will be found to gradually approach the line for the continuous-current arc until for an arc-length of $\frac{3}{32}$ in. they are practically coincident (Pl. II.).

The arc is quite stable at this point, and even in the alternating-current case may be supplied at more than 600 watts without causing hissing.

Any further decrease in the arc-length will result in the alternating-current efficiency line rising above the continuous-current line until at an arc-length of $\frac{1}{16}$ in. they have the position shown in the first curve in Pl. II.

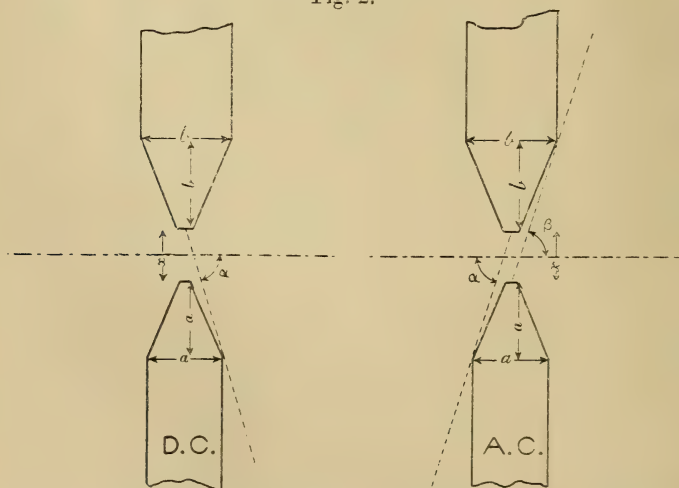
With so small an arc-length, however, the alternating-current arc begins to hiss badly when supplied at a rate greater than 350 watts, although the continuous-current arc is still silent up to 900 watts.

As far as the author is aware, this is the first case in which an alternating current arc of efficiency greater than the corresponding continuous-current arc has been realized in practice, although Dr. Fleming and Mr. Petavel recognized the possibility of such a phenomenon in their paper referred to above.

Why, then, does the alternating-current arc give the same efficiency as the continuous-current arc at this arc-length?

This can perhaps be most clearly seen by making some simple approximations with regard to the shape of the carbons, and calculating the length of arc necessary to give the same candle-power with the same power supply.

Fig. 2.



Let the carbons be supposed to assume the shape indicated in fig. 2, truncated cores whose heights are equal to their

bases; this being approximately the shape found by experiment.

Also let all the light in the case of the continuous-current arc be supposed to come from the positive crater.

Now, the expression for the mean spherical candle-power of a source symmetrical about an axis is

$$\text{M.S.C.P.} = \frac{1}{2} \int_{\frac{\pi}{2}}^{-\frac{\pi}{2}} I_{\theta} \cos \theta d\theta,$$

where I_{θ} is the luminous intensity in a direction making an angle θ with the horizontal.

Now let I be the luminous intensity of the crater when viewed normally.

Then, as a rough approximation, assuming the crater to be a flat circular disk, small compared with the arc length,

$$I_{\theta} = I \sin \theta.$$

Hence for the arc

$$\begin{aligned} \text{M.S.C.P.} &= \frac{1}{2} I \int_{\frac{\pi}{2}}^{-\frac{\pi}{2}} \sin \theta \cos \theta d\theta \\ &= \frac{1}{4} I \int_{\frac{\pi}{2}}^{-\frac{\pi}{2}} \sin 2\theta d\theta \\ &= \frac{1}{8} I \left[-\cos 2\theta \right]_{\frac{\pi}{2}}^{-\frac{\pi}{2}} \end{aligned}$$

Let a be the diameter of the lower carbon (—), and b the diameter of the upper carbon (+).

Then, making the above assumptions, all the light, in the continuous-current case, will be concentrated into the zone between the horizontal and an angle α below the horizontal: where

$$\alpha = \tan^{-1} \frac{a+x}{\frac{1}{2}a}.$$

Hence we get

$$\begin{aligned} \text{M.S.C.P.}_{\text{C.C.}} &= \frac{1}{8} I_{\text{C.C.}} \left[-\cos 2\theta \right]_0^{\alpha} \\ &= \frac{1}{4} I_{\text{C.C.}} \sin^2 \alpha. \end{aligned}$$

Similarly for the alternating-current arc, considering each

crater separately, we obtain

$$\text{M.S.C.P.}_{\text{A.C.}} = \frac{1}{4} I_{\text{A.C.}} (\sin^2 \alpha + \sin^2 \beta),$$

where

$$\beta = \tan^{-1} \frac{b+x}{\frac{1}{2}b}.$$

Hence

$$\frac{\text{M.S.C.P.}_{\text{C.C.}}}{\text{M.S.C.P.}_{\text{A.C.}}} = \frac{I_{\text{C.C.}}}{I_{\text{A.C.}}} \frac{\sin^2 \alpha}{\sin^2 \alpha + \sin^2 \beta}.$$

Some experiments mentioned by Dr. Fleming and Mr. Petavel give :—

for a continuous-current arc

$$I_{\theta} = 910 \quad \text{when} \quad \theta = 40.5^{\circ}.$$

Hence

$$I_{\text{C.C.}} = \frac{910}{\sin 40.5^{\circ}} = 1400;$$

and for a similar alternating-current arc

$$I_{\theta} = 300 \quad \text{when} \quad \theta = 60^{\circ} \quad \text{for top crater,}$$

and

$$I_{\theta} = 370 \quad \text{when} \quad \theta = 63^{\circ} \quad \text{for bottom crater.}$$

Hence as a mean

$$I_{\text{A.C.}} = 707.$$

So we get

$$\frac{I_{\text{C.C.}}}{I_{\text{A.C.}}} = \frac{1400}{707} = 1.99.$$

Therefore

$$\frac{\text{M.S.C.P.}_{\text{C.C.}}}{\text{M.S.C.P.}_{\text{A.C.}}} = 1.99 \frac{\sin^2 \alpha}{\sin^2 \alpha + \sin^2 \beta}.$$

Hence for equal efficiency we have

$$1.99 \sin^2 \alpha = \sin^2 \alpha + \sin^2 \beta.$$

$$\therefore .99 \sin^2 \alpha = \sin^2 \beta;$$

or putting in the values of the angles

$$\frac{.99(a+x)^2}{(a+x)^2 + .25a^2} = \frac{(b+x)^2}{(b+x)^2 + .25b^2}.$$

Now

$$a = 10 \text{ mm.}$$

$$b = 12 \text{ mm.}$$

Hence

$$\frac{.99(10+x)^2}{(10+x)^2 + 25} = \frac{(12+x)^2}{(12+x)^2 + 36}.$$

Writing out and arranging

$$x^4 + 44x^3 - 340x^2 - 6000x + 18000 = 0.$$

Solving this equation graphically we obtain as the positive finite value of x

$$x = 2.7 \text{ mm.},$$

which agrees very closely with that found by experiment.

This consideration, however, by no means gives a complete solution of the problem of the relative efficiencies of the continuous and alternating current arc, but is only intended to account to some extent for the phenomenon of continuous and alternating current arcs of equal efficiency.

A complete analysis of the subject could only be undertaken on the basis of the evidence of a much more extended series of experiments, of which the time at the author's disposal would not allow.

In conclusion the author wishes to thank Dr. J. A. Fleming, F.R.S., for his many suggestions and constant advice, and also for his kindness in placing the resources of the Pender Laboratory at his disposal, and to express his indebtedness to Messrs. J. S. Westerdale and H. C. Bullman for the very efficient assistance they have rendered him, at great personal inconvenience, in making some thousands of electrical measurements, without which the investigation would have been impossible.

XXVIII. *On Electrical Vibrations between Confocal Elliptic Cylinders, with special reference to Short Waves.* By J. W. NICHOLSON, B.Sc. (Lond. & Vict.), Trinity College, Cambridge*.

SOLUTIONS of the problem of electrical vibrations in confined spaces, bounded by perfectly conducting surfaces, have hitherto been limited to the cases of the circular cylinder (*vide* J. J. Thomson, 'Recent Res. in Elect. and Magn.') and the sphere (Macdonald, 'Electric Waves'). In the present paper, it is proposed to show that the case of the elliptic cylinder, although in general devoid of simplicity, will yet furnish results of great elegance, when the solution is merely carried to a close approximation.

When the surfaces are perfectly conducting, they cause no

* Communicated by the Author.

dissipation of energy, and the surface conditions may all be included in the statements:—

(1) The resultant magnetic induction at the surface is tangential.

(2) The resultant electric force is normal.

In an oscillation, these vectors are perpendicularly situated in the plane of the wave-front; hence the latter plane is tangential or normal to the surface.

The problem is confined to the case of an elliptic cross-section. If the section were parabolic or hyperbolic, the energy would be radiated into space, and the oscillation could not be permanent. If the axis of z be that of the cylinders, and (xy) the plane of a section perpendicular to the axis, a point of space is defined by $(\lambda\mu z)$, where (λ, μ) are the roots of

$$\frac{x^2}{\lambda} + \frac{y^2}{b^2 + \lambda} = 1.$$

$\lambda = \text{const.}$, $\mu = \text{const.}$, being systems of elliptic and hyperbolic cylinders respectively.

The space elements in the directions λ, μ are

$$\begin{aligned} dn_1 &= \frac{d\lambda}{2p_1}, & dn_2 &= \frac{d\mu}{2p_2}, \\ \text{where} & & p_1 &= \sqrt{\frac{\lambda(b^2 + \lambda)}{\lambda - \mu}}, \\ & & &= \sqrt{\frac{P(\lambda)}{\lambda - \mu}} \text{ (say),} \\ p_2 &= \sqrt{\frac{-P(\mu)}{\lambda - \mu}}. \end{aligned}$$

In the usual electrical notation, the current is denoted by (u, v, w) , the electric force by (X, Y, Z) , and the magnetic force and magnetic induction by (a, b, c) .

The two latter vectors are identical, if the medium between the two surfaces be non-magnetic.

If V is the velocity of light, and the current is entirely æthereal,

$$4\pi V^2(u, v, w) = (X, Y, Z). \quad . \quad . \quad . \quad (1)$$

By the circuital relation of Ampère,

$$\left. \begin{aligned} \frac{4\pi u}{p_2} &= \frac{\partial}{\partial \mu} (2c) - \frac{\partial}{\partial z} \left(\frac{b}{p_2} \right), \\ \frac{4\pi v}{p_1} &= \frac{\partial}{\partial z} \left(\frac{a}{p_1} \right) - \frac{\partial}{\partial \lambda} (2c), \\ \frac{2\pi w}{p_1 p_2} &= \frac{\partial}{\partial \lambda} \left(\frac{b}{p_2} \right) - \frac{\partial}{\partial \mu} \left(\frac{a}{p_1} \right) \end{aligned} \right\} . \quad . \quad . \quad . \quad (2)$$

By that of Faraday,

$$\left. \begin{aligned} -\frac{\dot{a}}{p_2} &= \frac{\partial}{\partial \mu} (2Z) - \frac{\partial}{\partial z} \left(\frac{Y}{p_2} \right), \\ -\frac{\dot{b}}{p_1} &= \frac{\partial}{\partial z} \left(\frac{X}{p_1} \right) - \frac{\partial}{\partial \lambda} (2Z), \\ -\frac{\dot{c}}{2p_1p_2} &= \frac{\partial}{\partial \lambda} \left(\frac{Y}{p_2} \right) - \frac{\partial}{\partial \mu} \left(\frac{X}{p_1} \right). \end{aligned} \right\} \dots \dots (3)$$

Dots denoting differential coefficients with respect to time.

The conditions that (2) and (3) should each form a consistent scheme are

$$\left. \begin{aligned} \frac{\partial}{\partial \lambda} \left(\frac{X}{p_2} \right) + \frac{\partial}{\partial \mu} \left(\frac{Y}{p_1} \right) + \frac{1}{2} \frac{\partial}{\partial z} \left(\frac{Z}{p_1p_2} \right) &= 0, \\ \frac{\partial}{\partial \lambda} \left(\frac{a}{p_2} \right) + \frac{\partial}{\partial \mu} \left(\frac{b}{p_1} \right) + \frac{1}{2} \frac{\partial}{\partial z} \left(\frac{c}{p_1p_2} \right) &= 0. \end{aligned} \right\} \dots \dots (4)$$

It must be recalled that (X, Y, Z) and the other vectors are in these equations defined by components along the directions λ, μ, z , and not by the rectangular components.

We now examine the possibility of a type of oscillation in which two of the components (X, Y, Z) of electric force are zero.

If $Y=0, Z=0$.

Then by (4)

$$X = p_2 \phi(\mu, z). \dots \dots (5)$$

If λ is the wave-length of the oscillation, and $k = \frac{2\pi}{\lambda}$,

$$\frac{\ddot{X}}{V^2} = -k^2 X.$$

The circuital relations readily give, in this case,

$$a=0; \dots \dots (6)$$

and by combination lead to the equation for X,

$$\frac{\partial^2 X}{\partial z^2} + k^2 X + \frac{4}{\sqrt{P(\lambda)}} \cdot \sqrt{\frac{-P(\mu)}{\lambda - \mu}} \cdot \frac{\partial}{\partial \mu} \sqrt{\frac{-P(\mu)}{\lambda - \mu}} \cdot \frac{\partial}{\partial \mu} (X \sqrt{\lambda - \mu}). \quad (7)$$

But by (5),

$$X = \sqrt{\frac{-P(\mu)}{\lambda - \mu}} \cdot \phi(\mu, z);$$

$$\therefore \frac{\partial^2 \phi}{\partial z^2} + k^2 \phi + 4 \frac{\partial}{\partial \mu} \sqrt{\frac{-P(\mu)}{\lambda - \mu}} \frac{\partial}{\partial \mu} (\phi \sqrt{-P(\mu)}) = 0.$$

Since ϕ cannot contain λ , this gives the two equations

$$\left. \begin{aligned} \frac{\partial}{\partial \mu}(\phi \sqrt{-P(\mu)}) &= 0, \\ \frac{\partial^2 \phi}{\partial z^2} + k^2 \phi &= 0. \end{aligned} \right\}$$

Hence

$$\phi = \frac{A}{\sqrt{-P(\mu)}} \cos(kz + \alpha) \cos(kVt + \epsilon);$$

whence the components of electric force are

$$X = \frac{A}{\sqrt{\lambda - \mu}} \cdot \cos(kz + \alpha) \cdot \cos(kVt + \epsilon), \quad Y = Z = 0. \quad (8)$$

By equation (6) the first surface condition is satisfied; and since (Y, Z) both vanish, so also is the second.

This solution represents the simple case of waves propagated along z ; and we note that the force is independent of the size of the boundaries. It holds also inside a single cylinder.

X is nowhere infinite, for $\lambda = \mu$ only on the imaginary curve

$$(x^2 + y^2 - b^2)^2 = -4x^2b^2.$$

The *magnetic force* at any point is normal to the confocal hyperbola through it, and has the value

$$b = - \frac{A}{V\sqrt{\lambda - \mu}} \cdot \sin(kz + \alpha) \cdot \sin(kVt + \epsilon). \quad (9)$$

It is readily seen that this is the only motion of the kind; for if we had taken $(X, Z) = 0$, we should have found

$$Y = \frac{B}{\sqrt{\lambda - \mu}} \cos(kz + \beta) \cos(kVt + e),$$

which cannot vanish at the conductors unless $B = 0$.

Again, if $(X, Y) = 0$, Z is a mere trigonometric function of z and t , and cannot vanish at the conductors unless it does so everywhere.

Finally, when waves are propagated along the axis, the electric and magnetic forces are normal to the confocal elliptic and hyperbolic cylinders respectively, and are of magnitudes given by (8) and (9).

General Solution.

It is readily seen from the equations and surface conditions that the general solution can be built up from two particular

Let $\lambda = b^2 \sinh^2 \xi$, and it becomes

$$\frac{d^2 L}{d \xi^2} = (\theta - k^2 b^2 \sinh^2 \xi) L, \quad \dots \quad (18)$$

which is known as the equation of the elliptic cylinder.

Let $E(\xi)$, $F(\xi)$ be two principal solutions.

If $\mu = -b^2 \sin^2 \eta$, we find similarly

$$\frac{d^2 M}{d \eta^2} = -(\theta + k^2 b^2 \sin^2 \eta) M. \quad \dots \quad (19)$$

And if solutions be $E_1(\eta)$, $F_1(\eta)$, then the magnetic force is given by

$$c = \{E(\xi) + AF(\xi)\} \{E_1(\eta) + BF_1(\eta)\} \cos (kVt + \epsilon), \quad \dots \quad (20)$$

the summation being for all possible values of θ .

While the surface condition gives, by (14),

$$\left[\frac{\partial c}{\partial \xi} \right] = 0; \quad \dots \quad (21)$$

therefore if ξ_1 , ξ_2 denote the elliptic surfaces, the free periods of the system arise from the values of k given by

$$\frac{E'(\xi_1)}{F'(\xi_1)} = \frac{F'(\xi_2)}{E'(\xi_2)}. \quad \dots \quad (22)$$

Owing to the impossibility of finding even a definite integral to represent the functions E , F , whose properties may be found in Forsyth (*'Theory of Differential Equations'*), this solution is of little interest. We now proceed to examine certain approximations.

If two very nearly spherical boundaries be considered, L must return to its old value as a point defined by η describes a path on a particular boundary.

We will therefore regard $(-\theta)$ as the square of a positive integer, and write it n^2 . In general,

$$\theta = -n^2 + a_0(kb)^2 + a_1(kb)^4 \dots,$$

where a_0 , a_1 , &c. decrease to zero when n is great.

Then the equation for L is

$$\frac{d^2 L}{d \xi^2} + (n^2 + k^2 b^2 \sinh^2 \xi) L = 0. \quad \dots \quad (23)$$

It will be noticed that the periods form a doubly infinite system, for the values of n are singly infinite in number, and each value of n gives rise to a singly infinite number of periods. We proceed to the case where n is very great.

Let $\frac{kb}{n} = \epsilon$, when n is large,

$$\therefore \frac{d^2 L}{d\xi^2} + n^2(1 + \epsilon^2 \sinh^2 \xi) L = 0. \quad (24)$$

The approximate value of the solution in this case may be obtained by a method due to Webb (Proc. Roy. Soc. lxxiv. 1904, p. 315).

Put $L = e^{in\phi} \cdot \psi$,

where (ψ, ϕ) are functions of ξ .

Substituting in the equation, and equating coefficients of n, n^2 to zero, since the terms are of different orders,

$$\therefore -\left(\frac{d\phi}{d\xi}\right)^2 + (1 + \epsilon^2 \sinh^2 \xi) = 0,$$

and

$$2 \frac{d\phi}{d\xi} \cdot \frac{d\psi}{d\xi} + \psi \frac{d^2 \phi}{d\xi^2} = 0;$$

$$\therefore \phi = \pm \int_0^\xi \sqrt{1 + \epsilon^2 \sinh^2 \xi} \cdot d\xi, \quad (25)$$

and

$$\psi = \frac{C}{\sqrt{\phi'}}; \quad (26)$$

$$\therefore L = \frac{A \cos n\phi + B \sin n\phi}{\sqrt{\phi'}}, \quad (27)$$

where (A, B) are functions of (η, t) only.

The free periods are given by $\frac{dL}{d\phi} = 0$, at $\phi = \phi_1, \phi_2$, which leads to

$$\tan n(\phi_1 - \phi_2) = \frac{n(\psi_2 \psi_1' - \psi_1 \psi_2')}{n^2 \psi_1 \psi_2 + \psi_1' \psi_2'}.$$

To the present order of approximation

$$\tan n(\phi_1 - \phi_2) = \frac{\psi_2 \psi_1' - \psi_1 \psi_2'}{n \psi_1 \psi_2}. \quad (28)$$

This can be expressed in terms of elliptic functions, for

$$\phi = \int_0^\xi \sqrt{1 + \epsilon^2 \sinh^2 \xi}, \quad \epsilon = \frac{kb}{n}.$$

Hence if $E(\theta, k)$ denote the incomplete elliptic function,

$$\int_0^\theta \sqrt{1 - k^2 \sin^2 \theta} d\theta. \quad (29)$$

Then

$$\phi = \iota E\left(\frac{\xi}{\iota}, \epsilon\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad (30)$$

where $\iota = \sqrt{-1}$.

Hence if $\lambda = b^2 \sinh^2 \xi$, a set of free periods between the surfaces $(\lambda_1 \lambda_2)$ is given by

$$\tan \iota n \left\{ E\left(\frac{\xi_1}{\iota}, \frac{kb}{n}\right) - E\left(\frac{\xi_2}{\iota}, \frac{kb}{n}\right) \right\} = \frac{\epsilon^2}{4n} \left[\frac{\sinh 2\xi}{(1 + \epsilon^2 \sinh^2 \xi)^{\frac{3}{2}}} \right]_1^2, \quad (31)$$

provided n be very great.

We may here recall the fact that b^2 is the difference of the squared semiaxes of any confocal, and λ is the minor semi-axis. Treating the equation for M in a similar manner,

$$M = \frac{C \cosh n\phi_1 + D \sinh n\phi_1}{\sqrt{\phi_1'}}, \quad . \quad . \quad . \quad . \quad (32)$$

where

$$\phi_1 = \int_0^\eta \sqrt{1 - \epsilon^2 \sin^2 \eta} d\eta, \quad . \quad . \quad . \quad . \quad (33)$$

which is also readily expressible as an elliptic function $E\left(\eta, \frac{kb}{n}\right)$.

The magnetic force is then

$$C = \frac{A}{\sqrt{\phi_1'}}, \cos(n\phi + \epsilon) \cosh(n\phi_1 + \epsilon_1) \cos(kVt + \epsilon_2). \quad (34)$$

And the electric force may be obtained by the proper operations.

For vibrations inside a single elliptic cylinder, we must have

$$L = B \frac{\sin n\phi}{\sqrt{\phi_1'}}, \quad . \quad . \quad . \quad . \quad (35)$$

in order that the forces may be everywhere finite.

The period equation for the cylinder defined by ξ , for large values of n , is accordingly

$$\frac{d}{d\xi} \left\{ \frac{\sinh E\left(\frac{\xi}{\iota}\right)}{(1 + \epsilon^2 \sinh^2 \xi)^{\frac{1}{4}}} \right\} = 0, \quad . \quad . \quad . \quad . \quad (36)$$

where the function E has a modulus $\epsilon = \frac{kb}{n}$.

Detailed Investigation of very Short Waves in the Space.

In the case of short waves, k is great.

We first take the case when n is an integer too small to be comparable with k . The dependence of θ on k ceases when k is very great, by analogy with the corresponding circular problem. Moreover, the result is periodic in η , of period 2π .

Then the equation for L is

$$\frac{d^2 L}{d\xi^2} + L \cdot k^2 b^2 \sinh^2 \xi = 0; \quad . \quad . \quad . \quad (37)$$

and by the previous method, the asymptotic expansion for large values of k is found to be

$$L = \sqrt{\operatorname{cosech} \xi} \{ A \cos (kb \cosh \xi) + B \sin (kb \cosh \xi) \}. \quad (38)$$

Similarly,

$$M = \sqrt{\operatorname{cosec} \eta} \{ C \cos (kb \cos \eta) + D \sin (kb \cos \eta) \}. \quad (39)$$

Now $\lambda = b^2 \sinh^2 \xi$;

$$\therefore L = \frac{A \cos (k\sqrt{b^2 + \lambda}) + B \sin (k\sqrt{b^2 + \lambda})}{(\lambda)^{\frac{1}{4}}}. \quad (40)$$

The period equation therefore becomes

$$\begin{aligned} \tan k (\sqrt{b^2 + \lambda_2} - \sqrt{b^2 + \lambda_1}) &= \frac{\frac{\sqrt{b^2 + \lambda_1}}{2k\lambda_1} - \frac{\sqrt{b^2 + \lambda_2}}{2k\lambda_2}}{1 + \frac{\sqrt{b^2 + \lambda_1} \cdot \sqrt{b^2 + \lambda_2}}{4k^2\lambda_1\lambda_2}} \\ &= \frac{\{\lambda_2\sqrt{b^2 + \lambda_1} - \lambda_1\sqrt{b^2 + \lambda_2}\}}{2k\lambda_1\lambda_2} \end{aligned}$$

since k is large.

Hence $\frac{2\pi}{k}$ is a short possible wave-length, if k is a large root of

$$\tan k \{ (b^2 + \lambda_2)^{\frac{1}{2}} - (b^2 + \lambda_1)^{\frac{1}{2}} \} = \frac{\{\lambda_2(b^2 + \lambda_1)^{\frac{1}{2}} - \lambda_1(b^2 + \lambda_2)^{\frac{1}{2}}\}}{2k\lambda_1\lambda_2}. \quad (41)$$

For a very elongated section, b is large, and we obtain

$$\tan \frac{k}{2b} (\lambda_2 - \lambda_1) = \frac{b}{2k\lambda_1\lambda_2} \cdot (\lambda_2 - \lambda_1). \quad . \quad . \quad (42)$$

The larger k may be, the more accurate is the approximation to the free period.

For these very short waves, the magnetic force is very approximately

$$c = \frac{A}{(\lambda\mu)^{\frac{1}{4}}} \cdot \cos(k\sqrt{b^2 + \lambda} + \epsilon_1) \sinh(k\sqrt{b^2 + \mu} - kb) \cdot e^{ikvt} \quad (43)$$

$$\alpha = \beta = 0,$$

and is along the axis of the cylinders.

The factor involving μ has been so adjusted that it cannot become infinite at any point.

In order to test the error involved in this approximation, we must find the asymptotic expansion under the assumption that n^2 is not zero, but a small positive quantity.

Let $\frac{n}{k} = \theta$, where θ is very small. Then

$$\frac{d^2 L}{d\xi^2} + k^2(\theta^2 + b^2 \sinh^2 \xi) L = 0.$$

If, following the previous method, we write

$$L = e^{\pm ik\phi} \cdot \psi;$$

we find

$$\phi = \int_0^\xi \sqrt{\theta^2 + b^2 \sinh^2 \xi} d\xi; \quad \dots \quad (44)$$

$$\psi = \frac{C}{\sqrt{\phi}}, \text{ as before.}$$

To the second order of small quantities,

$$\phi = b \cosh \xi + \frac{\theta^2}{2b} \log \tanh \frac{\xi}{2};$$

and therefore $\frac{1}{b} \sqrt{b^2 + \lambda}$ is to be replaced by

$$\frac{1}{b} \sqrt{b^2 + \lambda} + \frac{\theta^2}{2b^2} \log \frac{\sqrt{\lambda}}{b + \sqrt{b^2 + \lambda}} \quad \dots \quad (45)$$

in the period equation.

The ratio of the increase, to the original value, is of magnitude

$$\frac{\theta^2}{b\sqrt{b^2 + \lambda}} \cdot \log \frac{\sqrt{\lambda}}{b + \sqrt{b^2 + \lambda}}.$$

Now $\theta^2 = \frac{n^2}{k^2}$, while for light waves k is about 10^3 mm.

Hence the correction is quite inappreciable, even when n is fairly large.

The components of electrical force are

$$Z=0,$$

$$\begin{aligned} X &= \int_0^t 2V^2 p_2 \frac{\partial c}{\partial \mu} dt \\ &= \frac{-iAV \cdot e^{ikVt}}{\sqrt{\lambda - \mu}} \cdot \left(\frac{\mu}{\lambda}\right)^{\frac{1}{4}} \cos(k\sqrt{b^2 + \lambda} + \epsilon_1) \left\{ \sinh(k\sqrt{b^2 + \mu} + \epsilon_2) \right. \\ &\quad \left. - \frac{\sqrt{b^2 + \mu}}{2\mu} \cosh(k\sqrt{b^2 + \lambda} + \epsilon_2) \right\} \dots \dots \dots (46) \end{aligned}$$

$$\begin{aligned} Y &= \frac{-iAV \cdot e^{ikVt}}{\sqrt{\lambda - \mu}} \cdot \left(\frac{\lambda}{\mu}\right)^{\frac{1}{4}} \left\{ \sin(k\sqrt{b^2 + \lambda} + \epsilon_1) \right. \\ &\quad \left. + \frac{\sqrt{b^2 + \lambda}}{2\lambda} \cos(k\sqrt{b^2 + \lambda} + \epsilon_1) \right\} \cosh(k\sqrt{b^2 + \mu} + \epsilon_2) \quad (47) \end{aligned}$$

where ϵ_1 is known from the condition that $Y=0$ at boundaries, and k is one of the roots of (41).

If θ is the inclination of the resultant electric force to the normal drawn to the confocal ellipse through that point,

$$\tan \theta = \frac{Y}{X} = \left(\frac{\lambda}{\mu}\right)^{\frac{1}{2}} \left\{ \frac{\tan(k\sqrt{b^2 + \lambda} + \epsilon_1) + \frac{\sqrt{b^2 + \lambda}}{2\lambda}}{\tanh(k\sqrt{b^2 + \mu} - kb) - \frac{\sqrt{b^2 + \mu}}{2\mu}} \right\}. \quad (48)$$

And therefore the differential equation to the rays is

$$\frac{d\lambda}{\lambda \sqrt{b^2 + \lambda} \cdot \tan(k\sqrt{b^2 + \lambda} + \epsilon_1) + \frac{b^2 + \lambda}{2}} = \frac{d\mu}{\mu \sqrt{b^2 + \mu} \tanh(k\sqrt{b^2 + \mu} - kb) - \frac{b^2 + \mu}{2}}, \quad (49)$$

since they are propagated perpendicularly to the electric force in the normal section, and

$$\tan \theta = \frac{p_1}{p_2} \frac{du}{d\lambda}.$$

These rays are closed curves round the axis of z , in the plane of each section.

When the quantity n becomes appreciably great in the differential equation, if the asymptotic expansion be found, it appears that the ensuing waves cease to be short. The above theory therefore applies to *very* short waves in general, provided they are possible to the enclosed space. This possibility is determined by equation (41), and exists if the root k , to which they correspond, is large. The approximation

becomes closer as the successive roots are taken in an increasing sequence.

In the particular case of short waves inside a single elliptic cylinder, the forces must be finite at $\lambda=0$. Hence in equation (43),

$$\epsilon_1 = -kb + \frac{\pi}{2},$$

and the μ term must similarly be finite.

$$\therefore c = \frac{A}{(\lambda\mu)^{\frac{1}{4}}} \cdot \sin(k\sqrt{b^2+\lambda}-kb) \sinh(k\sqrt{b^2+\mu}-kb) \cdot e^{ikvt}, \quad (50)$$

is the approximate magnetic force for waves such as those of light. The electric force may be deduced at once from this expression.

The period equation for a single cylinder defined by λ is

$$\frac{d}{d\lambda} \left\{ \frac{\sin k \left\{ (b^2 + \lambda)^{\frac{1}{2}} - b \right\}}{\lambda^{\frac{1}{4}}} \right\} = 0, \quad . \quad . \quad . \quad (51)$$

where only the large values of k are chosen, the approximation being closer as they increase.

A similar investigation for the case of long waves appears impracticable. The waves peculiar to the cylinders cannot have a length sufficiently great to ensure the utility of a continued approximation to the solution of the equation. The assumption of the existence of very long waves, when tested, is found, as was perhaps to be anticipated, to lead to impossible values. The approximate solution given above for short waves is of remarkable simplicity, and almost quite accurate in the case of light.

A certain interest attaches to the class of waves worked out in terms of elliptic functions; but their period equation is so complicated that it cannot readily lead to any numerical calculations, to which, on the contrary, the short periods readily lend themselves.

The most general type of oscillation possible in the space consists of the combination of—

(1) Waves of the doubly infinite series of possible periods travelling round the axis of z .

(2) Waves of any period whatever travelling along z .

In this general vibration, the path of a ray is a spiral curve round the axis of the cylinders, and the following results will hold:—

(1) The magnetic force never has a component normal to the confocal elliptic cylinder through any point.

(2) The electric force never has a component along the axis of the cylinders.

XXIX. *On the Conduction of Electricity through Gases between Parallel Plates.* By ALFRED A. ROBB*.

THE differential equation

$$\frac{d^2X^2}{dx^2} = 8\pi e \left(\frac{1}{R_1} + \frac{1}{R_2} \right) \left\{ q - \frac{\alpha}{e^2 X^2 (R_1 + R_2)^2} \right. \\ \left. \times \left(i + \frac{R_2}{8\pi} \frac{dX^2}{dx} \right) \left(i - \frac{R_1}{8\pi} \frac{dX^2}{dx} \right) \right\} \quad (1)$$

occurs in the theory of the conduction of electricity through gases between parallel plates.

In this equation X represents the electric intensity at a point defined by the coordinate x , measured in a direction perpendicular to the plates. i represents the current per unit area; R_1 and R_2 are the velocities of the positive and negative ions under unit electric intensity; q is the rate of ionization per unit volume and α is the coefficient of recombination.

The equation has been solved by Professor J. J. Thomson for the case when $R_1 = R_2$; but in general it does not lend itself to solution. It is proposed here to show (i.) that this equation may readily be transformed so that it becomes a characteristic one for any gas under definite conditions of temperature and pressure; and (ii.) that for any gas for which R_1 and R_2 are unequal there exist two pressures at which the equation becomes soluble.

In order to effect the first of these objects, we have only to write:

$$\left. \begin{aligned} X^2 &= \frac{i^2}{q} y^2, \\ x &= \frac{i}{q} v; \end{aligned} \right\} \dots \dots \dots (2)$$

and the equation becomes

$$\frac{d^2y^2}{dv^2} = 8\pi e \left(\frac{1}{R_1} + \frac{1}{R_2} \right) \left\{ 1 - \frac{\alpha}{e^2 y^2 (R_1 + R_2)^2} \right. \\ \left. \times \left(1 + \frac{R_2}{8\pi} \frac{dy^2}{dv} \right) \left(1 - \frac{R_1}{8\pi} \frac{dy^2}{dv} \right) \right\}. \quad (3)$$

The form of this equation does not depend either on the strength of the current or the intensity of the ionization, and therefore is characteristic for the gas.

In order to show that for certain pressures this equation

* Communicated by the Author.

takes a soluble form, we shall write it

$$\frac{d^2y^2}{dv^2} = a + \frac{b}{y^2} + \frac{c}{y^2} \frac{dy^2}{dv} + \frac{\epsilon}{2y^2} \left(\frac{dy^2}{dv} \right)^2, \quad . \quad . \quad (4)$$

where

$$a = \frac{8\pi e(R_1 + R_2)}{R_1 R_2},$$

$$b = -\frac{8\pi\alpha}{eR_1 R_2 (R_1 + R_2)},$$

$$c = -\frac{\alpha(R_2 - R_1)}{eR_1 R_2 (R_1 + R_2)},$$

$$\epsilon = \frac{\alpha}{4\pi e(R_1 + R_2)}.$$

It is to be observed here that ϵ is the same quantity as that introduced by Langevin (*Recherches sur les Gaz ionisés*, University of Paris, 1902), and denoted by the same letter.

It follows from the physical meaning which he gives it that ϵ must be a positive fraction.

In order to reduce equation (4) we make the following series of substitutions: 1st. Putting $\frac{dy^2}{dv} = -\frac{2}{\epsilon} \frac{w}{z}$ and $y^2 = \frac{1}{z}$, we get

$$w \frac{dw}{dz} - cw + \frac{b\epsilon}{2} z = -\frac{a\epsilon}{2} z^{1-\frac{2}{\epsilon}}. \quad . \quad . \quad . \quad (5)$$

2nd. Putting $w = \frac{s}{K} \frac{dz}{ds}$, where K is a constant to be afterwards determined, we get

$$s^2 \frac{d^2z}{ds^2} + (1 - cK)s \frac{dz}{ds} + \frac{b\epsilon}{2} K^2 z = -\frac{a\epsilon}{2} K^2 z^{1-\frac{2}{\epsilon}}. \quad . \quad . \quad (6)$$

3rd. Putting $\theta = zs^n$, we have

$$\frac{1}{s^{n-2}} \frac{d^2\theta}{ds^2} = s^2 \frac{d^2z}{ds^2} + 2ns \frac{dz}{ds} + n(n-1)z.$$

If, therefore, K and n be determined by the equations

$$1 - cK = 2n,$$

and

$$\frac{b\epsilon}{2} K^2 = n(n-1);$$

we have n given by the equation

$$\frac{b\epsilon}{2c^2}(1-2n)^2 = n(n-1); \quad . \quad . \quad . \quad (7)$$

and the differential equation becomes

$$\frac{d^2\theta}{ds^2} = -\frac{a}{b}n(n-1)\theta^{1-\frac{2}{\epsilon}}s^{\frac{2n}{\epsilon}-2}. \quad . \quad . \quad . \quad (8)$$

Now equation (7) has two roots. If n_1 and n_2 be these two values, then $n_1+n_2=1$; and so unless $n_1=n_2=\frac{1}{2}$ there will be two equations of form (8) corresponding to any one of form (4). Regarded merely as a differential equation, (8) may be solved in several cases:—

- (i.) When $a=0$.
- (ii.) When $\epsilon=2$.
- (iii.) When $n=\frac{1}{2}$.
- (iv.) When $n=\epsilon$ or $1-\epsilon$.

As regards case (i.), our problem does not allow of a being zero.

Further, ϵ is necessarily a positive fraction, and so case (ii.) is useless for our purpose.

Case (iii.) corresponds to $c=0$, or in other words to $R_1=R_2$, and the equation of conduction under these circumstances has already been solved by Professor J. J. Thomson ('Conduction of Electricity through Gases,' p. 66, or Phil. Mag. vol. xlvii. p. 253, 1899).

Case (iv.) requires

$$\frac{b\epsilon}{2c^2} = \frac{\epsilon(\epsilon-1)}{(1-2\epsilon)^2} \quad \text{or} \quad b = \frac{2(\epsilon-1)}{(1-2\epsilon)^2} c^2. \quad . \quad . \quad (9)$$

But

$$\frac{b\epsilon}{2c^2} = -\frac{R_1 R_2}{(R_2 - R_1)^2}.$$

Thus we get either

$$\left. \begin{aligned} \epsilon &= \frac{R_1}{R_1 + R_2} \\ \text{or} \\ \epsilon &= \frac{R_2}{R_1 + R_2} \end{aligned} \right\} \quad . \quad . \quad . \quad (10)$$

Now both these values of ϵ are positive fractions, and therefore are possible. For air $\frac{R_2}{R_1} = 1.22$ approximately,

and thus the values of $\frac{R_1}{R_1 + R_2}$ and $\frac{R_2}{R_1 + R_2}$ are respectively about .45 and .55.

Now Langevin gives $\epsilon = .27$ at 760 mm. pressure and .62 at 1550 mm. for the case of air.

Thus $\epsilon = .45$ corresponds to about 1.53 atmospheres, and $\epsilon = .55$ to about 1.83 atmospheres.

Accordingly, for air at these two pressures the equation has the desired form, and may be solved.

Taking, then, for simplicity $n = \epsilon$ ($n = 1 - \epsilon$ gives of course the same final result), we have

$$\frac{d^2\theta}{ds^2} = -\frac{a}{b}\epsilon(\epsilon-1)\theta^{1-\frac{2}{\epsilon}}.$$

This gives

$$\int \frac{d\theta}{\sqrt{C_1 - \frac{a\epsilon^2}{b}\theta^{2-\frac{2}{\epsilon}}}} + C_2 = s,$$

where C_1 and C_2 are constants of integration.

By reversing the various steps indicated, we may finally obtain y and v in terms of θ as an auxiliary variable; but it is desirable to introduce a slight change of notation in order that the final result may not contain a third and superfluous constant.

We accordingly write

$$\phi \left(\frac{bC_1}{a\epsilon^2} \right)^{\frac{\epsilon}{2\epsilon-2}} = \theta,$$

and

$$\frac{C_2 \sqrt{-C_1}}{\left(\frac{bC_1}{a\epsilon^2} \right)^{\frac{\epsilon}{2\epsilon-2}}} = k_1,$$

a transformation which is only possible if C_1 be different from zero, and finally obtain y and v in terms of the auxiliary variable ϕ . Thus

$$\left. \begin{aligned} y &= \frac{-c}{\epsilon(1-2\epsilon)} \sqrt{\frac{2(1-\epsilon)}{a}} \frac{\left\{ \int \frac{d\phi}{\sqrt{\phi^{2-\frac{2}{\epsilon}} - 1}} + k_1 \right\}}{\phi^{\frac{1}{\epsilon}}} \\ v &= \frac{2c(1-\epsilon)}{a\epsilon^2(1-2\epsilon)} \left\{ \int \frac{\left\{ \int \frac{d\phi}{\sqrt{\phi^{2-\frac{2}{\epsilon}} - 1}} + k_1 \right\} d\phi}{\phi^{\frac{2}{\epsilon}} \sqrt{\phi^{2-\frac{2}{\epsilon}} - 1}} + \text{const.} \right\} \end{aligned} \right\} \quad (11)$$

As may readily be proved by differentiation.

Besides this solution, there is another corresponding to the case where C_1 is zero, and when therefore the transformation from θ to ϕ fails.

In this case the relation between y and v can be expressed directly without the use of an auxiliary variable, and may readily be shown to be

$$\pm 2\sqrt{\frac{1-\epsilon}{2a}} y - \frac{2c}{a} \frac{1-\epsilon}{1-2\epsilon} \log \left\{ \frac{2c}{1-2\epsilon} \pm \sqrt{\frac{2a}{1-\epsilon}} y \right\} = v + \text{const.} \quad (12)$$

The solution (11) may be somewhat simplified in form, for if we put

$$\phi^{1-\frac{1}{\epsilon}} = \cosh \omega,$$

and substitute the values of c and a , we get

$$\left. \begin{aligned} y &= \frac{4\pi}{\sqrt{\alpha}} \left\{ \frac{-\epsilon}{1-\epsilon} \int \frac{d\omega}{\cosh^{1-\epsilon} \omega} + k_1 \right\} \cosh^{\frac{1}{1-\epsilon}} \omega \\ v &= \frac{R_2 - R_1}{\sqrt{\alpha}} \frac{\epsilon}{1-2\epsilon} \left\{ \int y d\omega + k_2 \right\}, \end{aligned} \right\} \quad (13)$$

where $\sqrt{\alpha}$ is to be employed with the same sign in both places and where

$$\epsilon = \frac{R_1}{R_1 + R_2},$$

or

$$\epsilon = \frac{R_2}{R_1 + R_2},$$

according as the pressure is such as to make

$$\alpha = 4\pi e R_1,$$

or

$$\alpha = 4\pi e R_2.$$

The integrals in (13) may be evaluated in finite form when $\frac{1}{1-\epsilon}$ is an integer, and when therefore the velocity of one ion is an exact multiple of the velocity of the other. In other cases they may be easily calculated*.

The other solution (12) may be put in the form

$$\begin{aligned} 4\pi\epsilon^2 \frac{1-\epsilon}{1-2\epsilon} \frac{R_2 - R_1}{\alpha} \log \left\{ -\frac{8\pi\epsilon}{1-2\epsilon} \left(\frac{1}{R_1} - \frac{1}{R_2} \right) \pm 2\sqrt{\frac{\alpha}{\epsilon(1-\epsilon)R_1R_2}} y \right\} \\ \pm 2\sqrt{\frac{\epsilon(1-\epsilon)R_1R_2}{\alpha}} y = v + \text{const.} \quad (14) \end{aligned}$$

* The author has calculated a table of these integrals for $\epsilon = \frac{4}{q}$, which is approximately one of the values for the case of air. This he hopes to give in a supplementary paper at an early date.

in which, as before, ϵ has the values $\frac{R_1}{R_1 + R_2}$ and $\frac{R_2}{R_1 + R_2}$, corresponding respectively to the pressures at which

$$\alpha = 4\pi e R_1 \text{ and } \alpha = 4\pi e R_2.$$

Note.—The idea of adjusting the pressure so as to render the equation of conduction of electricity between parallel plates more manageable occurred independently to Mr. G. W. Walker and myself. It was used by Mr. Walker to complete the integration in the case when $R_1 = R_2$. It was shown by him (*Phil. Mag.* Nov. 1904) that this may be done if $\frac{\alpha}{8\pi e R} = 2, \frac{1}{2}, \frac{1}{3}, \frac{1}{4}$, and he selected 2 as the simplest.

I pointed out to him in a letter that according to Langevin's theory, $\epsilon = \frac{\alpha}{8\pi e R}$ must be a fraction, and that therefore the value 2 was physically impossible.

He replies that he had not observed the limitation imposed by Langevin's theory, and suggests that I should lay emphasis on the point that while the value $\frac{\alpha}{8\pi e R} = 2$ is possible if α be constant and R vary inversely as the pressure according to McClung's and Rutherford's results, yet according to Langevin $\frac{\alpha}{8\pi e R}$ can never be greater than unity.

There is thus a serious difference to be explained.

XXX. *The Rate of Recombination of Ions in Gases.*

By O. W. RICHARDSON*.

THE following is an attempt to explain the variation of the rate of recombination of ions formed in gases with the pressure of the gas. The basis of the method is a theorem due to Langevin †, which may be stated as follows:—If we describe around each negative ion a sphere whose radius is arbitrary, except that it must be large compared with the mean free path of an ion and small compared with the average distance between two ions—both of which conditions are capable of being satisfied in the case of ionization produced by Röntgen rays and radium radiation, in gases at moderate pressures—then the number of positive ions which

* Communicated by the Author.

† *Thèses de l'Université de Paris*, A. 431, p. 103 (Gauthier Villars).

will enter such a sphere in time dt is independent of the radius of the sphere and of the external field, and is given by

$$4\pi(k_1 + k_2)P e dt ;$$

where k_1, k_2 are the velocities of the two ions under unit field, P is the number of positive ions per c.c., and e the charge on an ion.

If we call each case of an ion crossing one of these surfaces a "collision," and suppose that each collision is equivalent to recombination, we get for the law of recombination

$$\frac{dp}{dt} = \frac{dn}{dt} = -4\pi(k_1 + k_2)pn,$$

where p, n are the densities of positive and negative electrification respectively. Since recombination does not necessarily result every time an ion comes within the sphere of action of another ion, the above expression only gives an upper limit to its amount. Langevin puts

$$\frac{dp}{dt} = \frac{dn}{dt} = -4\pi(k_1 + k_2)\epsilon pn,$$

where ϵ is a function of the pressure whose possible values range from zero to unity. The coefficient of recombination α is thus equal to $4\pi(k_1 + k_2)\epsilon$.

We shall now proceed to calculate a value of ϵ . To do this it will be necessary to consider what happens to an ion when it enters the region in the immediate neighbourhood of an ion of opposite sign where the electric force is very intense. If we describe round each negative ion a concentric sphere whose radius is 4×10^{-6} cms., then by the time that a positive ion reaches one of these spheres the work done on it by the attracting forces will be about one two-hundredth of the total work done during recombination. It is evident that inside this region the ion will be greatly accelerated and its behaviour will be quite different from what it was outside. To fix our ideas, we may for the moment regard the negative ion as fixed and the positive as projected through the spherical boundary. The positive ion will then describe an orbit round the negative and in general will return to the boundary at some other point. If this is the case it will have returned to the region where the kinetic energy, which it gains in virtue of the work done by the attractive forces, during its passage through a distance equal to the mean free path, is not great compared with the mean heat energy; in fact it

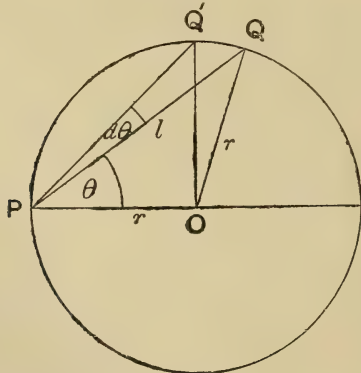
will again behave as a free ion. The criterion for recombination then is the condition that the ion should remain *permanently* within the region in question.

The condition that the positive ion should return to the boundary is a well-known result in the theory of attractions, and is that $\frac{1}{2}mV^2 + 2\Psi$ is positive: where V is the relative velocity of the ions, m is the mass of one of them, and Ψ is their potential energy reckoned from the surface of the sphere.

Since the number of cases in which the positive ion can enter the sphere without a finite velocity component along the radius is infinitesimal, it is evident that at the commencement of the path $\frac{1}{2}mV^2 + 2\Psi$ is necessarily positive; so that unless something happens which reduces the value of V recombination will not take place. We shall suppose that the necessary decrease in V is caused by collision with uncharged molecules, and try to find out how many collisions are required to produce the observed experimental results.

We shall first calculate the probability that an ion projected from the circumference of a sphere of radius r makes a collision within the sphere. To make the calculation possible, we shall make the rather approximate assumption that the path of the ion inside the sphere is straight. It is evident that this assumption cannot change the form of the function representing the probability: it can only modify the values of the constants which enter into it.

Fig. 1.



Of any number of ions starting from a given point P (see fig. 1) the fraction which get over a path of length l without collision is $e^{-l/\lambda}$, where λ = the mean free path of an ion in the gas; so that the fraction which collide in a path of length l is $1 - e^{-l/\lambda}$. Now the length of the chord $l = 2r \cos \theta$, and the ions for which this represents the

maximum possible path inside the sphere are those contained in the solid angle between two circular cones whose generators are inclined at θ and $\theta + d\theta$ to one another. Hence if P ions start from P in directions uniformly distributed throughout the hemispherical angle 2π , the number which lie between the infinitesimally near cones is

$$P \sin \theta d\theta ;$$

hence the number which collide within the sphere is

$$\begin{aligned} & P \int_0^{\pi/2} \left(1 - e^{-\frac{2r \cos \theta}{\lambda}} \right) \sin \theta d\theta \\ &= P \left\{ 1 + \frac{\lambda}{2r} \left(e^{-\frac{2r}{\lambda}} - 1 \right) \right\}. \end{aligned}$$

On the hypothesis that one collision is sufficient to stop the ion ever getting out of the field of force of the other ion, we have now calculated a value of ϵ ; for ϵ is the fraction of the number of positive ions, projected into the small sphere surrounding the negative ion, which never succeed in escaping. It is therefore equal to

$$1 + \frac{\lambda}{2r} \left(e^{-\frac{2r}{\lambda}} - 1 \right).$$

A brief examination of this function is sufficient to show that it is incapable of representing the values of ϵ found experimentally*.

If we put $\frac{r}{\lambda} = x$, then x is proportional to the pressure, and our function may be written

$$f = 1 + \frac{1}{2x} (e^{-2x} - 1).$$

It approaches unity asymptotically as x becomes infinite, and takes the value zero when $x=0$. This, however, is not sufficient to satisfy the experimental curves which, as Langevin† has shown, vary as the square of the pressure at low pressures. Expanding f in powers of x in the neighbourhood of the origin, we find the first term $=x$, which obviously does not satisfy the required conditions.

We shall now examine what results are obtained on the hypothesis that two is the minimum number of collisions within the prescribed region which are necessary for recombination. We have to calculate the probability that an ion

* Langevin, *Thèse*, p. 150.

† *Comptes Rendus*, vol cxxxvii. p. 177 (1903).

makes two consecutive collisions within the region considered. The general problem, in fact, is to find the chance that an ion, after any one of the above primary collisions, makes a second collision before leaving the spherical enclosure, and to integrate the value of this over all the primary collisions which occur. I have not been able to obtain the exact solution of the problem as thus stated; but it is very easy to obtain an upper limit to the required number, which can be shown to be very near the true value.

This may be done by supposing the ions which have undergone a primary collision at any point within the sphere to start on their next path from the centre of the sphere. The error introduced by this supposition is greatest in the case of an ion which has collided at the surface of the sphere and when λ is small. In the most unfavourable case, the probability would be increased in the ratio of two to one by supposing the ion displaced to the centre. But the number of cases near the boundary is necessarily small compared with the total number, so that the average error will be small compared with the maximum error. Thus we see that the upper limit obtained in this will not be far from the true value.

The probability that an ion, starting from the centre of a sphere of radius r , will collide before it reaches the circumference is readily seen to be $1 - e^{-r/\lambda}$; so that, out of P ions projected into the prescribed sphere, we obtain as an upper limit to the number which make two collisions before reaching the circumference the value

$$P \left(1 + \frac{\lambda}{2r} \left(e^{-\frac{2r}{\lambda}} - 1 \right) \right) \left(1 - e^{-\frac{r}{\lambda}} \right).$$

If two collisions within the prescribed sphere are necessary for recombination to take place, we find that

$$\begin{aligned} \epsilon &= \left\{ 1 + \frac{\lambda}{2r} \left(e^{-\frac{2r}{\lambda}} - 1 \right) \right\} \left(1 - e^{-\frac{r}{\lambda}} \right), \\ &= \phi(x) \text{ say, where } x = r/\lambda. \end{aligned}$$

Evidently ϕ approaches unity asymptotically as x becomes infinite; also on expanding it in powers of x in the neighbourhood of $x=0$ we see that the first term in the expansion is x^2 , so that $\phi=0$ when $x=0$ and ϕ varies as x^2 for small values of x . Thus the function we have found gives a qualitative explanation of the values of ϵ found by experiment.

By carrying the argument a step further we see that an upper limit to the number of ions which make three collisions

inside the sphere is given by

$$\left\{ 1 + \frac{\lambda}{2r} \left(e^{-\frac{2r}{\lambda}} - 1 \right) \right\} \left(1 - e^{-\frac{r}{\lambda}} \right)^2,$$

and generally,

$$\left\{ 1 + \frac{\lambda}{2r} \left(e^{-\frac{2r}{\lambda}} - 1 \right) \right\} \left(1 - e^{-\frac{r}{\lambda}} \right)^{n-1}$$

is an upper limit for the number which make n collisions. For high values of n , however, the above product will cease to represent, even approximately, the true value of the number required, since the error is proportional to the number of terms in the product.

We should expect from the nature of the preceding argument, that out of those molecules which make a sufficiently large number, say n , of collisions within the sphere practically the whole would result in recombination. Of those which made $n-1$, but not n , collisions a certain definite fraction would become fixed, and of those which made $n-2$, but not $n-1$, a smaller fraction, and so on. In fact, if c_n be written for

$$\left(1 + \frac{\lambda}{2r} \left(e^{-\frac{2r}{\lambda}} - 1 \right) \right) \left(1 - e^{-\frac{r}{\lambda}} \right)^{n-1},$$

the number of ions which make n collisions within the sphere, n being sufficiently large, we should expect ϵ to be of the form

$$\epsilon = c_n + a_{n-1}(c_{n-1} - c_n) + a_{n-2}(c_{n-2} - c_{n-1}) + \dots a_1(c_1 - c_2),$$

where $a_{n-1} \dots a_1$ are proper fractions which gradually become smaller as the suffix decreases.

We are now in a position to test the theory by the experimental results. This may be done by referring to fig. 2. The ordinates of the various curves plotted in this diagram represent the following functions:—

$$A = 1 + \frac{e^{-2x} - 1}{2x} = c_1,$$

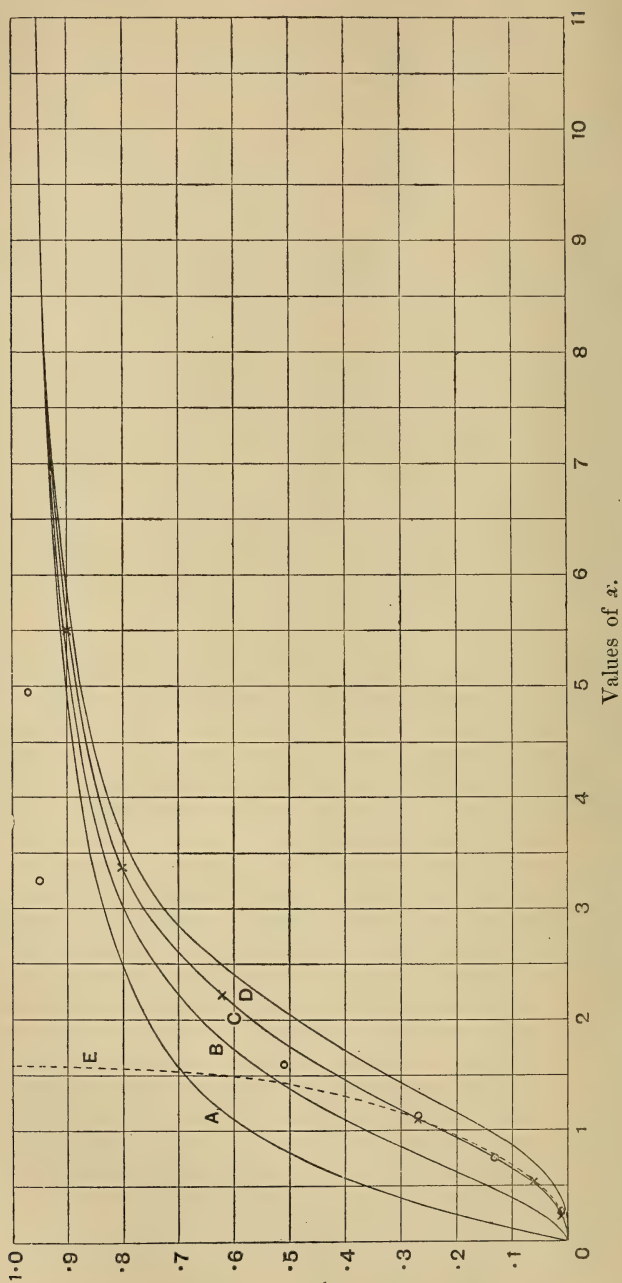
$$B = \left(1 + \frac{e^{-2x} - 1}{2x} \right) (1 - e^{-x}) = c_2,$$

$$C = \left(1 + \frac{e^{-2x} - 1}{2x} \right) (1 - e^{-x})^2 = c_3,$$

$$D = \left(1 + \frac{e^{-2x} - 1}{2x} \right) (1 - e^{-x})^3 = c_4,$$

$$E = 1 - (1 - x^2)^{\frac{1}{2}}.$$

Fig. 2.



The reason for the insertion of the last curve will be given later.

The other points in the diagram are Langevin's experimentally found values of ϵ . Those for air are shown thus : $\times$, and those for carbon-dioxide, thus : $\circ$. In order to compare the results, the scale of the abscissæ has been chosen so as to make the observed values coincide as nearly as possible with the curve C ($=c_3$). The pressures corresponding to any point are therefore different for the two gases; for the air points, $\times$, they are obtained from the abscissæ by the relation $p(\text{mms.})=691 x$, for the carbon-dioxide points, $\circ$, they are given by $p(\text{mms.})=438 x$.

In the case of air the experimental points all appear to be sufficiently close to the curve C or $y=c_3$, but a slight deviation from the curve can be detected at the two lowest pressures. The agreement with the curve

$$y=c_4+\cdot75(c_3-c_4)+\cdot17(c_2-c_3)$$

seems to be exact, or at any rate within the experimental error, as the following numbers testify :—

p mms.	Values of ϵ .	
	Experimental.	Calculated.
152	$\cdot01$	$\cdot009$
375	$\cdot06$	$\cdot072$
760	$\cdot27$	$\cdot27$
1550	$\cdot62$	$\cdot62$
2320	$\cdot80$	$\cdot80$
3800	$\cdot90$	$\cdot90$

The values for carbon-dioxide do not seem to be capable of being represented by the formula with the same degree of accuracy as those for air. If the scale of x is adjusted so that observations at low pressures come nearly on to the curve C, then the values at high pressures deviate considerably, the last but one being as much as 20 per cent. too high. It looks as though the experimental values were too large—there would be a tendency this way in the case of a strongly absorbing gas-like carbon-dioxide at high pressures, when the ionization would be large—although it is possible that there is some inherent complication in this case, of which the theory does not take account.

A different view of the nature of ϵ , which explains its variation at low pressures, has been given by Langevin*. On that view the criterion for recombination is that the two ions should undergo a direct collision, considered quite apart from their previous history. If we imagine a sphere described round each negative ion, the radius δ being some quantity of the order of, and proportional to, the mean free path; then the probability of any positive ion projected into the sphere striking the opposite ion at the centre is the chance that its originally straight trajectory would pass through a sphere about the negative ion of radius

$$s' = s \left\{ 1 + \frac{4e^2}{mv^2s} \right\} \dagger,$$

the mass of the two ions being supposed equal. In this formula e is the charge on an ion, m its mass, v the original relative velocity, and s the radius of the sphere of action of the two ions. It can easily be shown that the probability of an ion projected from the circumference of a sphere of radius δ striking the opposite ion is

$$= 1 - \left(1 - \left(\frac{s'}{\delta} \right)^2 \right)^{\frac{1}{2}},$$

so that Langevin's theory leads to the formula

$$\epsilon = 1 - \left(1 - \left(\frac{s'}{\delta} \right)^2 \right)^{\frac{1}{2}}.$$

In this formula δ varies inversely as the pressure and s' is independent of it, so that ϵ is of the form $1 - (1 - ap^2)^{\frac{1}{2}}$. It evidently varies as the square of the pressure for small values of the pressure.

The function $1 - (1 - \beta^2 x^2)^{\frac{1}{2}}$ is the dotted curve E in fig. 2, where the abscissæ have been chosen so as to make the curve fit the three lowest experimental values. The agreement is good enough at the three lowest pressures, but at higher pressures, as is readily deduced from an examination of the function, its rate of increase with x goes up continually as x increases, instead of falling off as the experiments require. For values of $x > 1/\beta$ the function is imaginary. For these reasons this view does not appear to afford a satisfactory explanation of the phenomena.

Returning to the exponential expression for ϵ , we shall now deduce a value of r , the radius of the spheres within which collisions have to occur, to determine recombination.

Since the charge on an ion appears to be the same for all gases, and r is determined by the field of force, presumably

* *Comptes Rendus*, vol. cxxxvii. p. 177 (1903).

† Richardson, Camb. Phil. Proc. vol. xii. p. 144 (1902).

r is independent of the gas. Also since $x=r/\lambda$, all we have to do is to find the value of λ corresponding to a particular value of x for any one gas. This may be done by means of the formula for the velocity of an ion under unit field, which is given by the kinetic theory of gases, viz., $v=\frac{1}{2}\frac{e}{m}\frac{\lambda}{c}$, where c is the mean velocity of agitation of the molecule. The author* has recently shown that the value of the velocity of the ions indicates that they have a definite complex structure, which is a function of the pressure and probably also of the temperature.

In the paper referred to it is shown that at pressures greater than atmospheric the negative ions are practically all complex, and probably consist of three gas molecules cemented together by a corpuscle. Hence the mass of a negative ion will be equal to three times the mass of an average molecule in air. The formula for λ may be written

$$\lambda = \sqrt{\frac{1}{2}mc^2} \sqrt{2m} \frac{2v}{e}.$$

Substituting the following values:

$$\frac{1}{2}mc^2 = 3.6 \times 10^{-14}$$

$$m = 101 \times 10^{-24}$$

$$v = 505 \quad \text{and} \quad e = 3.1 \times 10^{-10},$$

we find λ at atmospheric pressure $= 8.8 \times 10^{-6}$ cm. This value for λ seems to be obviously too high, being midway between the values for oxygen and carbon-dioxide. In the absence of any other evidence bearing on the question, we shall take the true value to be somewhere about 4×10^{-6} cms. at atmospheric pressure. The value of x at atmospheric pressure we find from the curves to be 1.1. This gives for the radius of the prescribed spheres $r = 4.4 \times 10^{-6}$. The work done on the ion by the electric forces before it reaches the boundary of the sphere is seen to be $= 2.2 \times 10^{-14}$ ergs, and the kinetic energy thus set free would be equivalent to raising the temperature of the ion to about 200° C. The work done inside the sphere on an ion which recombines is about two hundred times that done outside.

If we may assume that the value of r is independent of the gas, the ratio of the pressures corresponding to any given value of ϵ for two different gases should be the ratio of their mean free paths. Assuming that the values for carbon-dioxide at low pressures are the ones which the curve ought to fit, this gives for the ratio of the mean free path of an ion in CO_2 to that of an ion in air $\lambda'/\lambda = .635$.

* Phil. Mag. [6] vol. x. p. 177 (1905).

According to the formula $v = \frac{1}{2} \frac{e \lambda}{m c}$ the ratio of the velocities of the ions under unit fields for two different gases should be $= \frac{\lambda'}{\lambda} \left(\frac{m}{m'} \right)^{\frac{1}{2}}$, where m , m' are the masses of the corresponding ions. In the case of air and carbon-dioxide the mean of the values of the ratio found by Zeleny for the positive and negative ions in the dry gases $= .497$. This gives for the ratio of the masses of the ions $\frac{m'}{m} = 1.54$. The ratio of the densities of carbon-dioxide and air is 1.53, a result which indicates that an ion in carbon-dioxide at high pressures contains the same number of molecules as an ion in air. Since it has been shown that a negative ion in air at high pressures probably consists of three molecules held together by a corpuscle, presumably the same is the case for a negative ion in carbon-dioxide.

Objection may be made to the present theory on the ground that almost any function might be capable of being represented by a formula such as that given on p. 247 by taking a sufficient number of terms, each involving one of the undetermined constants a_{n-1} , &c. This is not, however, a valid objection. In the first place, the constants a are not arbitrary; they are always positive and less than unity, which they rapidly approach as n increases. In the second place, reference to fig. 2 shows that in the case of air a single term of the series, viz. c_3 , is sufficient to represent all the points but one within the limits of experimental error, and even that is not very far out. So that this very complex curve can be represented with almost the desired amount of accuracy by a formula containing only one arbitrary constant, which is equal to the ratio between the radius of one of the prescribed spheres and the mean free path of an ion when the pressure equals unity. In the case of carbon-dioxide the points which fall off the curve cannot be brought on to it by manipulating the constants without making the theory lose all its physical significance. The constants a ought to be capable of being calculated by making some hypothesis about the effect of a collision. At present the experimental results are not sufficiently accurate to enable us to more than guess at their values.

It will be noticed that roughly speaking the present theory makes $\epsilon = f(p)$ for different gases depend solely on one parameter λ , the value of the mean free path of an ion in the gas in question. The smaller the value of λ the more the curve $\epsilon = f(p)$ is displaced towards the axis of ϵ .

The principal results which have been obtained in this paper may be summarized as follows:—

(1) The hypothesis that recombination only takes place after the kinetic energy of the ions has been reduced by collision with neutral molecules, when the ions are very near each other, affords a satisfactory explanation of the variation of the rate of recombination with the gas-pressure.

(2) The results indicate that practically every ion which makes a collision with four molecules becomes fixed; that most of those which make only three, and about one-sixth of those which make only two, collisions, recombine. The terms with fewer collisions become more important at low pressures.

(3) The quantity obtained by dividing the coefficient of recombination by the sum of the velocities of the two kinds of ions, when expressed as a function of the pressure, is found to depend, in the cases of different gases, on a single parameter, which is the mean free path of an ion in the gas in question at some definite pressure.

(4) An ion in carbon-dioxide at high pressures probably contains the same number of gas molecules as an ion in air.

XXXI. A Portable Aëro-mercurial Tide-Gauge.

By K. HONDA, *Rigakuhakushi* *.

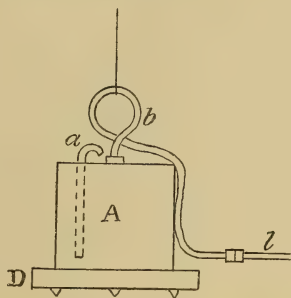
[Plates III. & IV.]

THE novel form of tide-gauge, which is a modification of Richard's hydrometer, or of W. Seibt's tide-gauge †, consists of a diving jar, a narrow lead (or copper) tubing and a recording apparatus.

Fig. 1 shows the diving jar. A is a closed cylindrical vessel made of brass, 12 cm. high and 12 cm. in diameter; it is screwed into a heavy lead disk D. By the tube *a*, water enters the vessel and compresses the enclosed air. The vessel A communicates with the recording apparatus by means of a brass tube *b* and a long lead tubing *l* (internal diam. = 3 mm. in my case); the brass tube is bent in a form as shown in the figure for convenience of transportation.

A is a closed cylindrical

Fig. 1.

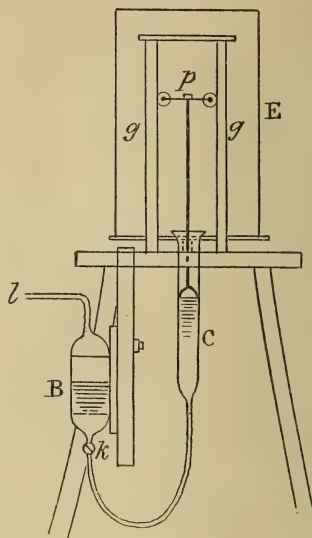


* Communicated by the Author.

† Seibt, *Instrumentenkunde*, xvii. p. 81, 1897.

Fig. 2 shows the recording apparatus. The lead tubing l is tightly connected to a glass vessel B (3.5 cm. in diam.), which is again connected, by means of a thick caoutchouc tube, to a glass tube C (1.8 cm. in diam.). The two arms of the U-shaped system are partially filled with mercury, and the pressure of the enclosed air is balanced by the pressure due to the difference in the heights of the two mercury columns. The change of pressure caused by the change of the water-level above the diving jar, appears then as the motion of the mercury column in the tube C.

Fig. 2.



To record the motion of the meniscus, the following arrangement* is used. A float made of a hollow ebonite box fits loosely into the open arm of the tube. Upon this, a thin aluminium rod is vertically erected. A pen-holder p , which carries two arms perpendicular to the pen, is fixed horizontally on the upper end of the rod. At the end of the arms, friction-wheels are attached which roll in V-shaped grooves of two vertical guides gg . In this way, the pen is constrained to move smoothly in a vertical line. Though the pen is always pressed against the recording cylinder E by a weak spring, the friction is quite insensible. The recording cylinder (20 cm. high and 9.4 cm. in diam.), which contains the clock-work inside it, is vertically placed just behind the two vertical guides and revolves once per day about its fixed axis. The zigzags of the curves recorded on the cylinder, which are due to the surface waves of short periods, may conveniently be eliminated to a desired degree by turning the cock k through a suitable angle.

Fig. 3 is the photograph of the apparatus ready for setting; the reduction is one-sixth of the natural size.

The relation between the change of water-level above the

* K. Honda, Y. Yoshida, and T. Terada, Reports of the Tōkyō Physico-Mathematical Society, vol. ii. no. 16, p. 222; *Phys. Zeitschrift*, no. 4, p. 115, 1905.

Fig. 3.

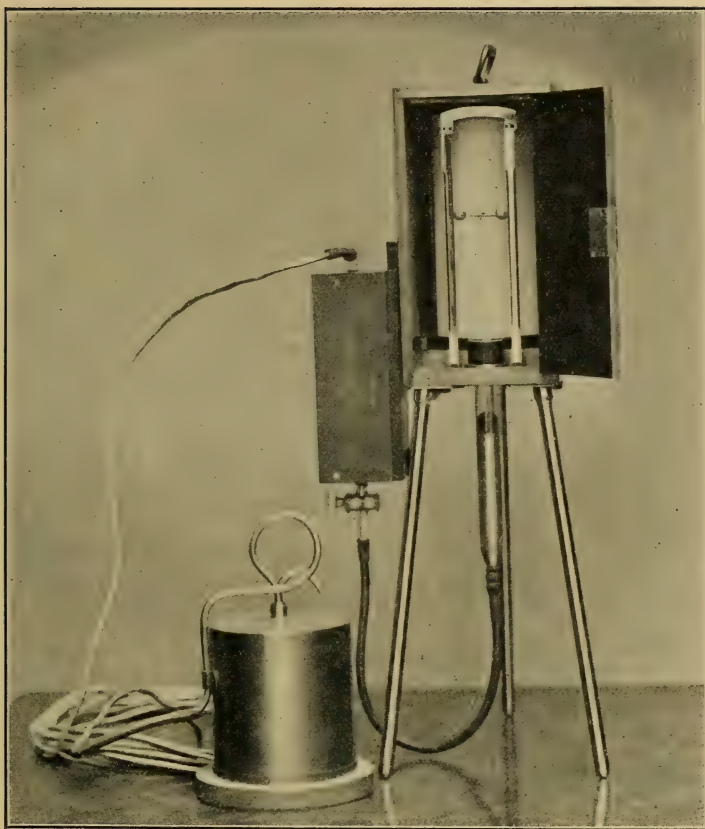
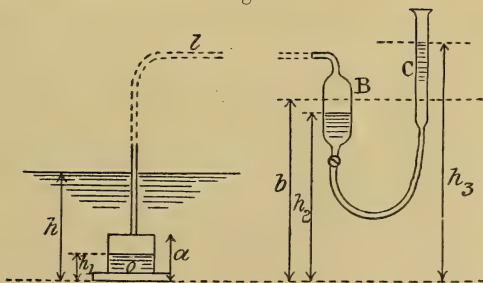


Fig. 4.



jar and the mercury meniscus in the tube C can be found in the following way:—

Let h, h_1 (fig. 4) be the levels of the water above and inside

the jar respectively; h_2, h_3 be the levels of mercury in the tubes B and C. Let b be the common height of the mercury in the tubes, when the jar is not dipped in the water. If Π and P be the pressure of the atmosphere and that within the jar respectively, we have

$$P = \Pi + h - h_1 = \rho(h_3 - h_2) + \Pi,$$

where ρ is the density of mercury.

If s_1, a be the cross-section and the height of the jar, and s_2, s_3 the cross-sections of the tubes B and C respectively, we have, by Boyle's law,

$$P\{s_1(a - h_1) + v + s_2(b - h_2)\} = \text{const.},$$

where v is the volume of the lead tubing, plus that of the part of the tube B lying above the level b .

The differentials of the above two equations are

$$dP = dh - dh_1 = \rho(dh_3 - dh_2),$$

$$dP\{s_1(a - h_1) + v + s_2(b - h_2)\} - P(s_1dh_1 + s_2dh_2) = 0;$$

we have also the equation of continuity

$$s_2dh_2 = -s_3dh_3.$$

Eliminating dh_1, dh_2, dP from these equations, we get

$$\frac{dh_3}{dh} = \frac{s_1P}{\rho\left(1 + \frac{s_3}{s_2}\right)\left\{s_1(a - h_1) + v + s_2(b - h_2) + s_1P\right\} + Ps_3}.$$

Since the first three terms in the denominator of the above expression are very small compared with the fourth, we have, neglecting these small terms,

$$\frac{dh_3}{dh} = \frac{1}{\rho\left(1 + \frac{s_3}{s_2}\right) + \frac{s_3}{s_1}}.$$

Hence the motion of the mercury meniscus in the tube C is practically proportional to the change of water-level. We may also infer from the exact expression for dh_3/dh that the volume of the lead tubing need not be small compared with that of the jar. Even, if the volume of the tubing is equal to that of the jar and the change of the water-level exceeds 10 m., the value of dh_3/dh in my apparatus remains constant up to 0.6 per cent., which is sufficient for practical purposes. Hence the recording apparatus can be set up at a considerable distance from the beach.

It is evident that the reduction-factor $\frac{dh_3}{dh}$ of the apparatus may have any value whatever, which is less than $\frac{1}{\rho}$, by suitably choosing the values of s_1, s_2, s_3 . In my case it was $\frac{1}{16.87}$. It is easy to find experimentally the value of the factor, by raising the immersed jar by a known height and comparing this height with the vertical line recorded on the cylinder.

The effect of temperature may be calculated in a similar way. For this purpose, the product of the volume and the pressure in Boyle's law must be put equal to RT , where T is the absolute temperature and R a constant. Here h is considered to be constant; we have the equations

$$dP = -dh_1 = \rho dh_3 \left(1 + \frac{s_3}{s_2}\right),$$

$$\rho dh_3 \left(1 + \frac{s_3}{s_2}\right) \{s_1(a - h_1) + v + s_2(b - h_2)\} - P(s_1 dh_1 + s_2 dh_2) = R dT,$$

$$s_2 dh_2 = -s_3 dh_3.$$

Eliminating dh_1, dh_2, dP , we have approximately

$$\frac{dh_3}{dT} = \frac{R}{P \left\{ \rho \left(1 + \frac{s_3}{s_2}\right) s_1 + s_3 \right\}} = \frac{R}{s_1 \rho P \left(1 + \frac{s_3}{s_2}\right)}.$$

If we neglect v in comparison with the volume of the jar, R is equal to $P s_1 a / T$; hence

$$\frac{dh_3}{dT} = \frac{a}{\rho T \left(1 + \frac{s_3}{s_2}\right)}.$$

In my case, $a = 12$ cm., $\rho = 13.6$, and $1 + \frac{s_3}{s_2} = 1.26$; hence at 10°C. ,

$$\frac{dh_3}{dT} = 0.0025 \text{ cm.}$$

When the temperature changes, the vapour-tension also changes; but the change of vapour-tension per degree rise of temperature is about $1/3$ the change of pressure due to the thermal expansion of air. Hence as the combined effect of these two, we may take

$$\frac{dh_3}{dT} = 0.0033 \text{ cm.}$$

I have also experimentally determined this ratio by heating the water in which the jar was immersed, and found 0.004 cm., which agrees fairly well with the above value. The greater part of the enclosed air, which is in the diving jar, is subject to the daily change of temperature by 3° or 4° C. of the sea-bottom. Hence in the degree of accuracy of the present tide-gauge, which records the motion of the mercury meniscus, the correction due to the change of temperature is quite to be neglected.

To estimate the effect of the barometric change, both h and T are to be considered as constant ; we then have

$$dP = d\Pi - dh_1 = d\Pi + \rho dh_3 \left(1 + \frac{s_3}{s_2} \right),$$

$$dP \{ s_1(a - h_1) + v + s_2(b - h_2) \} - P(s_1 dh_1 + s_2 dh_2) = 0.$$

$$s_2 dh_2 = -s_3 dh_3.$$

Eliminating dh_1 , dh_2 , dP , and neglecting small quantities, we get

$$\frac{dh_3}{d\Pi} = - \frac{a - h_1}{P\rho \left(1 + \frac{s_3}{s_2} \right)},$$

which is, in my case, nearly equal to -0.04 mm. for the change of barometric pressure by 1 cm. of mercury. Hence the barometric change of 10 cm. in mercury only causes the displacement of the pen not amounting to $1/2$ mm. Thus in the actual case, the correction due to the barometric change is quite insensible.

In the above calculations, I have neglected v in comparison with the volume of the jar; but in the actual case, the influence of v does not materially affect the above conclusions.

On the coast at high latitude, when the sea often freezes, the present tide-gauge works equally well. The tide-gauge may also be used for recording the tide at sea. For this purpose, it is better to suspend the recording apparatus by a rope, instead of placing it on the deck of a ship. The diving jar is plunged to the sea-bottom ; if the sea be too deep, it is necessary to hang the jar at a suitable depth by means of a buoy fixed by a three-way anchor-rope. To avoid the disturbance of surface waves, it is required to keep the buoy itself at a certain depth in the sea.

In passing, the following remarks may be made about the records obtained by my tide-gauge. Plates III. and IV. are the copy of four of the records obtained in Kiūshū.

In the bay of Hososhima on the coast of Hiūga, extremely regular undulations appear superposed on the tidal wave; the period varies from 17·7^m to 20^m according to the tidal phase. The period slightly decreases as the tide passes from the low water to the high. In very calm weather, the amplitude of undulation amounted even to 25 cm. This regular undulation is a stationary oscillation having its node and loop at the mouth and the end of the bay respectively, as actually shown by Y. Yoshida, T. Terada and the author* in several bays on the coast of Sanriku. The period of oscillation is therefore given by the formula

$$T = \frac{4l}{\sqrt{gh}},$$

where l is the length and h the mean depth of the bay. In the present case, the calculated value is 19^m, which fairly agrees with the observed one. The change of the period caused by the tidal motion has also the range which is to be expected from the above formula.

In the bay of Aburatsu in Hiūga, we also observe conspicuous secondary undulations, though they are not regular. The period varies from 15^m to 22^m; the amplitude frequently exceeds 13 cm. The calculated period is 17^m, which lies within the range of the observed periods.

The tidal wave at Kagoshima is generally very smooth; but frequently distinct secondary undulations of 19^m or 24^m are observed.

In the bay of Nagasaki, the secondary undulation is so conspicuous that it is usually known as "*Abiki*." The undulation is not, however, regular; its amplitude often exceeds half a metre. On one occasion, about ten years ago, the amplitude of the *abiki* amounted even to two metres, and a large number of boats and steamers are said to have been damaged. According to my observations, the period irregularly varies from 25^m to 43^m. Judging from the form of the bay, several nodal lines at the mouth are conceivable; thus the largest and smallest values of the calculated periods are 28^m and 41^m, whose interval fairly coincides with the range of the observed periods.

March 30, 1905.

Tokyo, Japan.

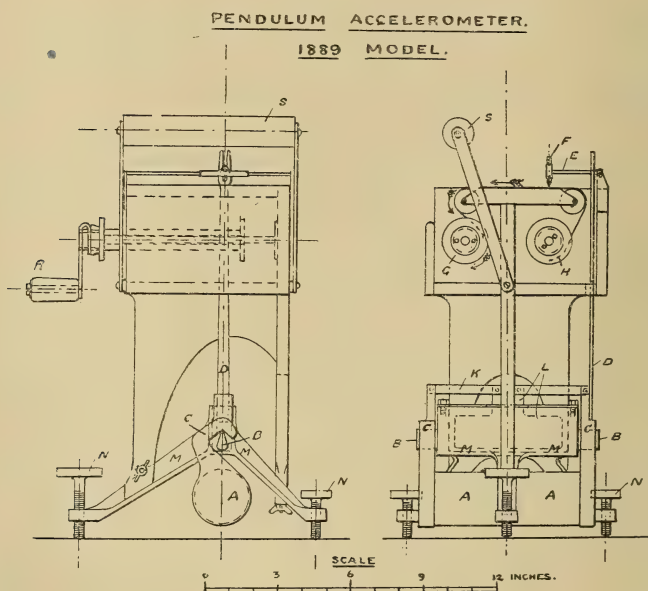
* Honda, Yoshida and Terada, *loc. cit.*

XXXII. *The Pendulum Accelerometer, an Instrument for the Direct Measurement and Recording of Acceleration.* By F. W. LANCHESTER*.

ONE of the problems connected with the testing of locomotive vehicles is that of measuring and recording the magnitude of the starting and stopping efforts, and generally experimentally determining what may be appropriately termed the "ballistics of traction."

The instrument that forms the subject of the present paper has been designed to facilitate determinations of this description with a sufficient degree of accuracy for the ordinary purposes of the engineer, without involving any special and expensive preparation.

Fig. 1.



Note to figs. 1 and 4.

- | | | |
|---------------------------|----------------------|----------------------|
| A. Pendulum-bob. | H. Spool of paper. | O. Anchorage screws. |
| B-C. Knife-edge supports. | J. Datum marker. | P-Q. Clock-work. |
| D. Motion arm. | K. Dash-pot bracket. | R. Wind. |
| E. Pencil arm. | L. Dash-pot piston. | S. Handle. |
| F. Pencil or stilus. | M. Frame casting. | |
| G. Winding drum. | N. Levelling-screws. | |

The fundamental principle on which the instrument works, is that the effect of the algebraic sum of tractive forces and resistances on the vehicle as a whole, is shared proportionately by every portion of its mass, and that consequently the

* Communicated by the Physical Society : read June 16, 1905.

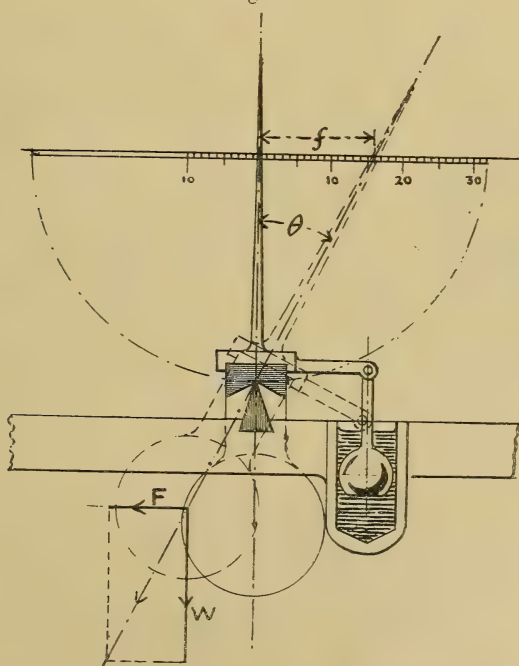
tractive effort on the whole vehicle can be ascertained if that acting on any portion of its mass be measured.

It is evident that the actual measurement on a known mass carried on the vehicle could be made by means of a spring-balance, the mass being mounted so as to permit of its horizontal motion only; but it is also possible and it is easier to arrive at the same result by arranging the mass as the bob of a pendulum and reading the *coefficient* of traction directly in the tangent of the angle of its deflexion.

It will be recognized at once that the quantity being measured in this manner is the acceleration of the vehicle: hence the instrument is in reality an accelerometer, the measurement of gross tractive efforts and resistances requiring in many instances the instrument to be used indirectly, the direct reading giving the net difference between the propelling and resisting forces.

The first pendulum accelerometer was made by the writer as far back as 1889; this somewhat rough model is still in existence (fig. 1), and many of the diagrams taken on the

Fig. 2.



railway in 1889 have been preserved (fig. 9). This instrument was not fitted with clockwork, so that the time-scale is irregular.

Fig. 2 shows in diagram form the essential working parts

of the pendulum accelerometer ; and fig. 3 shows the actual proportions of my present (1904) model. Referring to fig. 2 in which a resolution of forces is given, let the angle of deflexion of the pendulum be denoted by θ , " W " = forces on pendulum-bob due to gravity, and " F " = force due to acceleration " f ," then we have :—

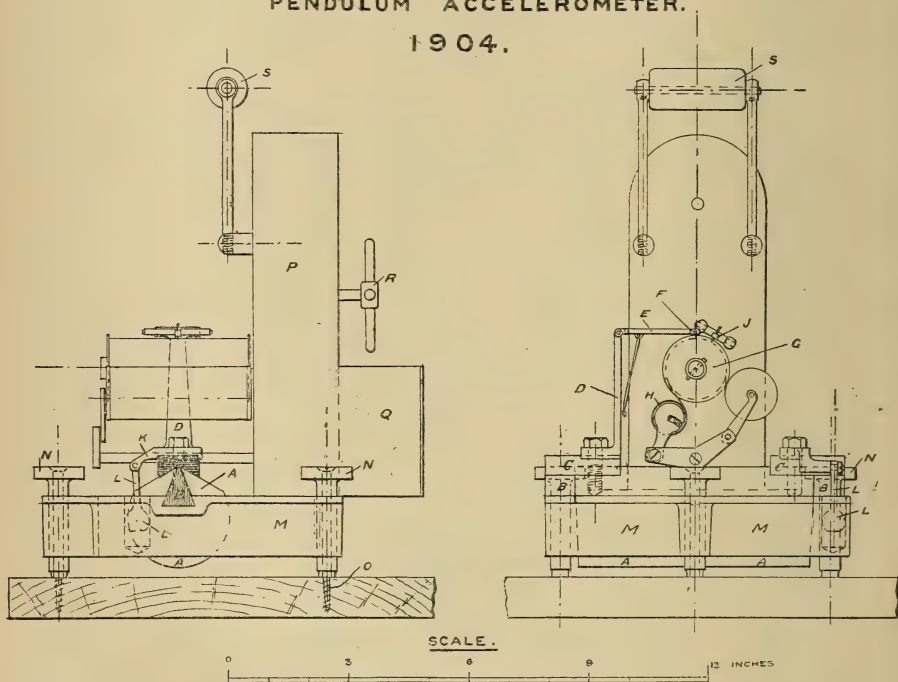
$$f = g \cdot F/W = g \cdot \tan \theta.$$

It will be seen, therefore, that the motion of the recording pencil in order to give a uniform acceleration scale must be

Fig. 3.

PENDULUM ACCELEROMETER.

1904.

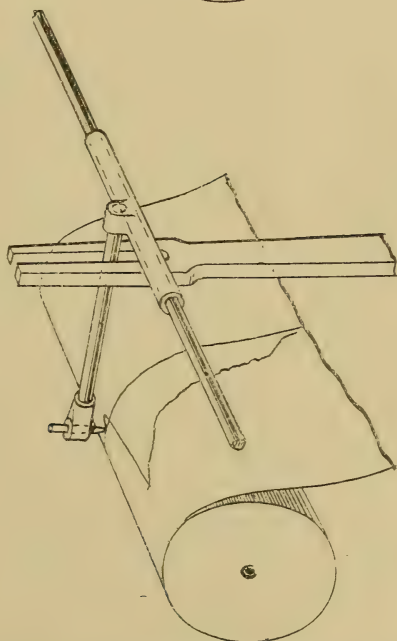


proportional to the tangent of the angle of deflexion. It is the form of "tan angle mechanism" that constitutes one of the improvements in the new model.

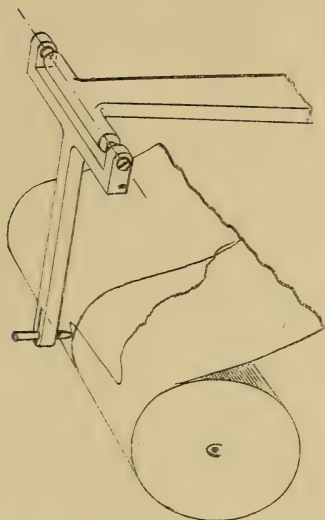
In the 1889 model the pencil was mounted on a sliding carriage (fig. 4) actuated by a forked arm, being an upward prolongation of the pendulum: in this arrangement, in addition to the pencil friction, two additional sources of sliding friction existed, those of the carriage mounting and forked operator.

Fig. 4.

TAN. - ANGLE MECHANISM.



1889



1904

In the present form of instrument the pencil arm is pivotted directly to the pendulum continuation in such manner that the point of the pencil lies always in the plane of the pendulum

axis. The only theoretical disadvantage of this arrangement is that a small time error is introduced, but provided the pencil arm be made reasonably long this error is infinitesimal.

In order that the accuracy of the instrument shall not be interfered with, the pendulum should be made as short as possible. It is assumed in the theory of the instrument, that the motion of the pendulum-bob is substantially that of the rest of the vehicle, and consequently its motion of swing should be negligible in comparison with the motion of the vehicle; also its time of oscillation should be small in comparison with the time of change of acceleration that it is desired to accurately record. This point was not overlooked in the original model, in which the length of pendulum was 3 inches only; in the present model this has been reduced to $1\frac{1}{4}$ in., beyond which it is doubtful whether it is desirable to go.

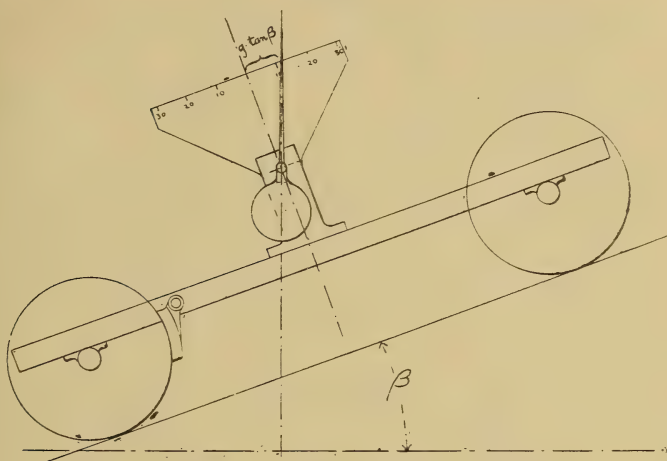
The dash-pot, which is an absolute necessity for working on road vehicles, in the original instrument took the form of an oil-well of large dimensions in which dipped a vane or paddle of considerable area. On the present model the dash-pot is a simple cylinder with spherical piston making an approximate fit, the cylinder is filled with viscous oil and is adjusted to make the pendulum as nearly as possible "dead-beat."

Considered purely as an acceleration meter, a correction would be necessary for any change of gradient; and if it were required, for instance, to integrate the diagrams to obtain velocity curves, either the precaution would have to be taken to work on level roads, or means found of making the necessary corrections. It is a point of great interest and importance that for the purposes for which this instrument is principally required, starting and stopping efforts, frictional and other resistance, &c., the readings are unaffected by gradient and no corrections are required. Put tersely, it may be said that the acceleration produced by the gravity component exactly compensates or neutralizes the direct effect of the gradient.

This point is one that it is quite easy to make clear. Let fig. 5 represent a car standing with brake applied on a gradient whose angle of slope is β , then the accelerometer will record an amount equal to $g \tan \beta$ (provided that it has previously been brought to zero on the level): now this will be a correct measure of the brake and frictional effect employed in holding the car up. Now let us suppose the brake removed, then the true acceleration (neglecting friction) will rise from zero to $g \tan \beta$, which will bring the accelerometer reading exactly to zero, which under these circumstances is the precise measure of the applied tractive forces.

This reasoning obviously extends to partial removal of the brake, and is in fact general.

Fig. 5.



In order to facilitate the accurate setting up of the instrument, it is fitted with a transverse level to ensure getting the knife-edges approximately level, the pendulum itself being relied on to obtain the correct zero position, a separate pencil or datum-line marker being fitted to the instrument, and the setting being correct when the datum-line and record-line correspond when the car is at rest on the level. When setting up on a motor-car, an approximately level place is found on the road, two trials of the setting are made with the car in opposite positions and the error divided. When on the railway a known level is chosen for setting up.

A few typical diagrams taken on the railway and on motor-cars are given in figs. 6, 7, and 8.

In fig. 8 we have a group of brake diagrams taken on the original model in 1889 on the Great Western and the Metropolitan Railways. These at once bring out a point of great interest, the sudden change of acceleration that takes place at the exact instant the train comes to rest; this is a characteristic of nearly all brake diagrams and will be referred to again. The max. negative acceleration recorded in these diagrams is about 4 ft./sec. sec. A starting diagram taken on the same journey is given. The maximum starting acceleration here recorded is about .6. There is unfortunately no record of the type or weight of engine or the number or weight of the coaches.

Fig. 6.

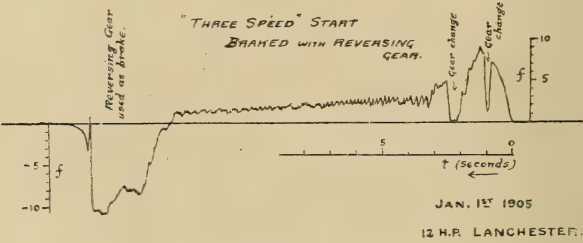
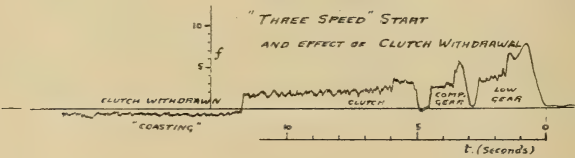
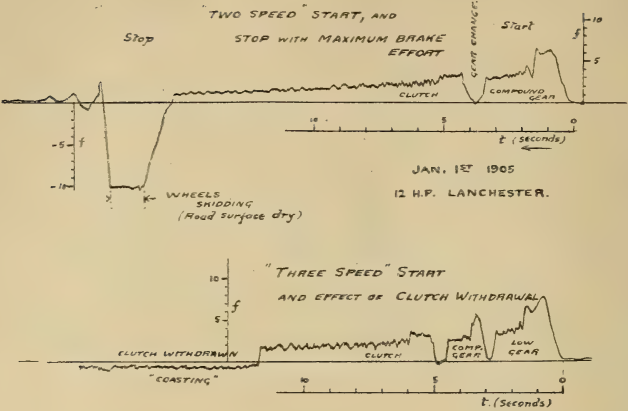
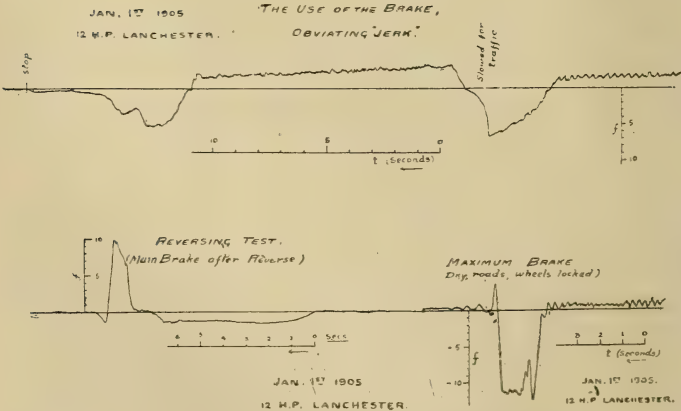


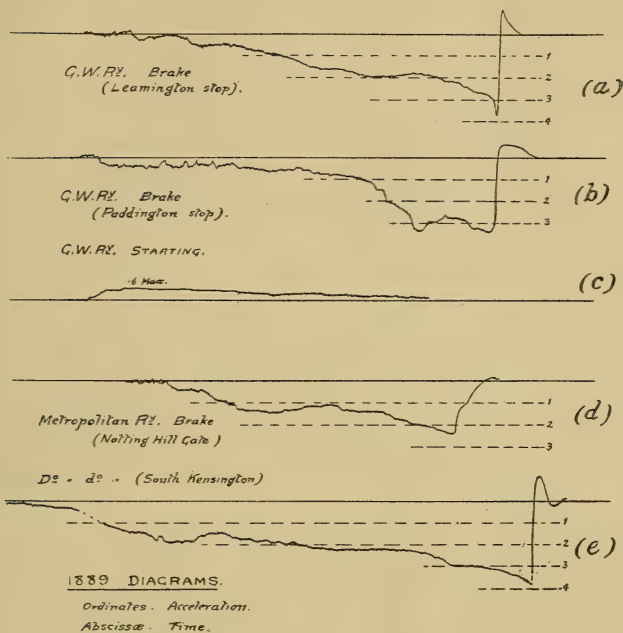
Fig. 7.



Figs. 6 and 7 give a few typical motor-car starting and brake diagrams. The greatest negative acceleration here recorded amounts to 11.5 ft./sec. sec. against a maximum of

8.5 ft./sec. sec. positive. These give respectively about 800 lbs. per ton brake and 600 lbs. per ton starting effort.

Fig. 8.



The ordinate (acceleration) scale employed in the present instrument is 10 ft./sec. sec. per inch, and is not quite open enough to give pleasing diagrams in the railway. The original model with its scale of 3 ft./sec. sec. per inch was better suited to this class of work; but the much greater range and general compactness of the 1904 model renders it much handier and better for motor-car work, the purpose for which it is primarily intended.

In figs. 6 and 7 the time-scale (abscissæ) reads from right to left; in fig. 8 the time-scale reads in the opposite direction from left to right.

It has been remarked that a characteristic feature of brake diagrams is the suddenness of the drop at the instant of stopping. This is a very interesting and important point, inasmuch as it is the cause of the "jerk" nearly always experienced just as a train comes to rest; it was in fact in

investigating this jerk in 1888 that the idea of the pendulum accelerometer occurred to the writer. At that time it was currently supposed that the jerk was the effect of the recoil of the buffer springs after stopping; whereas a very little consideration shows that it is in reality sudden *change of acceleration* that we recognize physiologically as "jerk," that is df/dt , and not change in the direction of motion. It suggests itself in fact that the term "jerk" might well be given a scientific meaning and be defined as d^3s/dt^3 .

The value of df/dt at the moment of stoppage in the case of a rigid body sliding on a rigid plane surface would be infinite. In actual fact, in the case of a train brought to rest by its brakes the value of df/dt is so high that the correct form of the curve at the moment of stopping is beyond the power of the instrument to properly record. In all the diagrams the lag of the pendulum will be noticed, and in most cases the line shows that the "dash-pot" has been insufficient and the pencil has overshot the mark. The peculiar form found on diagram 3 fig. 6 is due to a partial vacuum formed in the dash-pot on the sudden release of the pendulum from its extreme position. It is probable that the correct form of the drop would be sensibly perpendicular to the datum-line.

It should be more widely recognized that a great part of the art of properly braking a train, or in fact any other vehicle, consists in taking the brake nearly off just before stopping. It is not easy to obtain a train diagram in which this has been done, but diagram 1 in fig. 7 is a good example taken on a car in which the driver was paying special attention to the point.

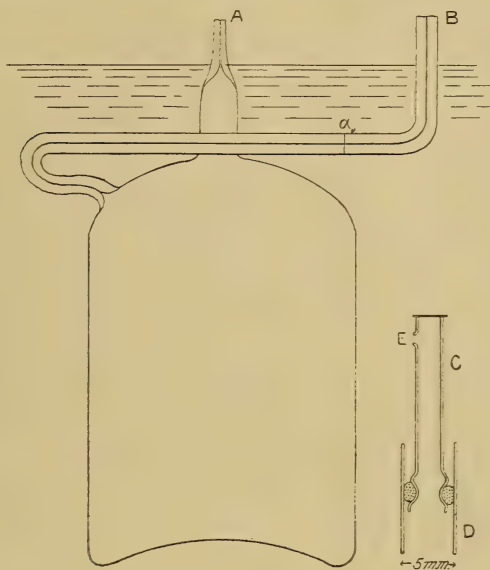
Many of the diagrams, figs. 6 and 7, are particularly interesting to the motor-car engineer: the loss of *starting effort area* (which is equivalent to velocity) during gear change, the exalted starting effort obtained whilst the energy of the flywheel is being drawn upon, the curve of falling effort (largely due to increasing wind resistance),—all these are points that repay the most careful study.

Other applications I anticipate for the pendulum accelerometer are its employment in the determination of the lateral (centrifugal) forces and coefficient of friction when rounding corners, the determination of wind resistance at different speeds and other resistances to traction, the determination of the thrusts and resistances of launches &c.; but these and the further discussion of the engineering aspect of the subject does not come within the purview of the present paper.

May 11th, 1905.

XXXIII. *A new Form of Pyknometer.**By R. V. STANFORD*.*

THE construction of this modification of the common U-shaped pyknometer will be seen from the accompanying sketch. It may be readily made before the blowpipe,



of any desired content from 1 c.c. upwards. It is filled with the liquid whose density is desired by attaching at B a tube which dips into the dish containing it, and applying suction at A. The apparatus, entirely filled with liquid at room temperature, is placed upon a stand in a thermostat, and the plunger CD affixed at B by means of rubber tubing. When the liquid has attained the temperature of the bath, a finger is placed over the aperture E in the piston-rod, and the piston depressed until the liquid meniscus has descended to the mark α . The finger is then removed from E, the plunger detached, and the instrument removed from the thermostat and weighed. The plunger consists merely of a piece of glass tubing some 0.5 cm. outside diameter, into which a smaller tube fits, as shown in the figure. The piston is made airtight by means of a small rubber ring.

This form of pyknometer has the advantages that it dispenses with a wire suspension, and that the adjustment to the

* Communicated by the Physical Society: read June 16, 1905.

mark α is much easier and more accurate with the plunger than is the usual one by means of blotting-paper applied at A, and in addition the horizontal arm permits of its employment in the common thermostats with opaque sides, while reducing to a minimum the amount of contained liquid outside the bath.

The University, Birmingham.

XXXIV. *A Simple Method of Determining the Radiation Constant: suitable for a Laboratory Experiment. By A. D. DENNING, M.Sc., Ph.D., Demonstrator in Physics in the University of Birmingham*.*

THE following experiment was suggested to me by Prof. Poynting, F.R.S., as a laboratory experiment, and as it is by no means difficult to carry out and appears to give good results, it may be useful to give an account of it.

The principle of the method followed may be thus briefly described:—A hemispherical radiator, blackened inside, was quickly placed over a flat silver disk of known dimensions and which formed one of the junctions of a constantan-silver thermoelectric couple. From observations, at definite intervals of time, of the deflexions of the moving coil of a low-resistance d'Arsonval galvanometer included in the circuit, the initial rate of rise of temperature† of the disk was obtained and this result, when substituted in the equation (α), given below, gives σ the radiation constant.

For, suppose m = mass of silver disk,

s = its specific heat,

dT/dt = initial rate of temperature change,

then $ms \cdot dT/dt$ = initial amount of heat received by disk.

But if A = exposed area of disk,

R = radiation from that area per unit of time,

and R_1 = radiation from blackened hemisphere,

then the gain of heat by the plate

$$= (R_1 - R)A,$$

$$= ms \cdot dT/dt.$$

Using Stefan's Law, we may write:

$$R = \sigma T^4 \quad \text{and} \quad R_1 = \sigma T_1^4$$

where σ = the radiation constant,

T & T_1 = the temperatures of disk and hemisphere, respectively, measured from -273°C .

* Communicated by the Physical Society: read May 12, 1905.

† The initial rate of temperature rise being taken to avoid gain of heat by disk from conduction and convection effects.

Consequently

$$ms \cdot dT/dt = A\sigma(T_1^4 - T^4),$$

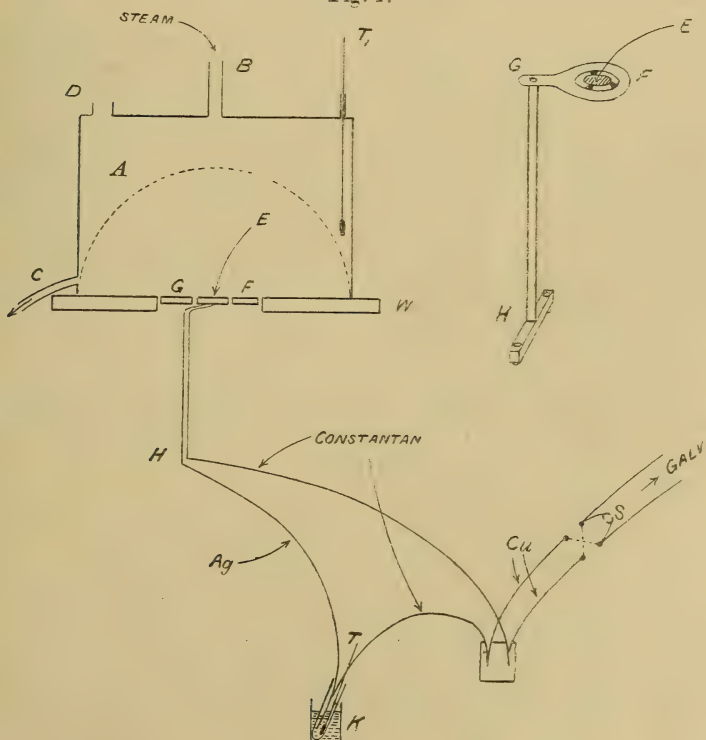
or

$$\sigma = \frac{ms}{A(T_1^4 - T^4)} \cdot \frac{dT}{dt} \dots \dots \dots (\alpha)$$

If the galvanometer readings were proportional to the current existing at the instant of reading, we should have dT/dt by simply noting successive deflexions at known times. But, owing to the moment of inertia of the moving part of the galvanometer and the viscous resistance, the reading will not always indicate the current at the instant, as will be seen from the investigation below (Appendix). In this case, however, the readings did indicate the temperature, when plotted to eliminate oscillations.

The actual arrangement of the apparatus is shown diagrammatically in fig. 1. A is a cylindrical vessel of

Fig. 1.



15.3 cms. internal diameter: to the underneath side of this vessel was soldered a polished copper hemisphere, indicated

in the figure by the dotted line, and which, before an experiment, was evenly coated with a layer of lampblack produced by the combustion of camphor. The space between the cylindrical and hemispherical vessels was used as a reservoir for steam or the other substances which were employed to maintain the hemispherical radiator at the temperature T_1 . The tube B served as an inlet and the tube C as an outlet for the steam; the wider tube, D, was subsequently added for the purpose of covering the hemispherical radiator with ice. During the course of a series of observations this part of the apparatus rested on the wooden board, W. (The stands supporting this latter are not shown in the diagram.)

As mentioned above, the elements of the thermo-junctions were silver and constantan. In order to have an appreciably large surface to receive the radiation at the one junction, the silver and constantan wires were soldered on to a silver disk, E. This disk, after having been thoroughly well polished, was mounted in a vulcanite frame, FG, which fitted into the wooden board carrying A. The disk also was lampblackened before being used. If it be considered desirable to prevent the disk receiving radiation at the sides, felt or some other non-conducting material might be so wrapped round the disk that only the top surface of it was exposed to the hemispherical radiator. The wires coming from the disk were led down the arm GH to the binding-screws at the bottom. The other junction of the thermo-couple was placed, together with a thermometer, T, in a test-tube containing oil in the glass beaker, K. The two constantan wires were further soldered to copper wires leading through a mercury commutator to the galvanometer. These last two (the Cu-constantan) junctions were passed through rubber tubing fitting into two holes in the lid of a tin canister packed with cotton-wool.

The dimensions of the silver disk were:

$$\begin{aligned} \text{Diameter} &= 2.015 \text{ cm.} & \text{Weight} &= 8.911 \text{ gr.} \\ \text{Thickness} &= 0.275 \text{ cm.} & \text{Sp. Ht.} &= 0.0567. \end{aligned}$$

This value for the specific heat was the mean of several determinations made by means of a Joly steam calorimeter.

Inasmuch as the source of steam and also the outlet from the vessel A were on the side CD of the apparatus, a long piece of sheet nickel, polished on the one side, was tacked on to the wooden stand W, with the idea of screening the underside of the silver disk from the hot air currents. It was also found advisable to further protect the disk from

draughts by loosely fixing under it a wide pad of cotton-wool, held in position by a wire passing between the retort stands supporting W, since otherwise the opening and shutting of doors, &c., was sufficient to occasion an irregular and jerky motion of the galvanometer-coil.

During the boiling of the water, the silver disk was covered by a screen with one edge resting on two thin corks to prevent actual contact between the disk and the screen. This screen was made by taking a piece of sheet nickel, approximately twice the size of the board W, doubling it into two with the dull side inwards and, after placing a layer of cotton-wool between the folds, wiring the two folds loosely together.

When steam had passed for some time through the vessel A, the latter was placed on the screen. At a particular instant the screen was removed, A was lowered over the disk and some six to ten readings of the position of the cross-wire image were taken at intervals of five seconds (in the majority of cases).

In order to find the temperature equivalent of the galvanometer deflexions, the silver disk was kept at a constant temperature, and the deflexions of the galvanometer were noted when the temperature of the other junction was altered by a known amount—subsequent reference is made to this point.

Before beginning an experiment it is necessary to ascertain that the two thermo-junctions are at the same temperature or to know the difference of temperature between them. In these experiments the following procedure was adopted:—By insertion of the shunt-key S the galvanometer was short-circuited and its zero-position found. Did the deflexions, consequent to the removal of the key, indicate that the temperature of the disk was too low, the hand was held over the disk until the cross-wire image returned to its initial position of rest; whilst if the temperature were too high, ether was poured on to a piece of cotton-wool resting on a crucible lid, and this was allowed to evaporate in the neighbourhood of the disk.

The results of a number of experiments extending over varying ranges of temperature are given in Table I. In the last column but one are given the values of the initial rate of change of temperature, whilst the last column contains the values found for $\sigma \cdot 10^5$. It will be noticed that in some of these experiments alcohol and acetone were passed through the radiating vessel and afterwards condensed in a condensing worm (not shown in fig.). But subsequent experiments

TABLE I.

	Date.	Temperatures.		dT/dt .	$\sigma \cdot 10^5$.
		T_1° .	T° .		
Steam {	29/11/04	100 ^o ·0 C.	18 ^o ·7 C.	9·44	5·19
	12/12/04	99·5	15·6	9·71	5·30
	12/12/04	99·5	16·3	9·55	5·22
	17/12/04	98·5	17·4	6·47	3·36
Alcohol {	19/12/04	78·6	14·8	4·95	3·92
	19/12/04	78·6	9·2	7·25	5·39
	20/12/04	78·6	14·5	6·71	5·32
Acetone {	20/12/04	57	12·8	4·24	5·43
	20/12/04	57	13·1	4·24	5·46
Ice {	21/12/04	0	15·2	1·06	5·24
	21/12/04	0	14·7	1·06	5·42

(results recorded in Table II.) showed that easier manipulation and equally satisfactory results were to be obtained by simply placing hot water in A ; owing to the large bulk of water which A could hold (nearly two litres), its rate of cooling was comparatively slow ; whereas the observations necessary for each experiment were made in less than a minute.

TABLE II.

Date.	Temperatures.		dT/dt .	$\sigma \cdot 10^5$.
	T_1° .	T° .		
27/1/05	100 ^o ·5 C.	16 ^o ·6 C.	9·83	5·26
27/1/05	100·5	17·8	9·83	5·29
28/1/05	75·0	17·8	5·99	5·30
28/1/05	73·0	17·8	5·76	5·32
28/1/05	60·5	17·6	4·32	5·49
28/1/05	60·0	17·8	4·32	5·39
28/1/05	45·0	15·5	2·64	5·32
28/1/05	43·8	15·8	2·40	5·40
31/1/05	0	15·7	1·08	5·14
31/1/05	0	14·9	1·08	5·48

By quickly removing the radiator after sufficient readings had been taken and placing the cotton-wool saturated with

ether near the disk, it was possible to take several sets of observations in fairly quick succession—especially if a current of air were maintained across the disk. Owing to the small range of temperature prevailing when the ice was used and the possibility of disturbances due to convection, such close agreement with the other results as was actually obtained was hardly to have been expected. It has been thought advisable to include all the experiments made to indicate what kind of accuracy can be expected.

A glance at the values for σ given in the last columns of both tables will at once show that two of the values are very much lower than the other nineteen. The most probable explanation for this is that somewhere in the galvanometer-circuit there was poor contact and a consequent increase in the circuit-resistance—most likely in the mercury commutator, since these at times are very unreliable. Indeed, determinations of the equivalent of the galvanometer readings made from time to time during the course of the first series of experiments showed variations in the sensibility of the galvanometer. Hence it were better to measure the sensitiveness of the galvanometer directly before taking a series of observations by arranging that the simple reversal of a commutating-switch might place the galvanometer in circuit with a low resistance, the fall of potential along which is a constant small fraction of the E.M.F. of a storage-cell.

Ignoring these two values, we find uniformity and agreement among the remainder—the average of the residual nineteen results being about 5.3. The mean of Kurlbaum's more elaborate experiments was 5.32. The simplicity of construction of the apparatus and the principles embodied in its use will, it is hoped, recommend the experiment as a laboratory method of illustrating an important and fundamental law.

APPENDIX.

Note on Motion of Galvanometer Coil.

In order to take into account the effect of the moment of inertia of the moving coil and the viscous resistance of the medium, let us suppose that a steady difference of 1° C. between the thermo-junctions gives a steady deflexion of the galvanometer-coil of λ radians, then a steady difference of temperature of T' would give a steady deflexion

$$\theta = \lambda T'.$$

If the torsional couple per radian exerted by the suspension is μ , then the couple on the fibre in a steady state would be

$$\mu\theta = \mu\lambda T'.$$

But the observations are taken during the motion of the galvanometer-coil, *i. e.* when the change of temperature is not steady.

In this case

$$\mu\theta = \mu\lambda T - I \frac{d^2\theta}{dt^2} - K \frac{d\theta}{dt};$$

where

I = the moment of inertia of coil,
 K = the viscosity coefficient of the air,
 T = the initial difference of temperature ;

that is

$$I \frac{d^2\theta}{dt^2} + K \frac{d\theta}{dt} + \mu\theta = \mu\lambda T ;$$

putting

$$K/I = k \quad \text{and} \quad \mu/I = n^2.$$

the equation of motion becomes

$$\frac{d^2\theta}{dt^2} + k \frac{d\theta}{dt} + n^2\theta = n^2\lambda T.$$

Now, if T be approaching a steady value A , according to the exponential law we have

$$T = A(1 - e^{-pt}) ;$$

consequently

$$\frac{d^2\theta}{dt^2} + k \frac{d\theta}{dt} + n^2\theta = n^2\lambda A(1 - e^{-pt}),$$

the solution of which gives

$$\theta = \lambda A \left(1 - \frac{n^2 e^{-pt}}{p^2 - kp + n^2} \right) - B e^{\frac{-kt}{2}} \sin \left(\sqrt{n^2 - \frac{k^2}{4}} \cdot t + \alpha \right).$$

The second term is evidently oscillatory so long as $n^2 > \frac{k^2}{4}$;

but if k be large it dies away rapidly, or to such an extent that it can be eliminated by plotting, and we have only to deal with

$$\theta = \lambda A \left(1 - \frac{n^2 e^{-pt}}{p^2 - kp + n^2} \right)$$

differentiating

$$\frac{d\theta}{dt} = \lambda p A e^{-pt} \cdot \frac{n^2}{p^2 - kp - n^2};$$

but from the differentiation of the exponential equation,

given above,

$$\frac{dT}{dt} = pAe^{-pt},$$

and since we only want the initial rate of rise of temperature

$$\frac{dT}{dt}_{t=0} = [pAe^{-pt}]_{t=0} = pA,$$

but

$$\frac{d\theta}{dt}_{t=0} = \frac{\lambda p A n^2}{p^2 - kp + n^2}.$$

Hence

$$\frac{dT}{dt}_{t=0} = pA = \frac{p^2 - kp + n^2}{n^2} \cdot \frac{d\theta}{dt} \bigg/ \lambda.$$

By plotting galvanometer deflexions against the times, we can eliminate the oscillations and obtain the initial rate of change of θ , i. e. $\frac{d\theta}{dt} \bigg/ \lambda$, from the curve. (And since we are only concerned with the ratio of $\frac{d\theta}{dt} \bigg/ \lambda$ we can use galvanometer scale-divisions instead of radians.)

Consequently, then, in order to obtain the initial rate of rise of temperature, the observed $\frac{d\theta}{dt} \bigg/ \lambda$ must be multiplied by

$$(p^2 - kp + n^2)/n^2.$$

Should $p^2 - kp$ be small in comparison with n^2 , this fraction may evidently be neglected. But to test this we must first find n , p , and k for the particular galvanometer in use.

(i) To find n :

If the circuit is open, k is negligible and $2\pi/n$ is the time of swing. Or, if the observed period of free swing be P then

$$n = 2\pi/P.$$

(ii) To find k :

For the deflexion θ on closed circuit we have

$$\theta = Be^{\frac{-kt}{2}} \sin \left(\sqrt{n^2 - \frac{k^2}{4}} \cdot t + \beta \right),$$

and if successive deflexions or elongations θ_1 , θ_2 , θ_3 , and θ_4 be observed it is easily seen that

$$\log \frac{\theta_1 - \theta_2}{\theta_3 - \theta_4} = \frac{k\pi}{\sqrt{n^2 - \frac{k^2}{4}}}.$$

(iii) To find p :

From the exponential law we have

$$\frac{dT_1}{dt} = Ape^{-pt_1},$$

and therefore

$$\frac{d\theta_1}{dt} \propto Ape^{-pt_1} = A_1pe^{-pt_1} \text{ say.}$$

Similarly for time t_2 (where $t_2 = 2t_1$)

$$\dot{\theta}_2 = A_1pe^{-pt_2} = A_1pe^{-2pt_1},$$

that is

$$\dot{\theta}_1 / \dot{\theta}_2 = e^{pt_1},$$

or

$$pt_1 = \log \dot{\theta}_1 / \dot{\theta}_2.$$

But by plotting the deflexions against the times we get the deflexion curve $\theta = A_1(1 - e^{-pt})$,

and from this curve we may measure $\dot{\theta}_1$ and $\dot{\theta}_2$ for a pair of times such that $t_2 = 2t_1$, and consequently we find

$$p = \frac{1}{t_1} \log \frac{\dot{\theta}_1}{\dot{\theta}_2}.$$

With the galvanometer used in these experiments the following values were obtained for these quantities :

$$p = 0.0069; \quad n = 0.654; \quad \text{and} \quad k = 0.127,$$

which, when substituted, give

$$\frac{p^2 - kp + n^2}{n^2} = \frac{427}{428},$$

which may be taken as 1, that is to say, the observed value of $\frac{d\theta}{dt} / \lambda$ could be taken as a measure of the initial rate of rise of temperature of the silver disk.

XXXV. *Notices respecting New Books.*

Ueber Harmonie und Complication. Von Dr. VICTOR GOLDSCHMIDT. Pp. 136+iv, with 28 figures in the text. Berlin: Julius Springer. 1901.

Über harmonische Analyse von Musikstücken. Von VICTOR GOLDSCHMIDT. Ostwald's *Annalen der Naturphilosophie*: Leipzig, 1904, vol. iii. pp. 449-508.

FOR centuries has it been a favourite study of able thinkers to investigate the fundamental principles upon which the various branches of knowledge have been developed, and to trace a concordant relationship in phenomena to all appearance entirely dissimilar and disconnected. No doubt it will be ultimately discovered that the varied and apparently divergent paths along which science progresses meet eventually in the same, though may be many-sided, goal—the constitution of matter. In these days of extreme specialization, when each worker sees little more than a small portion of some particular subject, it is of importance to have our thoughts occasionally brought back to the possible relations underlying the fundamental principles of different branches of science. Professor Goldschmidt is well known as one of the leading crystallographers of the day; and his work has always been distinguished by its versatile and philosophical character. In this remarkable treatise *Ueber Harmonie und Complication* he has, with characteristic energy, undertaken a task requiring a breadth of knowledge such as is rarely possessed by any individual. Few probably feel at home in all of such a diversity of subjects as are discussed by him; and, indeed, to many such a conjunction may seem altogether fantastic, to be dismissed with a jest. Nevertheless Professor Goldschmidt submits to the reader a strong case; and the evidence produced is far sounder than the kind met with in most philosophical writings dealing with uniformity in the universe.

More than a century has elapsed since Häuy established the great crystalline law on which the whole science of crystallography has been built. This law, usually known as the law of rational indices, states that the positions of the natural faces bounding a crystal are not haphazard, but obey a very simple relation. Häuy himself studied the more complicated question of the arrangement of the ultimate units in a crystal; and the law mentioned is an immediate deduction from the principles laid down by him. Later writers have, however, seldom gone behind the law, and they mostly ignore the absolute positions of the faces. The habit, that little understood character of crystals, which varies so strangely in crystals identical in other respects, is not considered or explained by this law, beyond that all crystals have the symmetry of one of thirty-two classes. Professor Goldschmidt, in the course of a number of important papers which have appeared in the *Zeitschrift für Krystallographie und Mineralogie* during recent years, proposes an extended form of this law, which includes the principle of symmetry and at the same time places the relative development

of the faces lying in the same zone in a novel and clearer light. Thus, suppose we have a series of such faces ABCDEFGHI, no two of which are parallel; we select the two most prominent faces, A and B, as coordinate faces and the next in importance, C, as the unit face. The remaining faces, then, fall into groups [DE], [FGHI], and so on, as shown in the following Table. The members of each group will be similar in their characters: size, frequency of appearance, &c.

Primary faces: A		B	
N_0	= 0	∞	Normal row 0.
1 complication: A		C	B
N_1	= 0	1	∞ Normal row 1.
2 complication: D		D	C E B
N_2	= 0	$\frac{1}{2}$	1 2 ∞ Normal row 2.
3 complication: AFDG		CHEI	B
N_3	= 0	$\frac{1}{3}$ $\frac{1}{2}$ $\frac{2}{3}$	1 $\frac{3}{2}$ 2 3 ∞ Normal row 3.

The numbers under each face are the ratio of the corresponding indices, supposing that A and B are the pinakoids (0 1) and (1 0). Each row is developed from the preceding in precisely the same way.

It was natural to one of Professor Goldschmidt's philosophical temperament not to confine his attention to the morphological characters of crystals, but to investigate the possibility of a wider application of this same law. His investigations resulted in the work under discussion, *Ueber Harmonie und Complication*.

The brief introduction deals with the arrangement of the faces on a crystal, from which, as we have pointed out, the author originally deduced his law of complication. Then follows probably the most important and—running as it does to 66 pages—certainly the longest part of the book, on the Sensations of Tones. The science of sound in its physical, its æsthetic, and its physiological aspect has received so much attention and study as to provide obviously a ready means of testing the principle of complication developed by the author. We may here remark that the later treatise, *Über harmonische Analyse von Musikstücken*, is an extension of this part with a somewhat different treatment, as will be explained below, and is intended more particularly for musicians.

The notes comprising the diatonic scale of C major and their relative vibrations (z) are

c	d	e	f	g	a	b	$\bar{c}$
$z = 1$	$\frac{9}{8}$	$\frac{5}{4}$	$\frac{4}{3}$	$\frac{3}{2}$	$\frac{5}{3}$	$\frac{15}{8}$	2
tonic	second	third	fourth	fifth	sixth	seventh	octave

By means of the simple transformation $p = \frac{z-1}{2-z}$ another series of harmonic numbers is obtained :

	<i>c</i>	<i>d</i>	<i>e</i>	<i>f</i>	<i>g</i>	<i>a</i>	<i>b</i>	<i>c̄</i>
$p = 0$		$\frac{1}{7}$	$\frac{1}{3}$	$\frac{1}{2}$	1	2	7	∞ ,

which bears an obvious resemblance to the third normal row. It, however, lacks the components $p = \frac{2}{2}, \frac{3}{2}$, and 3, and contains two with high symbols which do not occur in that row. The latter notes, as the author plausibly explains, have really no part in the scale of C, but belong to that of the dominant, being the fifth and third respectively in that scale. Of the additional notes required to complete the row, $b\flat$ ($p=3$) is very closely related to the tonic, and, indeed, forms the ordinary modulation to the chord of the sub-dominant. The remaining notes $f\sharp$ ($p=\frac{2}{3}$) and $a\flat$ ($p=\frac{3}{2}$) are both associated, though not so closely as the remaining notes of the series, with the harmony of C major. The complete series corresponding to the third normal row is finally

	<i>c</i>	<i>e</i>	<i>f</i>	<i>f\sharp</i>	<i>g</i>	<i>a\flat</i>	<i>a</i>	<i>b</i>	<i>c̄</i>
$p = 0$		$\frac{1}{3}$	$\frac{1}{2}$	$\frac{2}{3}$	1	$\frac{3}{2}$	2	3	∞

Since the harmonic numbers increase with the pitch, the author terms this series "rising harmony."

Another harmonic series is obtainable in the same way from the wave-lengths (*l*) by means of the transformation $\bar{p} = \frac{l-1}{2-l}$. The wave-lengths are inversely proportional to the vibrations, and we may for convenience assume that $z = \frac{2}{l}$, and consequently $p\bar{p} = 2$.

Applying this transformation to the scale of A minor, we have

	<i>a</i>	<i>g</i>	<i>f</i>	<i>e</i>	<i>d</i>	<i>c</i>	<i>b</i>	$\frac{a}{\bar{a}}$
$z = 2$		$\frac{7}{4}$	$\frac{8}{5}$	$\frac{3}{2}$	$\frac{4}{3}$	$\frac{6}{5}$	$\frac{8}{7}$	1
$l = 1$		$\frac{8}{7}$	$\frac{5}{4}$	$\frac{4}{3}$	$\frac{3}{2}$	$\frac{5}{3}$	$\frac{7}{4}$	2
$\bar{p} = 0$		$\frac{1}{6}$	$\frac{1}{3}$	$\frac{1}{2}$	1	2	3	∞

The bar over the letters distinguishes the method of transformation, and has no negative or minus significance.

As in the previous case, the note with the high symbol is omitted, and the missing members of the third normal row are introduced, giving the complete series :

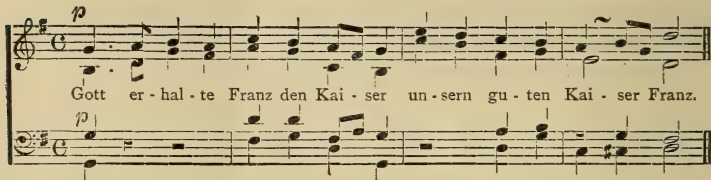
	<i>a</i>	<i>f</i>	<i>e</i>	<i>e\flat</i>	<i>d</i>	<i>c\sharp</i>	<i>c</i>	<i>b</i>	$\frac{a}{\bar{a}}$
$\bar{p} = 0$		$\frac{1}{3}$	$\frac{1}{2}$	$\frac{2}{3}$	1	$\frac{3}{2}$	2	3	∞ .

Since the harmonic numbers decrease with the pitch, the author terms this series "falling harmony."

Thus the major and minor scales appear in a new and interesting light as, so to speak, the inverse or the reciprocal of one another.

The close connexion between the scales of C major and A minor is familiar, and is emphasized by the fact that the combination of the two gives the chromatic scale. The author discusses the usual method of developing this scale, namely, by means of successive tempered fifths.

Either the rising or the falling harmony may be employed for the analysis of music; but Professor Goldschmidt finds it more convenient to adhere to one, the rising harmony, even for pieces composed in a minor key. He analyses and discusses in detail seven examples selected from the works of classical composers. To illustrate the method, we will give his analysis of Haydn's familiar hymn.



Gott er - hal - te Franz den Kai - ser un - sern gu - ten Kai - ser Franz.

Gott er - hal - te	Franz den Kai - ser	unsern guten	Kai -ser Franz
g a b a	c b a $f^\#$ g	e d c b	a b g d
b d g $f^\#$	a g c c b	c b $f^\#$ g	e e e d
g . . .	d d $f^\#$ a g	. . . a b	. g g $f^\#$
g . . .	$f^\#$ g d d g	. . . d g	c $c^\#$ $c^\#$ d
$0\frac{1}{3}$ 01 $0\frac{2}{3}$ $\frac{1}{3}1$	$0\frac{1}{3}13$ $0\frac{1}{3}1$ $0\frac{1}{3}13$ $0\frac{1}{3}13$ $0\frac{1}{3}$	$0\frac{1}{3}$ $\frac{1}{3}1$ $0\frac{1}{3}13$ $0\frac{1}{3}$	$0\frac{1}{3}2$ $0\frac{1}{3}2$ $0\frac{1}{3}$ $0\frac{1}{3}$
g d g d	d g d d g	c g	c g g d
$\underbrace{\begin{matrix} g & d \\ 0 & 1 \end{matrix}}$		$\underbrace{\begin{matrix} g & c & d \\ 0 & \frac{1}{2} & 1 \end{matrix}}$	
g			

No single cipher occurs which exceeds those included in the third normal row. The importance of the note ($p=3$) in this selection may be noticed.

The next section deals with the physiological and psychological grounds for the harmony of tones. Our knowledge of this interesting subject has been enormously advanced by the classical researches of Helmholtz, whose great work *Lehre von den Tonempfindungen* marks an epoch. It is still, however, uncertain how the sensation is conveyed to the brain, or by what means an educated ear is able to distinguish almost minute differences of pitch. Helmholtz considered that the fibres of Corti's organ correspond in a manner to the wires of a pianoforte: others, among them

Professor Goldschmidt, do not altogether agree with him and think that, numerous as these fibres are, they are not so numberless as to account satisfactorily for the minute gradations which are perceptible; the author believes the drum of the ear to play a very important part. There can be no doubt that the appreciation of musical tones depends wholly on the brain, which can, indeed, act quite independently of the organ of hearing; for do we not think, even dream, of tones?

The second chapter is taken up with the Harmony of Colours, and is analogous to the first chapter, but with some important modifications necessitated by the dissimilarity of the organs by means of which our senses are affected. Since the eye, unlike the ear, has a range of less than a single octave, and, if unaided, is incapable of resolving a composite wave into its constituent parts, there is nothing connected with light which corresponds to music; and the discussion must, therefore, proceed on different lines. The extent of our knowledge of the constitution of matter has been vastly increased by the discovery of the spectroscope. Much of what it has shown us we understand; but there still remains much which is not yet explained. Many writers have endeavoured to trace a relation between the spectral lines of the same elementary substance, and also between the different elementary spectra, but as yet with little success. If Professor Goldschmidt's principle of complication reveals the existence of some such periodicity in the elementary spectra, and determines for each the fundamental line which corresponds to the tonic in a musical scale, a great step will have been made towards the elucidation of the phenomena. In the present work the author, however, merely deals with the fringe of the subject; he shows that the Fraunhofer lines in the solar spectrum, with the exception of G, form a series of the falling harmony type, and so do the lines in the spectrum of hydrogen. Possibly he intends to discuss the subject at greater length in a separate paper.

The remaining sections of this chapter deal successively with the various theories of colour-vision which have been put forward, especially that of Young and Helmholtz, and the evolution of colour-vision in Man and its development in children.

In the concluding chapter it is briefly shown that the symmetry involved in the law of complication is discernible in the characters of the lower organisms, in corals and in plants; and further, that the methods of subdividing units of length, weights, and measures, and systems of money, all conform to the same principle.

We have already mentioned that the later paper, "*Über harmonische Analyse von Musikstücken*," is an expansion of the section in the earlier work treating of the Harmony of Tones, though the discussion is a little different. Since a knowledge of crystallography is not among the ordinary qualifications of musicians, the author has wisely based the discussion on the diatonic scale, and not on the development of faces on a crystal. He here shows that the transformations from the wave-lengths

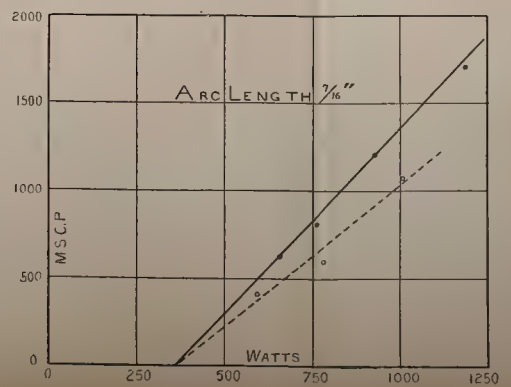
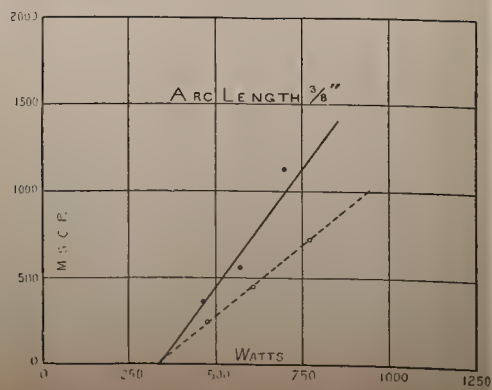
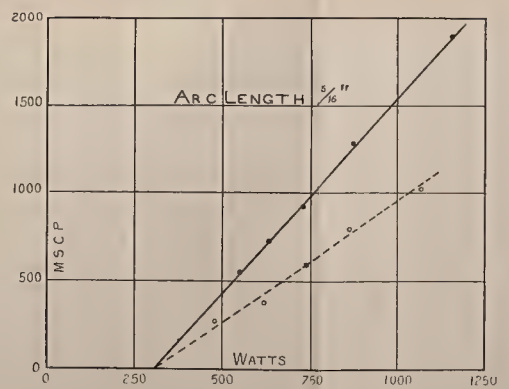
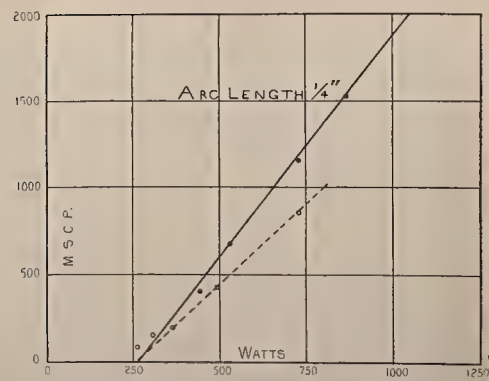
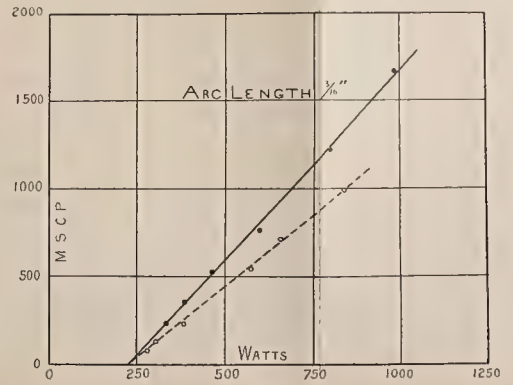
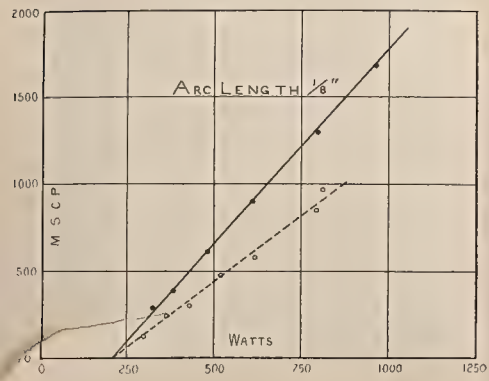
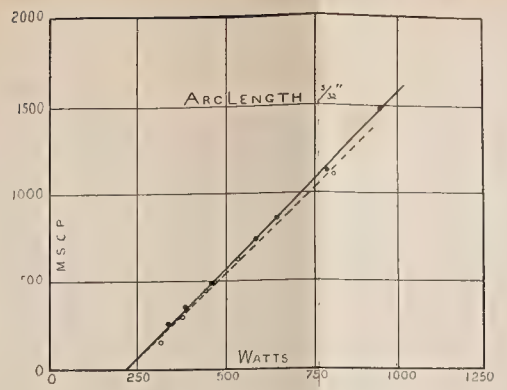
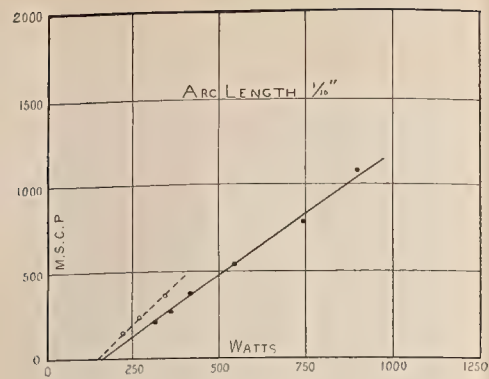
and the vibrations to the series-numbers have a real physical significance, and are not merely juggling with numbers. Thus $\bar{p} = \frac{l-1}{2-l}$ is the ratio of the two portions into which half a stretched string is divided by the node corresponding to the particular harmonic, and p is double of the inverse of this ratio. Musicians should find the author's method of analysing music of great practical value, since it enables the construction of the whole composition to be visible almost at a glance. The notation is simplified by the employment of letters to represent the principal chords; e. g., d for the major (Dur) and m for the minor (Moll) chord. To illustrate the method, Beethoven's "Die Ehre Gottes" is analysed in the minutest detail.

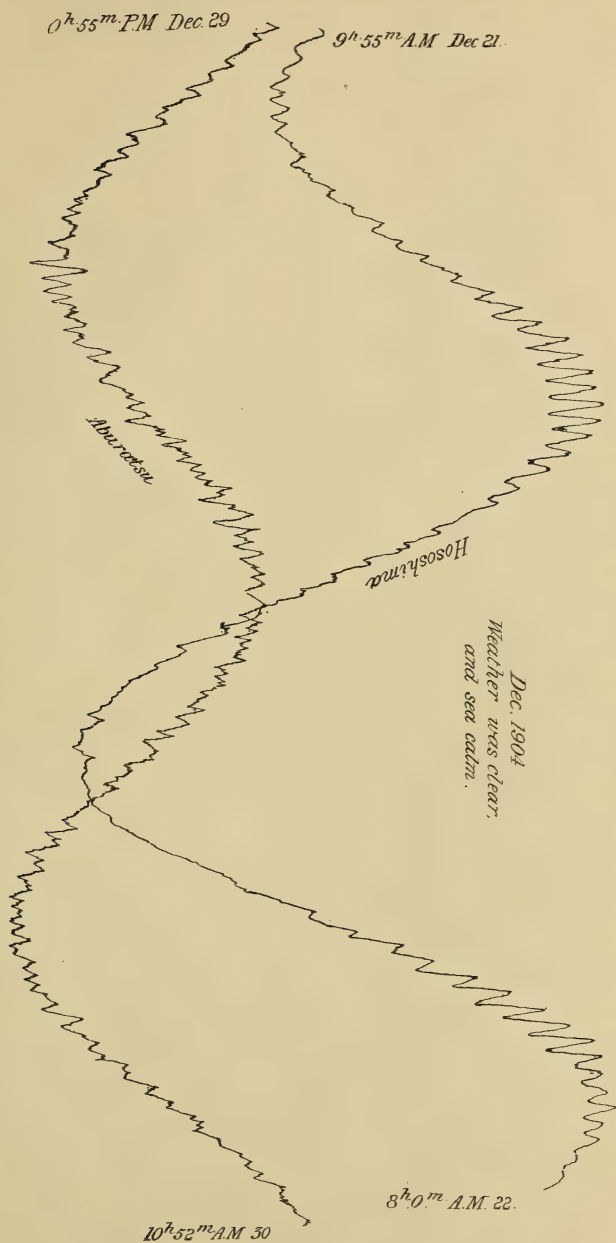
Professor Goldschmidt has undertaken a great task and has acquitted himself with conspicuous ability. The principle which he has enunciated and discussed has already attracted no small attention among continental writers. A new theory not only coordinates, and throws fresh light on, facts already known, but opens out vistas of fields for further research. We anticipate, therefore, that "Über harmonische Analyse von Musikstücken" is only the first of a series of papers in which the author discusses in detail various aspects of this fascinating subject.

Die Formelzeichen. Ein Beitrag zur Lösung der Frage der algebraischen Bezeichnung der physikalischen, technischen und chemischen Grössen. Von OLOF LINDERS. Leipzig: Jäh & Schunke. 1905. Pp. 96.

THERE can be no doubt that the subject of notation is one of growing importance in science, and the desirability of an international system is hardly open to question. The saving of time and worry which would result from the adoption of such a system would be considerable. Efforts in this direction have been made before, and although a few international symbols, such as π , B, H, μ , &c., have become firmly established, an extension of the somewhat meagre list is to be fervently desired. One cannot help admiring the patience of the author in drawing up a table of symbols for no fewer than 871 physical and chemical quantities, giving their dimensions in the C.G.S. system, and their proposed symbols, not only according to his own system, but also according to twelve other authorities. In many cases, the columns allotted to the latter are blanks. In his own system of notation, the author presses into service the Latin, German, Greek, and Russian alphabets. The author's effort is deserving of all praise, and we should be doing him an injustice did we omit to mention that he by no means regards his own proposals as final, but only as tentative and forming a contribution towards the subject; he clearly realises that the time is not yet ripe for the definite adoption of an international system of notation, and that such a step should only be decided upon after very careful consideration and free and thorough discussion of the various systems proposed.

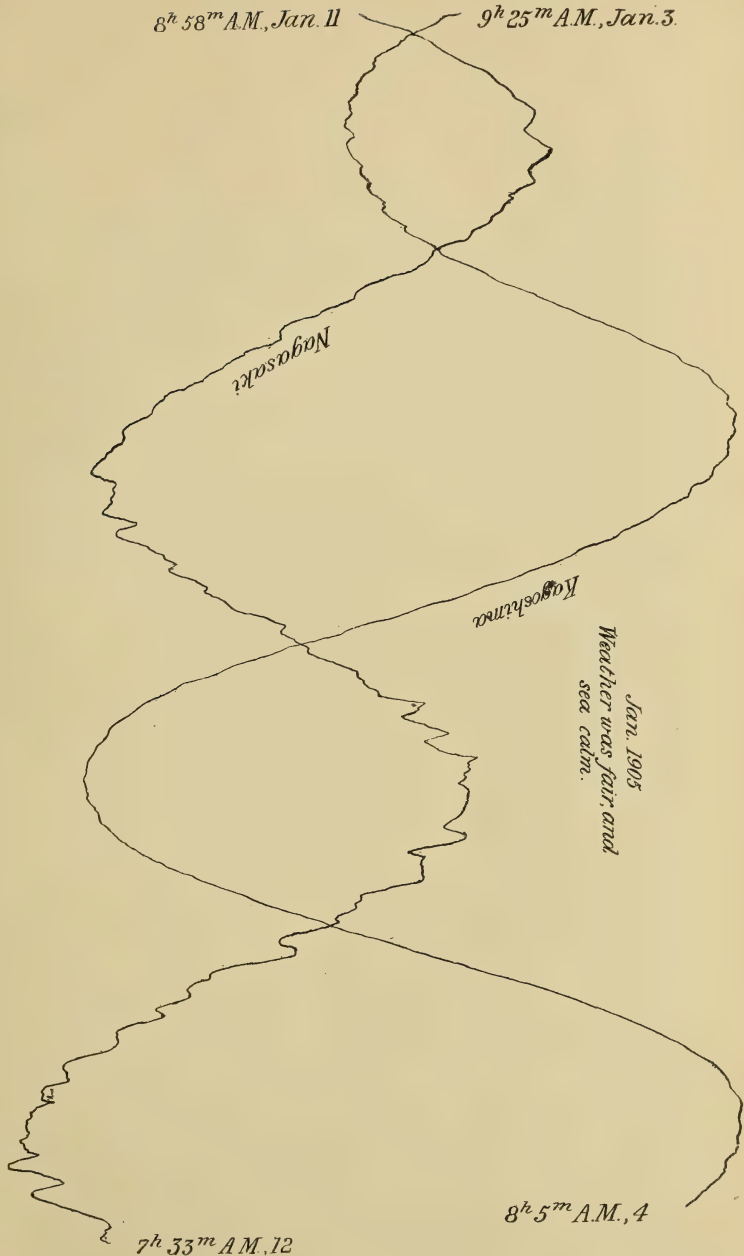






8^h 58^m A.M., Jan. 11

9^h 25^m A.M., Jan. 3.



INDEXED.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[SIXTH SERIES.]

SEPTEMBER 1905.

XXXVI. *On the Statistical Kinetic Equilibrium of Ether in Ponderable Matter, at any Temperature.* By Lord KELVIN*.

§ 1. CONSIDER first the simplest possible case, a piece of solid matter of a few millimetres or a few centimetres greatest diameter, placed in space at the earth's distance from the sun, say 150 million kilometres; for particular example, let us suppose two globes of metal, or rock, or glass, or the bulbs of two thermometers, of a centimetre diameter, one of them coated with black cloth and the other with white cloth, side by side, at a distance of a few centimetres or metres asunder. For the most extreme simplification, suppose no other matter in the universe than ether, the sun, and our test globes. From our knowledge of the properties of matter, it is obvious that each of our test globes will, in a few minutes of time, come to a steady temperature. In these circumstances, each globe sends out by radiation as much energy of waves travelling out through ether, as it takes in from the sun; after it has been long enough exposed to come to a steady temperature.

§ 2. The internal mechanism in each globe consists of atoms of ponderable matter, with ether permeating through the whole volume of the globe, and locally condensed and rarified in the space around the centre of each atom; as I

* Communicated by the Author, having been despatched for Section A of the British Association at Capetown.

have assumed, with explanations, in §§ 162, 163, 164 of pp. 412, 413, and in § 3 of pp. 487, 488 of my volume of Baltimore Lectures.

§ 3. The action of this mechanism in our case under consideration, involves the communication of energy from incident waves of sunlight to the atoms of the solid, in the surface of the hemisphere illuminated by the sun; and the communication of energy from the atoms to ether outside the globe, in the form of waves travelling out *in all* directions from the surface of the globe. The travelling of this energy through the volume of the globe is carried on according to the laws of the conduction of heat through solids; modified, but scarcely perceptibly modified, by convection currents in the case in which the globe is the bulb of a mercury thermometer.

§ 4. Our present knowledge of the radiational properties of matter does not quite suffice to let us pronounce for certain, which of the two globes will have the higher steady temperature: as this depends, not only on the well-known higher receptivity of the black surface than of the white for sun-heat; but also, on the difference of radiational emissivities of all parts of the two surfaces to surrounding space. It seems most probable that the black globe will be steadily warmer than the white; but we cannot say with certainty that this is true. Suppose for a moment that the steady temperatures of the two are the same; and now whiten the hemisphere of the black globe remote from the sun. This will cause the globe which is now black and white, to be warmer than it was, because it will radiate less into void ether than it did when it was all black.

§ 5. Now blacken the hemisphere facing from the sun, of the globe originally all white. Its temperature obviously will be lowered. Thus we have, side by side, two globes each with a white hemisphere and a black hemisphere; facing respectively towards and from the sun. The globe of which the black hemisphere is towards the sun, will certainly be warmer than the other, when a few minutes of time has been given for the temperature of each to become steady.

§ 6. It is not possible for a human experimenter to attain to the extreme simplicity ideally prescribed in §§ 1-5 above. But it has occurred to me (and probably to many others) that instructive experiments might be made by observing the temperatures of two equal and similar thermometers, placed beside one another on a wooden table (or on two similar tables of the same materials) or on a cushion or layer of very fine cotton wool: each thermometer between the folds of a doubled sheet; one of white cloth and the other of black; both

exposed in the open air under sunlight, or under the light of a more or less cloudy sky, or under moonlight or starlight, or in the darkest attainable cellar.

§ 7. Not being at present able to undertake experiments of the kind, I asked Dr. Glazebrook a few weeks ago if he could conveniently allow some such experiments to be made under the auspices of the National Physical Laboratory. He kindly consented, and asked Dr. Chree to commence an investigation of the kind. I have to-day (28th July, 1905) received the annexed description of his work, and statement of results.

§ 8. It is very interesting to see in Dr. Chree's results how large are the differences in the temperatures of the thermometers under black and under white cloth, ranging from $\cdot 5^{\circ}$ to $\cdot 6^{\circ}$ cent., even at times when the sky is covered with dark clouds; and how comparatively moderate are the differences ranging from $1^{\circ}\cdot 1$ to $3^{\circ}\cdot 6$ cent., at times of exposure to direct sunshine.

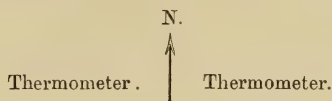
§ 9. Returning to § 4 with one of the globes black over its whole surface and the other white: suppose the two to be taken to 1000 times the earth's distance from the sun; and suppose, all at about the same distance (for simplicity of calculation), 999 stars, each equal to our own sun, to be scattered through space, round the place of our ideal experiment. The total of radiational energy coming from all these suns to the place of observation per unit area, will be one one-thousandth of the amount coming from our own sun in the case of §§ 1, 2, 3; and the difference of steady temperature between the white globe and the black globe may be about one one-thousandth of that which it would be in §§ 1, 2, 3. This last arrangement would be somewhat similar to an exposure to starlight on a cloudless night, at the top of a high mountain of our earth, with two or three polished silver screens between the tested globes and the mountain top. It does not, however, seem probable that any differences of temperature will be perceptible on the two thermometers exposed only to stellar radiation from the sky. Even less of difference may be expected when the two thermometers are placed in the darkest attainable cellar. The bolometric method would of course be much more sensitive than the comparison of two ordinary thermometers: even of the most extreme sensibility: and it will, I think, be worth while to try it in cases in which the thermometric method fails, or almost fails, to show any difference between the temperatures in the two cases.

*Dr. Chree's Report on Temperatures of Thermometers
under Black Cloth and under White Cloth.*

National Physical Laboratory
(Kew Observatory Department),
Richmond, 27th July, 1905.

*Experiments with two ordinary Thermometers,
Nos. 1184 & 1207.*

THERMOMETERS placed horizontally on small stands fastened to the outside window-sill of the wooden room on the Observatory roof, some 40 feet above the general level of the ground. The arrangement was as shown diagrammatically.



The north side of this upper room is entirely in the shade until well on in the afternoon. The cloths were wrapped several times round the bulbs and a small adjacent part of the stems. The thermometers were interchanged and the cloths were interchanged to eliminate difference between position and between thermometers. Readings were taken at intervals throughout two days. In every single case the (corrected) reading was higher for the thermometer with the black cloth. The conditions at the time were noted either as (a) bright sunshine, (b) sun shining through cloud, (c) cloudy, (d) black cloud, *i. e.* dark generally.

An analysis appears in the following sheets. The readings are all centigrade, corrected for scale errors.

Woollen Cloths.

JULY 12.	POSITION ON RIGHT.		POSITION ON LEFT.		Excess of Reading under Black			
	Black Cloth.		White Cloth.		Cloth, conditions being:			
Time.	No. 1184.	No. 1207.	No. 1184.	No. 1207.	(a)	(b)	(c)	(d)
11.0	23.9	...	...	22.7	1.2			
20	...	25.8	23.4	...	...	...	2.4	
40	26.2	...	...	23.8	2.4			
12.0	...	26.8	24.6	...	2.2	o		
20	25.5	...	...	24.5	...	...	1.0	
40	...	25.0	24.1	...	...	...	0.9	
2.0	25.6	...	...	24.8	...	...	0.8	
10	...	26.3	25.3	...	...	1.0		
20	26.5	...	...	25.8	...	...	0.7	
30	...	25.8	24.9	...	...	...	0.9	
40	26.4	...	...	25.2	...	1.2		
50	...	27.4	25.6	...	1.8			

Cloths changed over.

JULY 12. POSITION ON RIGHT.		POSITION ON LEFT.		Excess of Reading under Black Cloth, conditions being:				
White Cloth.		Black Cloth.						
Time.	No. 1184.	No. 1207.	No. 1184.	No. 1207.	(a)	(b)	(c)	(d)
3.0	25.1	...	...	26.2	1.1	...	...	...
10	...	24.8	26.1	...	...	1.3	...	...
20	25.1	...	...	26.3	1.2	...	...	...
30	...	25.3	26.2	...	...	...	0.9	...
40	25.6	...	...	26.1	...	...	...	0.5
50	...	24.5	26.1	...	1.6	...	...	...
4.0	25.0	...	...	26.2	1.2	...	...	...
10	...	24.5	25.5	...	...	...	1.0	...

Cotton Cloths.

JULY 13.		POSITION ON RIGHT.		POSITION ON LEFT.		Excess of Reading under Black Cloth, conditions being:			
		White Cloth.		Black Cloth.					
Time.	No. 1184.	No. 1207.	No. 1184.	No. 1207.	(a)	(b)	(c)	(d)	
11.0	...	24.6	25.3	...	...	0.7			
15	23.5	...	...	24.8	1.3				
30	...	23.0	23.7	...	0.7				
45	23.6	...	...	24.8	1.2				
55	...	23.6	24.5	...	...	...	0.9		
12.10	23.2	...	...	24.4	1.2				
20	...	23.3	24.2	...	...	...	0.9		
30	24.3	...	...	24.9	...	...	...	0.6	
40	...	24.3	25.1	...	...	...	0.8		

Changed Positions of Cloths.

JULY 13. POSITION ON RIGHT.			POSITION ON LEFT.		Excess of Reading under Black			
	Black Cloth.		White Cloth.		Cloth, conditions being:			
Time.	No. 1184.	No. 1207.	No. 1184.	No. 1207.	(a)	(b)	(c)	(d)
	○	○	○	○	○			
2.10	27.0	...	...	24.2	2.8			
20	...	26.0	25.2	...	...	...	0.8	
35	27.3	...	...	25.5	1.8			
45	...	26.6	25.3	...	1.3			
55	26.7	...	...	25.6	...	1.1		
3.5	...	25.5	24.9	...	...	...		0.6
15	26.4	...	...	25.3	1.1			
25	...	26.9	25.6	...	1.3			
35	27.0	...	...	25.8	1.2			

Observations made on a stand in the Garden about 4 feet above ground. No shade.

Woollen Cloths.

JULY 18. POSITION ON RIGHT.		POSITION ON LEFT.		Excess of Reading under Black Cloth, conditions being:			
Black Cloth.		White Cloth.					
Time.	No. 1184.	No. 1207.	No. 1184.	No. 1207.	(a)	(b)	(c)
11.50	28.2	...	...	25.9	2.3	...	...
12.0	...	27.1	25.6	...	...	1.5	...
10	25.9	...	...	24.6	...	...	1.3
20	...	25.6	24.1	...	...	...	1.5
30	24.7	...	...	23.2	...	...	1.5
White Cloth.		Black Cloth.					
2.50	24.8	...	...	25.5	...	0.7	...
3.0	...	24.6	25.6	...	1.0	...	...
10	24.9	...	...	26.6	1.7	...	...
20	...	24.4	25.3	...	...	...	0.9
30	24.2	...	...	25.8	1.6	...	...

Cotton Cloths.

JULY 18. POSITION ON RIGHT.			POSITION ON LEFT.		Excess of Reading under Black		
Black Cloth.			White Cloth.		Cloth, conditions being:		
Time.	No. 1184.	No. 1207.	No. 1184.	No. 1207.	(a)	(b)	(c)
3.40	29.1	...	...	25.8	3.3		
50	...	26.9	25.5	...	...	1.4	
4.0	27.1	...	...	25.4	1.7		
10	...	26.0	24.6	...	...		1.4
	White Cloth.		Black Cloth.				
4.20	...	24.4	26.3	...	...	1.9	
30	24.4	...	...	28.0	3.6		
40	...	24.3	26.2	...	...		1.9
50	24.9	...	...	26.2	1.3		

Aix-les-Bains, Savoie, Aug. 16, 1905.

P.S.—I have made some rough experiments in this place, about 250 metres above the sea-level; with two small dismantled bath-thermometers hung side by side from a horizontal bar in an open window about 12 metres above the ground. The thermometers were double coated; one with black silk, the other with white cotton, round the bulb and up to about 17° cent. of the scale. The black was always warmer during daylight. The greatest difference which I have hitherto observed in the course of eight days was this morning, $37^{\circ}6 - 30^{\circ} = 7^{\circ}6$ in bright sunshine. This was with air freely circulating round the two bulbs. In a special experiment with the two thermometers laid side by side on a slab of red blotting-paper, in bright sunshine, the black-coated one ran rapidly up to above 40° (the end of its scale), and had to be removed to escape breakage, as it had no safety space above the top of its tube. The white-coated thermometer did not rise as high as 40° .

KELVIN.

XXXVII. *Slow Transformation Products of Radium.* By E. RUTHERFORD, F.R.S., Macdonald Professor of Physics, McGill University, Montreal*.

IN a previous paper (Phil. Mag. Nov. 1904) I described experiments made to elucidate the changes occurring in the active deposit of slow rate of transformation, which is left behind on a substance after exposure to the radium emanation. I showed that this active deposit contained two distinct substances, called radium D and radium E, the latter of which arose from the transformation of the former. The product radium D gave out only β rays, while radium E gave out only α rays.

* Communicated by the Author.

At the time of publication a sufficient interval had not elapsed to experimentally determine the rate of decay of the activity of these substances, but it was calculated from purely radioactive data that radium D should be half transformed in about 40 years, and radium E in about one year.

The present paper contains an account of further experiments upon the variation of activity of the different products with time, and of the isolation of a new product which was previously overlooked.

If a body is left for some time in the presence of the radium emanation, the activity after removal, measured by the α , β or γ rays, rapidly decays with the time. At the end of 24 hours, the products radium A, B, and C, deposited on the plate, have been almost completely transformed. There then remains a small residual activity, which comprises both α and β rays. The magnitude of this residual activity depends upon the quantity of the emanation, and upon the time of exposure, but is, in general, of the order of one millionth of the activity observed immediately after removal.

It was shown that the α ray activity of the body was small at first, but increased nearly proportionately with the time over a period of two months. In another set of experiments, it was shown that the α ray activity was still increasing after standing for nine months.

The β ray activity was examined about a month after removal, and was then found to remain sensibly constant for a further period of nine months. It was considered advisable to see whether the β ray activity remained constant during the first few weeks after removal. For this purpose, the active deposit was obtained on a platinum plate exposed to a large quantity of radium emanation for 3.8 days. Observations were begun on the β ray activity 24 hours after removal. The activity was measured by placing the plate under an electroscope, the base of which was covered with a sheet of aluminium of sufficient thickness to absorb all the α rays.

It was found that the β ray activity was small at first, but increased with the time, reaching a practical maximum about 40 days later. The results are shown graphically in fig. 1, where the ordinates represent the activity, and the abscissæ time in days. The time was reckoned from the middle of the time of exposure to the emanation. The first observation was, in consequence, made after an interval of 2.9 days. If the curve is produced backwards to the origin, its shape is very similar to the recovery curves of UrX and of other radioactive products, and the activity I_t at any time t can be

expressed by an equation of the form

$$\frac{I}{I_0} = 1 - e^{-\lambda t}$$

where I_0 is the maximum activity. The activity reaches half

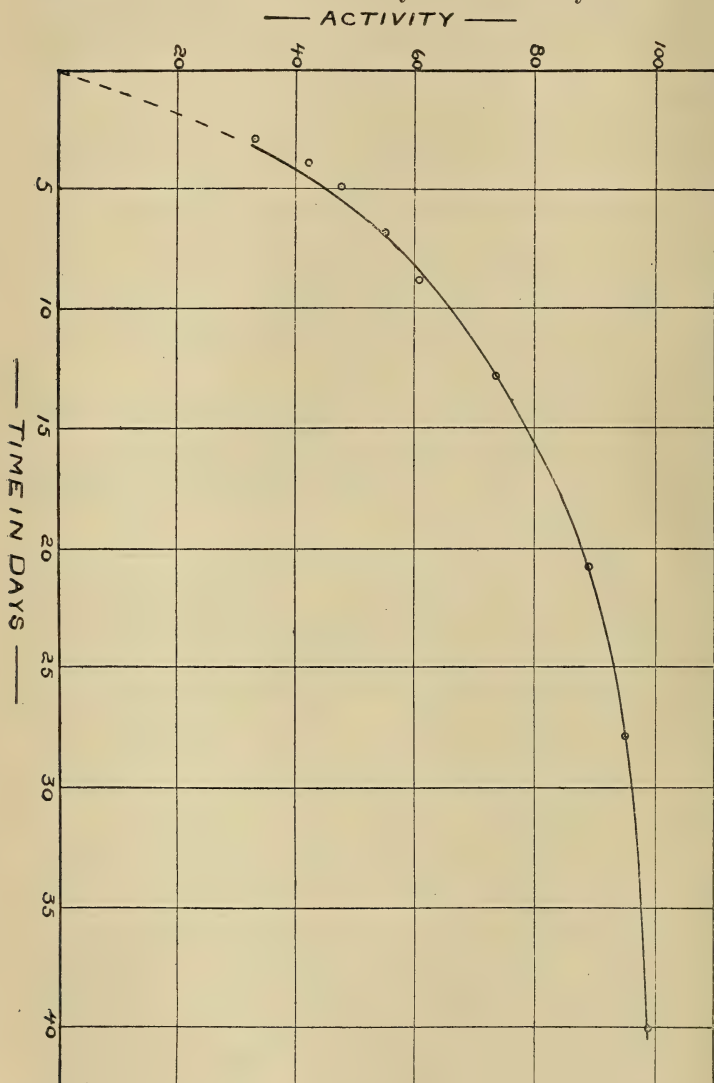


Fig. 1.

its final value in about six days. The value of λ is given by $\cdot 115 \text{ (day)}^{-1}$. A recovery curve of this character indicates

that the β ray activity arises from a product which is half transformed in six days, and which is supplied at a constant rate from a primary source, the latter being transformed at a very slow rate. The results indicate that the primary source itself does not emit β rays, but that these arise from the successive product. We have already seen that the α ray activity, tested immediately after removal, is very small. We may thus conclude that the primary matter is "rayless," that is, it does not emit either α or β rays, but that these arise from the products of its transformation.

In conformity with the nomenclature adopted in the previous paper (*loc. cit.*) this rayless product will be called radium D. The β ray product which arises from it will be called radium E. The α ray product (previously termed radium E) will be called radium F, for we shall see later that the β ray product is the parent of the α ray product.

We have seen that the increase of the α ray activity to a maximum indicates the existence of a product which emits β rays, and which is half-transformed in about six days. We shall now consider a method by which this product radium E can be isolated, and its rate of transformation directly measured.

In my previous paper it was shown that if a platinum plate, coated with the active deposit, were subjected to a temperature of about 1000°C ., the α ray activity was nearly all lost, while the β ray activity remained unaffected. On examining the platinum plate about two months later, it was found that the β ray activity had in the meantime decayed to a small fraction of its original value. This at once suggested a possible means of separation of radium E. A platinum plate, containing an active deposit two months old, was heated in an electric furnace for four minutes at about 1000°C . On testing it, the loss of the α ray activity showed that the product radium F had been mostly volatilized. The β ray activity, as before, was not immediately altered by the heating, but was found to lose its activity with the time.

The activity decreased to about one quarter of the maximum value and then remained constant. Subtracting this constant value, the activity was found to decrease exponentially with the time, falling to half value in about 4.5 days. This result shows that not only most of the radium F, but also the greater part of the parent substance radium D, had been volatilized at or below 1000°C . Radium E was left behind, and, since most of the parent matter had been removed, immediately commenced to lose its activity. The constant activity which remained behind was due to the fact that some of the radium

D on the plate had not been volatilized. It is seen that the transformation periods, deduced from the recovery curve (6 days), and from the decay curve (4.5 days), are somewhat different. I do not think this difference in the periods can be ascribed to experimental errors, but is more probably due to an alteration of the rate of transformation of radium E, resulting from the high temperature to which it has been subjected. A similar effect of temperature on the rate of transformation of radium C has been noted by Curie and Danne. It is intended to continue these experiments in order to settle definitely the cause of this difference in the period obtained by the two methods.

The period deduced from the recovery curve is more likely to be the correct value under normal conditions.

This method of separation is of interest, and shows in a striking way how the difference in physical properties of the products can be utilized to effect a partial or complete isolation of a substance which is mixed with several others.

The volatility of radium D, as well as of radium F, has also been noted in other experiments, for it has been found that the α ray activity of a platinum plate after exposure to a temperature of 1000°C . does not increase with time to the same extent as that of a similar plate which has not been so treated. Such a result is to be expected if a large part of the parent matter radium D is removed by the heating.

The experiment of heating the platinum plate was also utilized to prove that radium E was the parent of the α ray product radium F. The α ray activity of the plate was determined immediately after heating, and at subsequent intervals. The increase of activity of the plate with time after heating is shown in fig. 2.

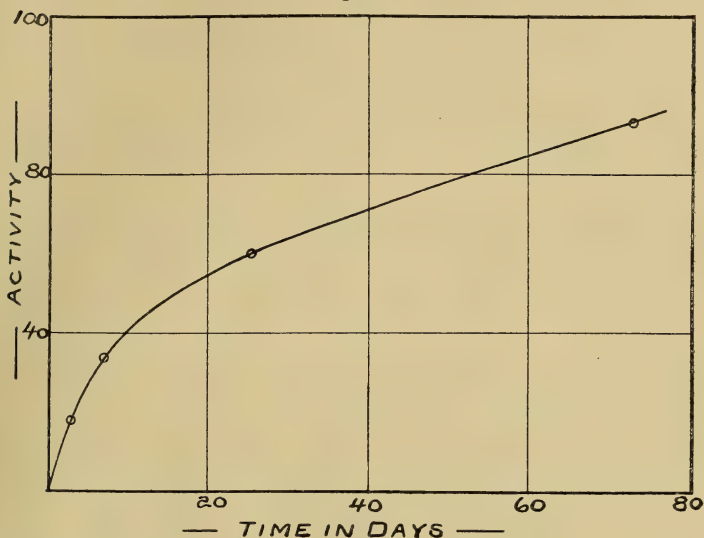
The activity rose far more rapidly during the first two weeks than for equal intervals later. The slow period of increase is due to the production of radium F from the radium E, which is continuously produced from the fraction of radium D which was not volatilized. The rapid initial increase of the activity is at once explained if the excess of radium E changes into F.

We may then conclude that E is the parent of F, and that the three products D, E, F are successive, for we have previously shown that E is produced from D.

In the previous paper it was calculated that radium D should be half transformed in about 40 years. This result still holds good, although it has been found that radium D itself does not emit rays. About 40 days after removal, the β ray activity of radium E is constant, and the number of

β particles expelled from it per second is then a measure of the number of atoms of radium D which break up per second.

Fig. 2.



Thus the activity of the successive product, when in equilibrium with the parent substance, can be utilized to determine the period of a substance which itself does not emit rays.

For convenience, the physical and chemical properties of the three radium products are tabulated below. It will be shown in the next section that radium F is half transformed in 143 days. It is probable that radium E emits γ rays as well as β rays, but the intensity of the former in the experiments has been too small to measure.

Active deposit of radium of slow transformation.	Radiations.	Time to be half transformed.	Some physical and chemical properties of the product.
Radium D.....	no rays.	40 years.	Soluble in strong acids, not deposited on bismuth, volatile below 1000° C.
Radium E.....	β (and γ ?).	6 days.	Non-volatile at 1000° C., soluble in acids, not deposited on bismuth.
Radium F.....	α .	143 days.	Volatile at 1000° C., deposited on bismuth, soluble in acids.

Decay of the Activity of Radium F.

The product radium F is deposited on a bismuth plate from a solution of the active deposit. The surface of such a plate, which has been left for some hours in the solution, becomes strongly active. The activity consists only of α rays, the β rays being altogether absent. In this way the α ray product is almost completely removed. Radium D is not deposited on the plate, for the solution, deprived in this way of radium F, at once commences to grow a new supply of radium F.

The solution of the active deposit was obtained in the following way:—The emanation from 30 milligrams of radium bromide was condensed in a glass tube, which was then sealed. After standing for a month the tube was opened, and dilute sulphuric acid introduced. This almost completely dissolved the active deposit on the glass. The solution was evaporated to dryness in a glass vessel, and allowed to stand undisturbed for nine months. During this time a large quantity of radium F was produced by the primary active substance radium D. The deposit was then again dissolved in sulphuric acid, and three bismuth disks were successively introduced into the solution. These three disks, coated with radium F, were then placed aside, and the α -ray activity examined at intervals over a period of about 9 months. The activity of each disk was found to decay according to an exponential law with the time. The results are shown graphically in fig. 3, where the abscissæ represent days, and the ordinates the logarithm of the activity in arbitrary units. It is seen that the points lie nearly on a straight line, showing that the activity decreases according to an exponential law. The curves of the activity of the three disks are marked radium F (I.), radium F (II.), radium F (III.). F (I.) was found to decay to half value in 150 days, F (II.) in 136 days, F (III.) in 142 days. The mean value is 143 days.

We may thus conclude that the activity of radium F decays to half value in about 143 days.

The initial rate of decay of two of the curves was somewhat slower than the final value. This may possibly be due to the fact that a small quantity of radium D was also deposited on the bismuth plate. This would produce more radium F, so that the apparent rate of decay would be somewhat less than the true rate of decay of radium F alone.

The observations of the rate of decay of the bismuth disks were made with a specially designed electroscope, which was standardized at the time of each observation with the aid of a constant sample of uranium.

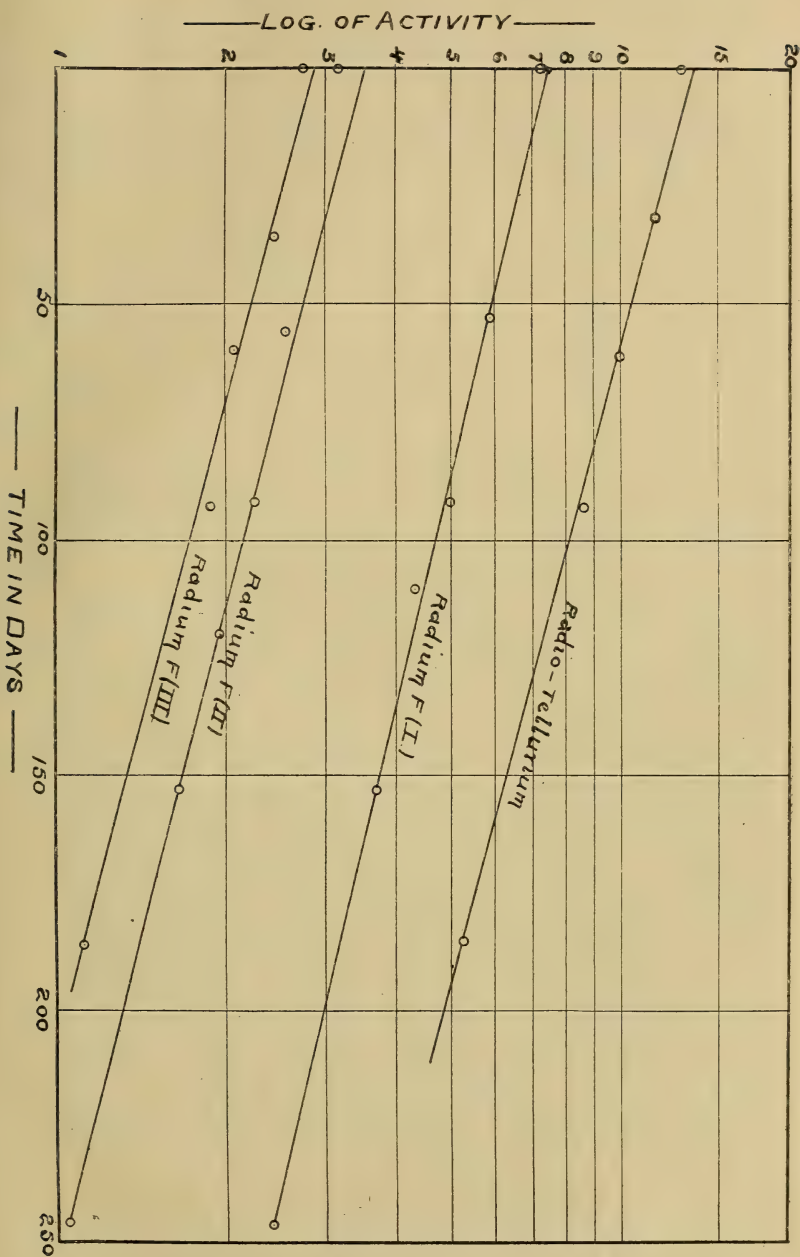


Fig. 3.

Rise of the α Ray Activity of the Active Deposit.

It was shown in the previous paper that the α ray activity of the active deposit increased for the first 60 days at a nearly uniform rate. Observations of the α ray activity of a platinum plate, coated with the active deposit, have been continued for a period of nine months. The results are shown in the table below.

Time in days.	α ray activity in arbitrary units.
9	0.127
43	0.57
135	1.44
163	1.65
196	1.85
290	2.17

The results are shown diagrammatically in fig. 4. The activity is still increasing, but not nearly so rapid as at first. Observations have also been made on an active deposit 15 months old. The activity is still increasing, but slowly, and is obviously tending towards a maximum value.

The curve of fig. 4, as far as observations have gone, is very similar in shape to the usual recovery curve, in which half the final activity is reached in 143 days. Such a result is to be expected since radium F is transformed far more rapidly than radium D.

From a comparison of the experimental curve with the theory discussed below, it can be deduced that the activity should finally reach the value 2.90. The activity after 290 days has thus reached 75 per cent. of the maximum value.

The full explanation of the rise of the α ray activity involves the theory of three successive changes, since the substance initially deposited, radium D, changes into radium E, and radium E into radium F, and the latter substance alone gives out α rays. Since, however, radium E (half transformed in six days) has a very short period compared with radium F (half transformed in 143 days), for the purpose of calculation the intermediate change may be neglected, and radium D may be supposed to change at once into radium F.

If λ_1 , λ_3 are the constants of change of radium D and radium F respectively, then the number q of atoms of radium F present at any time t is given by

$$q = \frac{n_0 \lambda_1}{\lambda_3 - \lambda_1} (e^{-\lambda_1 t} - e^{-\lambda_3 t}),$$

where n_0 is the number of atoms of D originally deposited.

The α ray activity at any time is proportional to q , and a maximum value is reached at a time T , when

$$\lambda_1 e^{-\lambda_1 T} = \lambda_3 e^{-\lambda_3 t}.$$

Since D is half transformed in 40 years, $\lambda_1 = \cdot 0173 \text{ (year)}^{-1}$, and since radium F is half transformed in 143 days, $\lambda = 1\cdot77 \text{ (year)}^{-1}$.

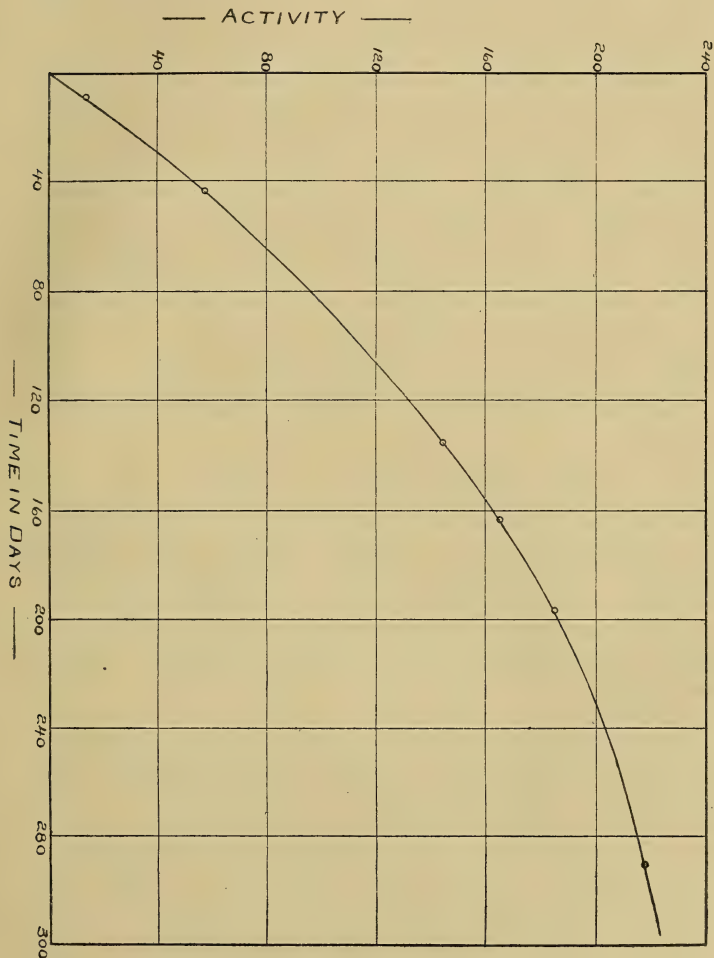


Fig. 4.

A maximum will be reached after a period of about 2.7 years. The activity will then diminish exponentially with the time according to the period of radium D, that is, it will

be half transformed in 40 years. The activity 180 years after the active deposit is formed will be about the same as that observed after an interval of 9 days.

Origin of Polonium and Radio-Tellurium.

Contemporaneously with the experiments on the rate of decay of radium F deposited on a bismuth plate, observations were made on the decay of activity of a specimen of the radio-tellurium of Marckwald, which had been obtained from Dr. Sthamer of Hamburg. The activity was found to decay exponentially with the time, falling to half value in 143 days. During these experiments, results of a similar character upon the decay of activity of radio-tellurium were published by Meyer and Schweidler*, and by Marckwald†. The former experimenters found that the activity fell to half value in 135 days, while Marckwald obtained a corresponding value of 139 days. Considering the difficulty of making accurate experiments of the rate of decay over long periods of time, the numbers obtained by Meyer and Schweidler, Marckwald, and the writer are in remarkably good agreement.

We may thus conclude that radio-tellurium loses its activity according to an exponential law, falling to half value in about 140 days.

In my previous paper I pointed out that there were strong reasons for believing that the active constituent in radio-tellurium was identical with the product radium F. Both active substances possess similar physical and chemical properties. Both only give out α rays, and both are deposited on bismuth from the active solution. In addition, I showed that the α rays from radio-tellurium and radium F were identical in their power of penetrating matter. Such a result afforded very strong evidence of the identity of the two products.

We have seen that radium F loses half its activity in 143 days—a value agreeing closely with the rate of decay observed for radio-tellurium. The agreement of physical and chemical properties, coupled with the identity of the radioactive constant λ , conclusively shows that the active constituent in radio-tellurium is identical with radium F. We may thus conclude that the active constituent present in radio-tellurium is in reality a transformation product of radium, and that the radio-tellurium obtained from radioactive minerals is

* *Wien. Ber.* Dec. 1, 1904.

† *Ber. d. D. Chem. Ges.* No. 2, p. 591 (1905).

derived from the decomposition of the radium contained in them.

The amount of radio-tellurium to be extracted from the radioactive minerals will always be proportional to their content of radium, and, in consequence, also to the content of uranium, for the investigations of Boltwood and others have conclusively shown that the amount of radium in radioactive minerals bears a constant ratio to the content of uranium.

The relative activity of radium and radium F, and also the relative amount present in radioactive minerals can easily be deduced. Let N_1 be the number of atoms of radium F in one gram of radioactive mineral, and N_2 the corresponding number of radium atoms. Let λ_1 , λ_2 be the constants of change of radium F and radium respectively. In an old radioactive mineral, the amount of radium and radium F have reached a steady state and the same number of atoms of each break up per second. Thus

$$N_1\lambda_1 = N_2\lambda_2.$$

Now radium F is half transformed in .38 years, and radium in about 1200 years. Thus

$$\frac{N_1}{N_2} = \frac{.38}{1200} = 3.2 \times 10^{-4}.$$

Now it is probable that the atomic weight of radium and radium F are not very different. Consequently the weight of radium F in the mineral is .00032 the weight of radium. Now corresponding to each gram of uranium in a radioactive mineral, there is about one millionth of a gram of radium. Consequently, from a ton of mineral, which has an average content of 50 per cent. of uranium, the amount of radium F present is 0.14 milligram. Assuming that the α particles expelled from radium itself and from radium F produce about the same amount of ionization in the gas, the activity of radium F in the pure state will be 3200 times the activity of pure radium at its minimum activity, or 800 times the activity of radium in radioactive equilibrium.

Marckwald* has worked up 5 tons of uranium residues, corresponding to 15 tons of the Joachimsthal mineral, in order to extract the radio-tellurium (radium F) from it. Simple but very efficient methods of separation were devised, and

* *Ber. d. D. Chem. Ges.* No. 2, p. 591 (1905).

he finally succeeded in obtaining 3 milligrams of intensely active material.

Since the mineral contains about 50 per cent. of uranium, the theoretical yield is about 2.1 milligrams. It is not likely, however, that the total amount would be separated, so that the three milligrams obtained probably contain some impurity. The enormous activity of this substance so prepared has been pointed out by Marckwald. A copper plate, which had been coated with about 1/100 of a milligram of the material by dipping it in a solution of the active substance, lighted up a zinc sulphide screen to a sufficient extent to be clearly visible in a dark room at 15 metres distance.

The results deduced from theoretical considerations are thus in harmony with the experimental results.

I have previously pointed out that there were strong reasons for supposing that polonium and radio-tellurium contained the same radioactive constituent. The most direct test of this point is a comparison of their rates of decay. Mme. Curie has given some results in this direction. A sample of polonium nitrate lost half its activity in 11 months, and 95 per cent. in 33 months. A sample of the metal lost 67 per cent. in six months.

These results are not at all concordant. The sample of metal loses half its activity somewhat faster than radium F, while the nitrate at first loses it much more slowly. In addition, the activity does not decrease according to an exponential law with the time. The results indicate that the polonium nitrate contained more than one constituent. I think it is not improbable that this second constituent is radium D. The presence of some of this substance, which gives rise to radium F, would at first cause the α ray activity to decrease more slowly than the normal rate when only radium F is present.

I had in my possession a sample of polonium about three years old. During that time it had lost the greater proportion of its activity, but, on testing it, the activity was found to have reached a small and nearly constant value. However, from rough observations which I had made from time to time in the interval, the activity decayed to half value in about six months, but the observations were not sufficiently accurate to more than indicate the probable period of the decay of the activity.

More accurate determinations of the decay of activity of polonium are required before the point can be considered definitely settled; but I think there can be no reasonable

doubt that the α ray constituent in polonium is the same as in radio-tellurium, and is identical with the product radium F.

The presence of radium D in polonium could be determined by testing if it emitted any β rays. From the method of separation of polonium from the mineral, it is not improbable that some radium D would be removed with it.

Radium D and Radio-Lead.

In my previous paper it was suggested that radium D was the primary constituent in the radio-lead separated from pitchblende by Hofmann. This conclusion has been very strongly confirmed by recent investigations. I have examined a specimen of radio-lead, kindly separated for me by Dr. Boltwood of New Haven. This specimen was four months old at the time of testing. The β ray activity has not appreciably changed in the course of the following six months, but the α ray activity has been steadily increasing. This is exactly the result to be expected if it contains radium D. The product radium E would be in radioactive equilibrium at the time of testing, so that the β ray activity would remain constant. Since radium F is not continuously produced, the α ray activity should increase with time for several years.

Hofmann has observed that the activity of the radio-lead prepared by him did not appreciably decay with the time, indicating the presence of a slow transformation substance. In some recent experiments, Hofmann, Gonder, and Wölf* have made a careful chemical examination of radio-lead, and have shown the presence of two distinct radioactive constituents, which are probably identical with radium E and F. The radioactive measurements were unfortunately not very precise, and the periods of change of the separated products have not been very closely examined.

Experiments were first made by them on the effect of adding substances to a solution of radio-lead and then removing them by precipitation. Small quantities of the platinum metals in the form of chlorides were left in the solution for several weeks, and then precipitated by formalin or hydroxylamine. All of these substances after separation were found to give out both α and β rays. A large proportion of the β ray activity disappeared in the course of six weeks, and of the α ray activity in one year. The β ray activity is probably

* *Annal. d. Phys.* **v.** p. 615 (1904).

due to radium E, which is half transformed in six days, and the α ray activity to radium F.

This conclusion is further confirmed by experiments on the effect of heat on the activity of these substances. By heating to a full red-heat, the α ray activity was lost in a few seconds. This is in agreement with my experiments on radium F, which is volatilized at about 1000°C .

Salts of gold, silver, and mercury added to the radio-lead were found to show only α ray activity on removal. This is explained by supposing that radium F alone is removed with these substances. Bismuth salts, on the other hand, showed initially both α and β ray activity, but the latter rapidly died away. This is in agreement with the experiments of Mme. Curie, who found that radioactive bismuth at the moment of separation possessed both α and β ray activity, but that the latter soon disappeared.

The α and β ray activity of the radio-lead is much reduced by the precipitation of bismuth added to the solution, but recovers itself again with time. This result is exactly what is to be expected if radio-lead contains radium D, E, and F. Radium E and F are removed with the bismuth, but the parent substance radium D is left behind, and in consequence a fresh supply of radium E and F is produced.

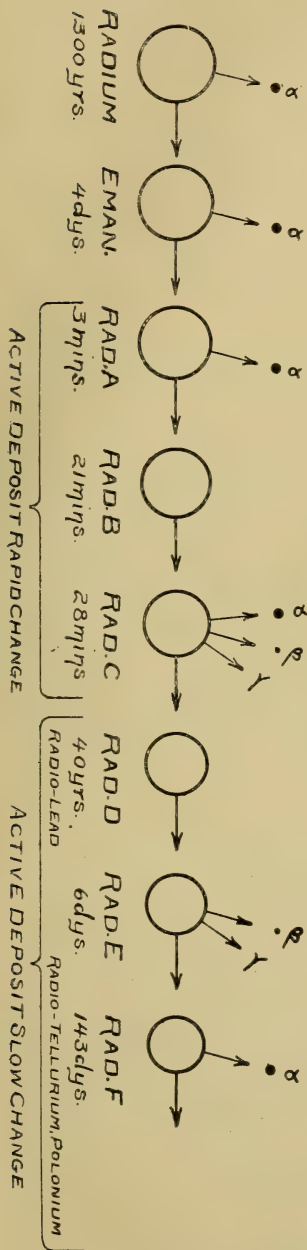
While further experiment is required to settle conclusively whether the products separated from radio-lead are identical with radium E and F, there can be little doubt that such is the case.

The evidence at present is not sufficient to decide whether the lead, immediately after its separation from pitchblende, contains only radium D, or whether radium E also appears with it. It seems likely, however, that the bismuth which is present in the solution with the lead will retain both radium E and F, and that the presence of these products in old radio-lead is due to their production after separation by the parent substance radium D.

The results of the comparison of the products of radium with those contained in polonium, radio-tellurium, and radio-lead are summarized below.

Products in old Radio- lead.	{	Radium D=product in <i>new radio-lead</i> , no rays. Half transformed in 40 years.
		Radium E=gives out β rays, separated with bismuth, and iridium. Half transformed in 6 days.
		Radium F=product in polonium and radio- tellurium. Gives out only α rays. Half transformed in 143 days.

Fig. 5.



The family of substances produced by the disintegration of radium, together with the time for each to be half transformed, is shown diagrammatically in fig. 5.

It is now fully established by the researches of Boltwood, Strutt, and McCoy that the amount of radium present in radioactive minerals always bears a constant ratio to the amount of uranium. The investigations of Boltwood, in particular, have shown a surprisingly good agreement between the content of radium and uranium for minerals obtained from various localities, which differ very widely in their content of uranium. This proportionality is a strong indication that radium is produced from uranium; and a conclusive proof of this point of view is given by the experiments of Soddy and Whetham, who find that there is a slow growth of radium in uranium which was initially freed from radium. In addition, the actual amount of radium in radioactive minerals is of the right order of magnitude to be expected from theoretical considerations, if uranium is the parent of radium.

Soddy finds that the present growth of radium from uranium is only a very small fraction of the theoretical amount. This is most simply explained by supposing that one or more products of slow period of transformation intervene between UrX and radium. The uranium-radium family and their connexion with

one another is summarized below.

Uranium.

↓
UrX.

↓
?

↓
Radium and its family of rapidly-changing products, viz., the emanation, radium A, B, and C.

↓
Radium D=primary constituent in radio-lead.

↓
Radium E.

↓
Radium F=active constituent in radio-tellurium and polonium.

No evidence has been obtained that any further active products exist after radium F has been transformed. If the α particle is a helium atom, remembering that five products are present in radium which emit α particles, the atomic weight of the transformation product of radium F should be $225 - 20$ or 205 . This is very close to the atomic weight of lead, 206.7 . The view that lead is the final or end product of the transformation of radium is supported by the fact that lead is always found in the radioactive minerals in about the amount to be theoretically expected from the content of uranium, when the quantity of helium, present in the mineral, is used to compute its probable age*. A similar suggestion has recently been advanced by Boltwood†.

McGill University, Montreal,
May 1, 1905.

XXXVIII. *Deductions from Magnetic Disturbances at Greenwich.* By C. CHREE, *Sc.D., LL.D., F.R.S.*‡

§ 1. **I**N three recent papers in the Monthly Notices§ of the Royal Astronomical Society, Mr. E. W. Maunder has discussed the phenomena of magnetic storms at Greenwich. In the first paper he concluded that magnetic storms tend to follow one another at an interval of 27.275 days. In a review of this paper in the March number of 'Terrestrial Magnetism,' p. 12, I pointed out that "With the S storms

* A full discussion of this question was given by the writer in the Silliman Lectures, Yale University, March 1905.

† Phil. Mag. April 1905.

‡ Communicated by the Author.

§ November 1904, April 1905, May 1905.

(i. e. those of sudden commencement)...the irregularities in the hourly distribution shown...might be fortuitous. But of the ...not-S storms no less than ...71 per cent. are ascribed to the 8 hours noon to 7 P.M., and there is a most pronounced maximum at 1 P.M.”; adding: “If the phenomenon is a true physical one it indicates a very dominant influence which has nothing to do with the Sun’s rotation period, and which would have to be dealt with in any complete theory, or in any estimate of probabilities.”

The question of the diurnal variation of magnetic disturbances has been treated from a variety of standpoints. Some, e. g. Sabine, have taken a disturbance as existing at any hour when the observed value of a magnetic element has differed by more than a certain amount from the mean monthly value for that hour, and have treated positive and negative disturbances separately, finding diurnal variations for each as well as for the two combined. Others have taken the difference between the mean hourly values for all days in a month and those for a few specially quiet days, considering the amplitude of the departure at any given hour as an estimate of the tendency to disturbance at that hour. Van Bemmelen* has specially considered the incidence of the tiny undulatory movements not unusual at times when large movements are altogether absent.

In all cases “disturbances” have shown a diurnal variation, but the times of maxima and minima have varied according to the particular standpoint adopted, and for any given definition of disturbance different results have been obtained at different parts of the Earth. A good deal of information on these points is given in well-known treatises, such as Balfour Stewart’s article in the 9th Edition of the *Encyclopædia Britannica* and Mascart’s *Magnétisme Terrestre*.

There was thus nothing surprising in the existence of a diurnal inequality in the figures of Mr. Maunder’s Table I.; but some features of the inequality, especially the suddenness of the rise to a maximum at 1 P.M., led me to suspect that the phenomena were not wholly due to Nature. Accordingly, on the appearance of Mr. Maunder’s second paper I examined its figures with interest, and found that they also showed a conspicuous diurnal inequality, but that it differed markedly in some details from the inequality I had found in the first paper. Meantime Mr. Maunder read the third paper referred to above, in which he too recognized the existence of a diurnal inequality, and also noticed the difference between his first two papers. Having seen a

* *Natuurkundig Tijdschrift voor Nederlandsch-Indië*, 1902, p. 71.

reference to the fact in the 'Observatory,' I waited to see exactly what his conclusions were. Having now read the paper, I find that there are so many points on which I differ from Mr. Maunder, both as regards the facts and as regards the inferences to be drawn, that it has seemed worth while to put my views on record.

§ 2. In Table XIV. of his third paper, Mr. Maunder analyses the times of commencement of magnetic storms, treating separately 1882 to 1903 * (the period dealt with in his first paper) and 1848 to 1881 (the period of his second paper). As the corresponding table which I had formed agrees substantially with Mr. Maunder's, I omit it and pass to the following Table I. For brevity it gives only the total number of disturbances for the twelve hours 0 to 11 A.M., but it subdivides Mr. Maunder's two periods of years to bring out certain points. Five storms to which no hour of commencement was assigned are omitted.

TABLE I.

Numbers of disturbances commencing at hours stated.

Years.	0 to 11 A.M.	Noon.	1	2	3	4	5	6	7	8	9	10	11	Total.
1848-58	22	7	6	8	9	15	14	12	14	6	9	5	5	132
1859-69	37	3	9	13	14	11	17	24	16	10	8	8	10	180
1870-81	37	2	7	5	7	13	11	9	13	10	7	3	9	133
1882-92	42	6	20	17	12	11	6	11	14	6	6	3	5	159
1893-1903	29	17	20	12	9	8	4	4	2	4	3	3	2	117
1848-81	96	12	22	26	30	39	42	45	43	26	24	16	24	445
1882-1903	71	23	40	29	21	19	10	15	16	10	9	6	7	276
1848-1903	167	35	62	55	51	58	52	60	59	36	33	22	31	721
Percentages of Totals.														
1848-81	22	58								20				
1882-1903	26	63								12				
1848-1903	23	60								17				

Mr. Maunder's explanation (*l. c.* p. 670) of the striking difference between the results for the periods 1882 to 1903 and 1848 to 1881 is that "In (his) Table I. (disturbance data from 1882 to 1903) the times were taken from the reproductions... in the plates of the Greenwich volumes, and these in the majority of cases began with Greenwich noon; hence the

* There are a few trifling differences between Mr. Maunder's hourly figures and those which I had previously given in 'Terrestrial Magnetism,' due presumably to difference of choice made in cases where the commencement was ascribed to an exact half-hour.

fluctuations of the early afternoon more frequently caught the attention." This suggests a more casual inspection than even a severe critic would have ventured to suggest, especially considering that the times were given to 0.1 hour in the case of all the storms having a notably sudden commencement. Also under the conditions mentioned the hours principally overlooked ought surely to have been the *forenoon* hours. But, as my Table I. shows, the percentage of occurrences between 0 and 11 A.M. is not conspicuously less, but even absolutely greater, for the period 1882 to 1903 than the period 1848 to 1881. Where the period 1882 to 1903 actually shows a relatively smaller number of commencements is in the late afternoon and evening hours. The chief difference between the two periods consists in an alteration in the incidence during the afternoon. It is also clear from my analysis that there is a marked difference between the first and second halves of the period 1882 to 1903, though one would infer that the whole period was supposed by Mr. Maunder to have been treated in a uniform way.

§ 3. In discussing the diurnal inequality shown by his data for 1848 to 1881 (which agree substantially with those in my Table I.), Mr. Maunder says (*l. c.* p. 667) "Table IX. rises, with a regularity which precludes the possibility of accident, to a most unmistakable maximum at 18 hours (6 P.M.)." This is surely not an accurate description of the facts. There seems a very poorly defined maximum at 6 P.M., a sort of high level plateau extending from about 4 to 7 P.M., with a sharp drop at 8 P.M. Also of the three sub-periods into which 1848 to 1881 is divided in Table I., only one gives a distinct maximum at 6 P.M.

To explain apparently how this supposed maximum at 6 P.M. comes about, Mr. Maunder in his Table XV. gives the "hourly distribution of small wave-movements, 1894-5." The nature of these movements is not clearly indicated, but as they are referred to on p. 668 as "isolated" they are presumably not the regular wave-movements considered by van Bemmelen. The diurnal variation in Table XV is certainly very different from that assigned by van Bemmelen (*l. c.* p. 81) for the regular wave-movements at Kew for the year 1897. However this may be, I cannot regard as satisfactory Mr. Maunder's explanation that in getting out the times of commencement of magnetic storms the eye was caught by the movements whose incidence he records in his Table XV. What that table shows is a high frequency of easterly movements from 5 to 10 P.M., and a less but considerable concentration of westerly movements from 0 to 3 A.M.

Grouping Mr. Maunder's numbers, we have

	Easterly.	Westerly.	Total.	Average per hour.
0 to 11 A.M. (12 hours).....	12	59	71	6
Noon to 7 P.M. (8 hours) ...	89	3	92	11½
8 to 11 P.M. (4 hours)	61	9	70	17½

The average per hour between noon and 7 P.M. is certainly greater than between 0 and 11 A.M.; but it is just as conspicuously less than the average from 8 to 11 p.m., a period when Mr. Maunder's times of commencement of storms are relatively few.

§ 4. The next point where I differ from Mr. Maunder is as regards his conclusion (*l. c.* p. 669) that in his tables "The 'great' disturbances are distributed throughout the 24 hours with almost complete impartiality." As some importance attaches to this point in connexion with the theory of disturbances I shall discuss it in some detail.

In Mr. Maunder's first paper, storms were divided into four classes, G, V, A, M, in descending order of magnitude, and to each storm only one letter was attached. In his second paper he combined the two first classes into one, called G, and when a storm lasted over two or more days a letter was attached to each day. I have adopted the three classes of Mr. Maunder's second paper; but have attached to a single storm, however long it lasted, only one letter, that letter corresponding to the most violent part of the storm. It must, I think, be conceded that this is the proper choice of letter if the disturbance be assumed all one storm.

Table II. shows what the incidence of the commencements really was for each class of storm, the number occurring in the 24 hours being taken in each case as 100.

TABLE II.—Actual percentage incidence of storms (for normal incidence 0 to 11 A.M., 50; noon to 7 P.M., 33; 8 to 11 P.M., 17).

	Midnight to 11 A.M.			Noon to 7 P.M.			8 to 11 P.M.		
	G	A	M	G	A	M	G	A	M
Period 1848-81 ...	34	27	16	57	52	62	9	21	22
„ 1882-1903	39	28	19	49	61	70	11	11	12
„ 1848-1903	37	27	17	53	56	64	10	17	18

Table II. shows, what Mr. Maunder already admits, that the A ("active") and still more the M ("moderate") storms show a marked concentration of commencements between noon and 7 P.M.; but it further shows a similar though less conspicuous concentration of the G ("great") storms. In fact, in the period 1848-81 the percentage of G storms commencing between noon and 7 P.M. is greater than that of the A storms*.

§ 5. There is also reason to think that *part* of the apparent difference between the different classes of storms arises from a cause wholly unrecognized by Mr. Maunder.

In Tables I. and II. it has been tacitly assumed that there is no essential difference between one year and another, but this is far from the case. In years of many sun-spots, whilst the amplitude of the regular diurnal changes of the magnetic elements in England seems enhanced at all hours, the increase seems relatively greater by night than by day: the distinction between day and night is thus less marked than usual. As we shall presently see, a similar phenomenon presents itself as regards the times of commencement of magnetic storms, the diurnal incidence being notably more regular in years of many than in years of few sun-spots. We shall also show that the distribution of G, A, and M storms is widely different in years of many and of few sun-spots. In years of the latter type G storms are exceedingly rare, and so the majority of storms of this class are taken from the years of sun-spot maximum; *i. e.* from years which, as has already been stated, show an exceptionally regular incidence of commencements. I now proceed to prove these statements, dealing with the latter first.

The 56 years of Mr. Maunder's investigation included the following 14 of exceptionally large sun-spot frequency:—

1848, 49, 59, 60, 61, 70, 71, 72, 82, 83, 84, 92, 93, 94.

This group I shall refer to as S max. years.

Amongst the 56 years the following 15 were of exceptionally small sun-spot frequency:—

1854, 55, 56, 65, 66, 67, 77, 78, 79, 88, 89, 90, 99, 1900, 1901.

This group I shall refer to as S min. years.

* Mr. Maunder particularizes storms of sudden commencement only for 1882 to 1903. According to my figures in 'Terrestrial Magnetism,' the percentage distribution of these was:—0 to 11 A.M., 47; noon to 7 P.M., 40; and 8 to 11 P.M., 13 (Mr. Maunder's figures would give 44, 40, and 16 respectively). This *suggests* a concentration between noon and 7 P.M. even for sudden storms.

Table III. gives the number* of storms of the three classes in each of these groups of years, and the percentage which they form of the total number of storms of the same class in the whole 56 years.

TABLE III.

Class of Storm.	S max. years.		S min. years.	
	Number.	Percentage.	Number.	Percentage.
G	62	57	7	6
A	82	37	31	14
M	112	29	88	23
All classes.....	256	36	126	17

We see from Table III. that the numbers of A storms from the two groups of years are comparatively nearly in the proportion one would anticipate from the total numbers; but the G storms are relatively few in years of S min., and are so numerous in years of S max. as to form considerably more than half of the total from the whole 56 years.

§ 6. The data on which the assertion was based that the diurnal incidence is different in years of S max. and S min. are presented in Table IV. It gives the number of the storms commencing in the three portions of the day already employed in Tables I. and II., and the percentages these form of the whole number occurring in the 24 hours. Results are given for the M storms separately, as well as for all the storms irrespective of size.

TABLE IV.

Group of Years.	Numbers.						Percentages.					
	0 to 11 A.M.		Noon to 7 P.M.		8 to 11 P.M.		0 to 11 A.M.		Noon to 7 P.M.		8 to 11 P.M.	
	All	M	All	M	All	M	All	M	All	M	All	M
S max.....	73	24	134	65	49	23	29	21	52	58	19	21
S min.....	24	12	86	66	16	10	19	14	68	75	13	11

* Omitting three whose times of commencement are not given, owing to imperfect registration at Greenwich.

Whether we consider all the storms, or only the M storms in Table IV., we see a notable difference of the kind already indicated between S max. and S min. years. If the difference had existed between the "All" storms, but not between the M storms, then of course it would naturally have been suggested that the difference was solely a matter of the absolute size of the average storm in the two groups of years; but the fact that the difference is conspicuous when we confine ourselves to M storms renders this position untenable. The G and A storms are so few in the S min. years that comparative data based on them are too uncertain, but the M storms number 88 in this group and 112 in the S max. group; so that the difference shown in Table IV. can hardly be assigned in any large measure to chance.

We have now shown (Table IV.) that the diurnal incidence of commencements is notably more uniform in years of S max. than in years of S min., and (Table III.) that an unduly large proportion of the G storms come from the S max. years. It is thus clear that a difference of the kind shown in Table II. between storms of the three classes would naturally arise, even if the size of the storm were absolutely without effect on the hour of commencement or its estimate.

At the same time, it should be clearly understood that our investigation has not shown that the difference of behaviour in S max. and S min. years is the *sole* cause of the phenomena appearing in Table II. In fact it suggests that there is a real difference depending on the size of the disturbance; for Table IV. shows in both groups of years a greater concentration of M than of "All" storms between noon and 7 P.M., and in the case at least of the S max. group the number of G and A storms is sufficient to render chance an improbable explanation.

§ 7. The difference between S max. and S min. years extends, as will be presently shown, to the annual distribution, a subject to which Mr. Maunder also refers in his third paper. Before dealing with this, I might explain that the tendency to a maximum near the equinoxes in the number of disturbances, to which Mr. Maunder refers on his pp. 680-1, has long been known to occur at certain stations, including Toronto and Greenwich—whose long series of results have been so fully dealt with by Mr. Ellis; but other stations of whose data Mr. Maunder seems unaware show different phenomena. Some of the older results are discussed in Balfour Stewart's article in the *Encyclopædia Britannica*, and some more recent results are given by Mascart in his *Magnétisme Terrestre* (pp. 296 *et seq.*).

§ 8. Table V. advances the evidence of the statement made above, that the annual incidence of storms differs in years of many and of few sun-spots. The data are the times assigned for the commencement of the 726 storms included in Mr. Maunder's two lists. The year is divided into three seasons: Winter (November to February), Equinox (March, April, September, October), and Summer (May to August); the monthly results are given in the order mentioned, *i. e.*, month 1 denotes November, March, or May, according as the season is Winter, Equinox, or Summer. In the case of the whole 56 years the results for the M storms are given as well as for the "All" storms. In the case of the S max. and S min. years—which represent the same years as in Tables III. and IV.—only the results for "All" storms are given.

TABLE V.

Month.	WINTER.				EQUINOX.				SUMMER.			
	56 years.		S max.	S min.	56 years.		S max.	S min.	56 years.		S max.	S min.
	All.	M.			All.	M.			All.	M.		
1	56	27	24	7	74	48	21	19	46	27	10	12
2	40	21	15	2	75	40	28	9	40	18	16	6
3	59	37	18	14	75	43	21	11	45	24	21	3
4	78	33	34	13	83	40	28	22	55	34	22	9
Total ...	233	118	91	36	307	171	98	61	186	103	69	30
Percentage	32	30	35	28	42	44	38	48	26	26	27	24

The total number of storms is too small, especially in the S min. group of years, to give smooth results for an annual variation, and much weight cannot be assigned to the data for individual months. But taking the seasons, there seems an unmistakable difference between S max. and S min. years, at least in Winter and Equinox. The tendency in S max. years is obviously towards a more regular distribution of storms throughout the year. The phenomena are in short exactly analogous to what we have already seen to be true of the diurnal distribution. Also, as the difference between "All" and M storms from the 56 years is, if existent, exceedingly small, the difference between the S max. and

S. min. years can hardly be assigned in any large degree to the fact that M storms form a larger percentage of the whole in the latter than in the former group.

§ 9. Returning to the subject of the diurnal inequality, the conclusion to which our investigations point is that whilst Mr. Maunder's first paper may be, as he himself now maintains, somewhat affected by a systematic error in the estimate of times, the general agreement between it and his second paper affords overwhelming evidence that a marked diurnal inequality exists in the times of commencement of magnetic storms at Greenwich as estimated by an unprejudiced mind. The consequences of this are hardly, I think, adequately recognized by Mr. Maunder. The "local peculiarities," he says, "tend to blur the evidence for the Interval Relation;" but he adds, "they by no means efface it, for the whole of the evidence . . . in my two former papers . . . is still outstanding." But the certainty of the existence of a pronounced diurnal period in the figures, and the admission of uncertainty in the individual data, make the proof of the existence of a true 27 to 28 day period more difficult, and, supposing it to exist, increase the probable error in its estimate.

If the determining factor, as Mr. Maunder believes—and as others before him, *e. g.* Arrhenius and Birkeland, have supposed—be some form of electrical discharge from the sun, the time at which that leaves the sun or reaches the earth's atmosphere cannot well have anything to do with whether it is day or night at Greenwich. We must thus suppose that the effects at any given station often remain almost entirely latent, for a time determined by circumstances which if not local, in the ordinary sense of the word, have at least a marked relationship to local time. There is of course nothing very improbable in this, if we suppose that the emanation, or whatever it is that proceeds from the sun, is not the immediate cause of the disturbance, but only introduces conditions which facilitate the development in the atmosphere of electrical currents, similar to but irregularly distributed as compared with those to which the ordinary diurnal inequality is due*. But evidently if this be the case, the determination of the circumstances to which a particular disturbance is due is a less simple matter than would appear at first sight, and we cannot but view with increased reserve estimates such as that of Riccò—quoted by Arrhenius† in support of theoretical conclusions—of the time required for the supposed electrical discharge to travel from the sun to the earth.

* Phil. Trans. A, vol. ccii. p. 436.

† Terrestrial Magnetism, March 1905, p. 4.

§ 10. Supposing a period existent in disturbances, it is obviously desirable to determine it as exactly as possible, if only with a view to ascertaining the true source. At least two previous investigators had, unknown to Mr. Maunder, investigated the matter from practically the same standpoint. How closely similar Mr. Allan Broun's* views were may be seen from his paper in the *Phil. Trans.* for 1876, p. 400, or from the account given by Balfour Stewart in the *Enc. Brit.*; but the time which he deduced for the period from the Makerstoun disturbances was a day less than Mr. Maunder's estimate. So again it appears that the Toronto disturbances led Mr. Arthur Harvey† a year or two ago to conclusions almost identical with Broun's and Maunder's, but the period he deduced (27·246 days) differs slightly from Maunder's.

This all suggests the advantage of some more strictly mathematical method, whether the periodogram method of Prof. Schuster or another, which can not merely assign a value for a period, but also afford a measure of its probability. There is also obviously an advantage to be derived from having a variety of data. The horizontal force at least might well be treated, and quite independently of the declination, and the criteria for selecting disturbances should be varied. Even supposing a *fixed* minimum range limit assigned, it is at least highly doubtful whether Mr. Ellis's is ideal for an investigation of the kind. His limit is such that in the 22 years of Mr. Maunder's first paper the *average* interval between two successive storms was approximately 29·1 days, while in the 34 years of his second paper it was approximately 27·6 days. It does not seem desirable that the average interval should approach so closely to the period it is desired to investigate. There ought under the circumstances—period or no period—to be an exceptionally large number of cases in which storms follow one another at intervals of from 27 to 28 days.

Obviously if Mr. Ellis's lowest limit were much raised the number of storms would become so small as to render it difficult to recognize any 27 to 28 day period however real. Thus, if a fixed limit be adopted, the better alternative would be to lower ‡ Mr. Ellis's limit, though it might be necessary to

* Mr. Broun accepted apparently three periods, viz., 26, 27·3, and 29·5 days, as occurring in the small regular changes of horizontal force, attributing the former to the sun, the two latter to the moon. But he found distinct trace in the disturbances only of the 26-day period.

† *Trans. Can. Inst.* 1898-9, p. 345; *Roy. Ast. Soc. of Canada Selected Papers & Proc.* 1902-3, p. 74, 1904, p. xiv &c.

‡ In some months it is difficult to get more than 5 or 6 days free from very appreciable disturbance. Thus any large reduction in Mr. Ellis's qualifying limit would hardly be compatible with the application of his method.

make it a condition that the qualifying movement took place in a comparatively short definite interval of time, otherwise complications might arise from the large size of the regular diurnal range at certain seasons.

§ 11. There is at least theoretically a good deal to be said in favour of a limit varying in a definite way. In the average year the regular diurnal variation is much larger in some months than in others. In England the declination range in December is only about a third of what it is in several summer months. Again, the average range may be 40 or 50 per cent. larger for a year near sun-spot maximum than for a year near the adjacent sun-spot minimum. Thus in the course of an 11-year period the regular diurnal ranges in two individual months may bear a ratio such as 4 or 5 to 1. One cannot but doubt under such circumstances whether a system which defines a day as disturbed, and the disturbance as M, A, or G, by reference to an unvarying absolute scale of magnitude is a natural one. If the immediate cause alike of regular and irregular movements be electric currents in the upper atmosphere, whose intensity is increased in years of sun-spot maximum owing to the increased presence of some ionizing or otherwise effective agent, it would seem clearly better to take limits varying with the average diurnal range for the individual month considered. Even if the large disturbances have an absolutely distinct source, an allowance would seem desirable for the movement that would in the natural course of events have taken place between the times when the maximum and minimum were recorded.

§ 12. The considerations already advanced would suffice to raise doubts as to whether the energy of the source to which a particular storm is due can be supposed to answer in all cases to the letter assigned to the storm on Mr. Ellis's and Mr. Maunder's lists. But an even stronger reason for such doubts remains to be mentioned. On Mr. Ellis's, or any analogous system, a single to and fro movement *may* count as a disturbance, whereas a dozen movements occurring in close succession, of but slightly inferior amplitude, may fail to be classed. Again, disturbances in which the extreme range is the same may differ enormously in the number of the oscillatory movements and in the rapidity with which they take place. Thus a classification which takes account only of the extreme range must in many cases fail to arrange disturbances according to their real intensity. Consequently the conclusions which have been drawn as to differences between storms of different intensities might have to be modified if some more exact measure of energy were adopted.

July 10, 1905.

XXXIX. *On the α Particles of Radium, and their Loss of Range in passing through various Atoms and Molecules.* By W. H. BRAGG, M.A., Elder Professor of Mathematics and Physics in the University of Adelaide, and R. KLEEMAN, B.Sc., Demonstrator*.

IN previous papers which we have contributed to this Magazine† we have shown that the α particle moves always in a rectilinear course, spending its energy as it traverses atoms of matter, until its velocity becomes so small that it cannot ionize and there is in consequence no further evidence of its motion. Each α particle possesses therefore a definite range in a given medium, the length of which depends on the initial velocity of the particle and the nature of the medium. Moreover, the α particles of radium which is in radioactive equilibrium can be divided into four groups, each group being produced by one of the first four radioactive changes in which α particles are emitted. All the particles of any one group have the same range and the same initial velocity.

The present paper may be regarded as a continuation of the papers cited. Its contents are arranged under the following heads:—

1. Improvements in the apparatus used for measuring the ranges and relative strength of the four groups of rays.

2. Results of experiments with the new apparatus giving the following values of the ranges in air at a pressure of 76 cm. and a temperature of 20° C.

Radium	3·50 cm.
Emanation or } Radium A. }	 4·23
Radium A or } Emanation. }	 4·83
Radium C	7·06

These are correct, we believe, to ·05 cm. The pressure and temperature are stated, since a change of 1 cm. in the pressure or 5° C. in the temperature would produce an effect which could be observed. We have not yet determined which of the two middle groups belongs to the Emanation and which to Ra A; but we have constructed a special piece of apparatus which will, we hope, settle the point.

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† Phil. Mag. Dec. 1904.

The results also show that the four groups are alike in all respects save that of initial speed; and further, that the α particle spends its energy at a rate which is approximately inversely proportional to the square root of its speed.

3. Determinations of the loss of range of α particles in consequence of their passage through various substances, from which it appears that for all the materials examined the loss in traversing any atom is nearly proportional to the square root of the weight of that atom. The loss in the case of a complex molecule is proportional to the sum of the square roots of the weights of the constituent atoms.

4. Discussion of these results.

§ I.

In the apparatus which we first used for these experiments we found that our measurements were liable to considerable irregularities. The most troublesome of these was an occasional sudden and violent increase of the ionization current which would cause the electrometer spot to fly off the scale. This would occur in the middle of a period of satisfactory working. It appeared to be due to the liberation of emanation from the radium under observation, and we have therefore enclosed the whole of our apparatus in a metal vessel through which a current of dry air is made to pass continually.

In other respects the apparatus now used differs but little from that described on p. 728 of the *Phil. Mag.*, Dec. 1904: except that we have added an arrangement by which an aluminium screen can be drawn over the radium so as to cut off all α radiation, and we have made it possible to set the ionization-chamber at any desired depth. Both these arrangements can be worked from the outside of the enclosing vessel. It is necessary that the air which is drawn through should be fairly dry. In this country a difference of 30° F. between the wet and dry bulb thermometers is not unusual, and at these times artificial drying is hardly necessary; but we find it more satisfactory to draw the air at all times through a set of large calcium-chloride tubes, the current passing day and night during the progress of any series of experiments. The need for dryness appears to arise from the fact that moist air acts on the thin film of radium which we employ and liberates emanation.

At one time we endeavoured to secure the complete removal of any liberated emanation by passing the whole stream of air directly over the radium just before it left the vessel;

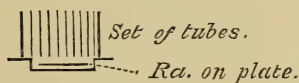
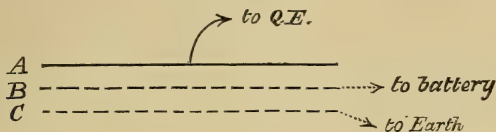
but with this arrangement the remaining moisture of the air seemed to do more harm in liberating emanation than the current did good by carrying it away. We now pass the current of dry air through the vessel from one end to the other; but the radium film probably feels little draught, being protected by the set of vertical metal tubes which stand over it (see the previous papers). When this is done the leak which the electrometer shows, the radium being out of range, is very small compared with the principal leak and is fairly constant. In the experiments described later it amounted usually to one or two millimetres in ten seconds, whilst the principal leak might be as much as sixteen centimetres in the same time.

This residual leak is due to various causes. One of them is the leak which takes place from the metal of the ionization-chamber, and the wire which connects it with the electrometer, to the earthed metal case which screens the chamber and its connexions from external influence. This current is supplied by the ions which are continually formed by various agents in the air within the earthed casing, and is driven by the Volta potential-differences between the metals of the connexions and the casing. It can be materially altered by changing the material of the casing, or part of it, *e. g.* exchanging zinc for copper. If a speck of radium is placed within the casing so as to increase the supply of ions the effect is surprising. As an illustration we allowed the electrometer connexions to end in a brass wire 15 cm. long, which projected horizontally in the open air, the ionization apparatus being disconnected. When a large earthed copper tube containing the radium speck was placed so that the brass wire was along its axis, the electrometer showed a large deflexion, whereas, when the copper was displaced by zinc, the deflexion was also large, but in the opposite direction.

The whole of the apparatus including the keys which connect the electrometer to earth or the ionization-chamber is enclosed in earthed metal casing. The keys are worked by electromagnets which are fixed externally to the casing, and operate on iron armatures within. We have found it necessary to be careful as to the contacts of these keys. Want of perfect contact causes incomplete charging or discharging of the electrometer quadrants, shown by kicks and irregular movements of the light-spot when the keys are used. We have obtained good results by making the contacts of platinum surfaces, so arranged as to rub each other at the make or break. This does not introduce any want of definition into the beginning or ending of a leak, since both

these times are determined by the breakings of contacts, and these are done by the electromagnets with great suddenness.

An arrangement which we find to be of great importance, is the ion-trap which is placed under the gauze of the ionization-chamber. The space below the gauze B, forming the lower plate of the ionization-chamber AB, is of course



filled with the ions formed by the rays which cross it. When the gauze C is not present, then although B is at a high potential, some of these ions manage to cross it and add themselves to those in the chamber AB, thus disturbing the results to be measured. We were first aware of this effect occurring in any marked degree when we began to use a platinum dish on which had been evaporated a few drops of water containing a speck of very pure radium bromide. We were so very fortunate as to be presented by Mr. Soddy with a milligramme or two of this material; by its use our experiments were very greatly improved, as will be described a little later. On the first occasion of its use, however, we were puzzled by a rather curious result. The radium and the tubes above it were rather close up to the gauze B. We were measuring the various leaks as, millimetre by millimetre, we pushed the radium still closer. Every now and then we drew the aluminium screen over the radium so as to discover how much of what was observed was really due to the α rays. We found that after the screen had been so used and withdrawn, the proper leak was materially lessened, but recovered its old value in a short time. When the screen was drawn over again, the leak (which was then not due to α rays) fell instantly to its usual very small value, and again, when the screen was withdrawn, the proper leak was for a time too small. We guessed therefore that some of the leak, with the screen off, was due to ions made outside the chamber, which afterwards drifted into it. The interposition of the aluminium

screen instantly cleared all the space between itself and the gauze B of ions, because the gauze was at a high potential and the screen was earthed. No more ions were made in this space until the screen was withdrawn. When this was done, the space just below the gauze and above where the screen had been was momentarily empty of ions, and though the continued passage of the α rays through it soon refilled it, yet there was a short time during which the ions in the space were below normal value, and therefore the drift upwards into the ionization-chamber was small. If this drift existed, it was certainly to be avoided, and we therefore placed below the gauze B a parallel gauze C about three millimetres away, which was put to earth. No ions could cross the powerful field which existed between the two gauzes. The second gauze cut off a number of α rays which would otherwise have entered the chamber, but there were still quite enough. With this new arrangement the irregularity described above has disappeared completely. Another irregularity has also disappeared at the same time; it was probably due to the same cause, and removed in the same way. It occurred when one or other of the bundles of rays was just out of range of the ionization-chamber, and took the form of a small leak which seemed to prelude the arrival of the real rays; it formed a slight projection on the ionization curve just above the true and large increase which represented the entrance of the rays of the bundle into the ionization-chamber, as the radium was pushed higher and nearer. It often blurred the corners of the ionization curves, and made it more difficult to determine the exact ranges of the rays.

The electrometer used in these experiments is of the Dolezalek pattern. It has been frequently calibrated, either by charging it and causing it to share its charge with an auxiliary condenser, or by measuring a given radium leak, first, when the electrometer was in parallel with the condenser, and again when it was not. The deflexion for a given charge is wonderfully constant over a wide range of voltages of the needle; though the deflexion for a given potential varies considerably. It is not found satisfactory to maintain the conductivity of the quartz fibre by dipping it in a solution of calcium chloride; in dry weather it does not conduct. It is better to recharge the needle to 250 volts every few hours, if it is used for so long; the needle may then leak considerably without alteration of the deflexion for a given charge.

§ II.

Having made these various improvements in our apparatus, and become more familiar with its use, we made a new layer, as already mentioned, from the pure radium bromide which Mr. Soddy had so kindly sent us. This at once gave good results. The curves shown in fig. 1 show the considerable precision which it was now possible to attain.

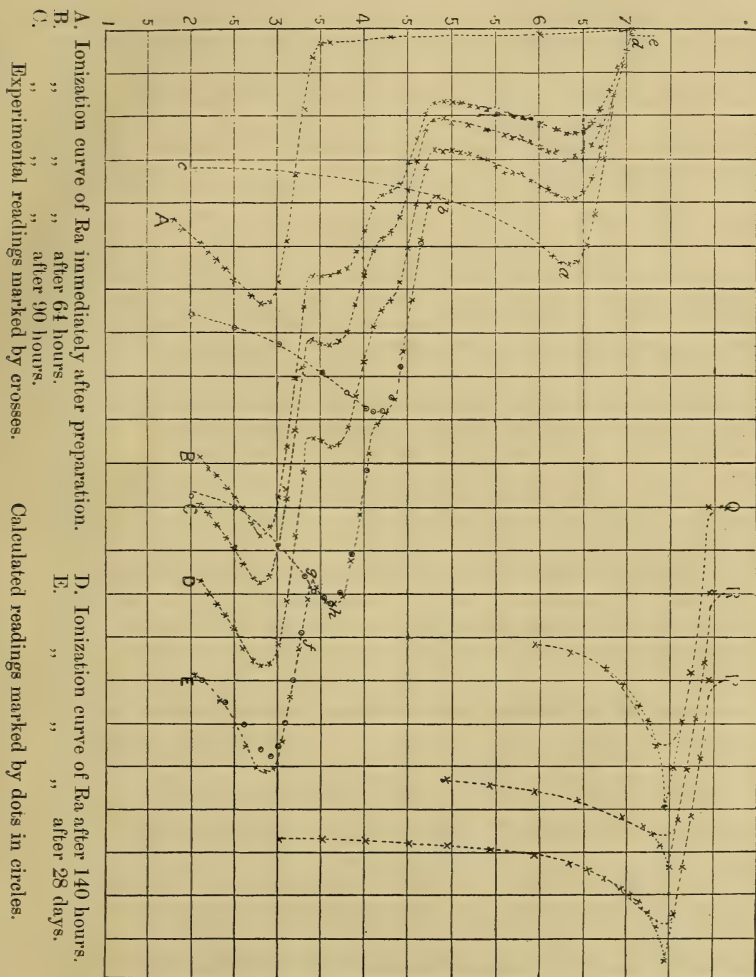


Fig. 1.

It may be well to repeat here that these ionization curves show the amount of the ionization due to the α rays at various distances from the radium layer, the medium ionized being

air at ordinary pressures and temperatures. A bundle of copper tubes is placed in a vertical position over the horizontal layer, so that only such α particles as move in a direction nearly vertical are allowed to proceed upwards. The bundle used at present contains about 100 tubes made of thin copper, each 2 mm. in diameter and 1 cm. long. The ionization-chamber consists of an upper horizontal plate of aluminium, and a lower plate of brass gauze; the distance between the plates in most of the experiments described in this paper was set at two millimetres. The ordinates represent the distances from the radium plate to the gauze; and the abscissæ the leaks on the quadrant electrometer. The absolute value of these leaks was not accurately measured, as there was no object in knowing more than their relative values; but it was calculated that the greatest leak measured was about 10^{-11} ampere.

Curve A, in fig. 1, shows the ionization due to the α rays when the dish was first prepared. It had been kept at a bright red heat for some minutes after preparation. The shape of the curve shows that all radioactive products except the radium itself had been completely removed. On comparison with the similar curve on p. 730 of the paper already cited, it will be seen that the new radium layer is thinner than that from which our first results were obtained; for the maximum value of the ionization occurs at 2.85 cms. instead of at 2.5. The greatest range is about 3.4 as before. This means that the α particles from the bottom of the new layer lose, in coming up through the material of the layer, a range of 5.5 mm. measured in air: the corresponding amount for the older layers was 9 mm. The new layer is nevertheless nearly twenty times stronger than the old. The term layer is, in fact, rather inappropriate. Actually the radium is not distributed over the dish in a thin uniform sheet but is studded over the surface in the form of small crystals which can be easily seen under the microscope. Consequently a weaker solution of radium bromide does not give, when evaporated, a thinner layer; but simply a smaller number of crystals. The various bundles, and in particular those from Ra, Emanation, and Ra A do not therefore stand out any better from each other when a weaker solution is evaporated, as we found on making the experiment. But the greater purity of the new layer gives the result which we formerly sought to obtain by evaporating very weak solutions. Presumably the crystals are now more regularly arranged, and are not overlaid by other matter which acts as a screen.

Since the radiation of the new layer is so strong, the

residual errors of the experiment become relatively small. In particular, consecutive readings of the same leak become much more nearly in agreement than before; when the readings are large, the difference between two such readings is generally less than one per cent. of the whole. For small readings it is proportionately greater; and it is quite clear that much of this irregularity is due to some extraneous cause, and is not due to variability of the principal leak, or inaccuracy either of the observer or his apparatus. Other experimenters have made a similar observation.

In the case of all the measurements recorded in this paper, the current was allowed to run into the electrometer for ten seconds; the mean of the first and second points of rest of the needle was taken as the correct reading. This was justifiable, as relative, not absolute, measurements were wanted; and as the difference between these two measurements was very nearly proportional to the whole deflexion.

Curves B, C, and D in fig. 1 show the state of the radium at definite intervals after preparation of the layer. Similar curves were published in our previous paper, but the curves now shown are much more accurate, and supply much more information. In the first place, the division of the α rays into four groups is now seen very clearly, and the range of each group can be measured accurately. In the former curves, the separation of the two central groups was not well effected, but now it is plain; the improvement being mainly due to the greater thinness and evenness of the radium layer.

In the same figure, curve E shows the state of the radium when 28 days old. According to Rutherford's theory the number of α particles in each group is the same, provided the radium preparation is old enough to be in radioactive equilibrium. This can be proved from the curve E in the following manner.

The curved side ab , belonging to Ra C, is produced to c , as shown. The line de represents a certain ionization produced by β and γ rays which is intercepted by the aluminium screen. It is the small difference between the readings at (say) 7.5 cm. when the aluminium is on and off. When the radium is within range, and the ionization of the α rays is measured as the difference between the on and off readings, this small quantity of β and γ ionization is included in the difference. Its value must be nearly constant over all ranges, as it is a certain fraction of the total β and γ ionization, which latter is found to be constant by experiment. We may therefore

consider the figure bounded by the vertical line through de and by the curved line $dabc$ to represent the ionization by the Ra C particles over the whole of their course except the first two centimetres.

This curve is now added to itself, being first lowered through 2.23 cm., and the points represented by dots in circles represent some of the results of the addition. It will be seen how nearly these points lie on the actual ionization curve, on which the experimental readings are marked by crosses. Again the curve $dabc$ is lowered, by 6.0 mm. this time, and added on to the sum already obtained, and the new points which show the result of the addition are also marked as dots in circles. These also lie very nearly on the ionization curve. For the last time the curve is added on, being this time lowered 7.3 mm.; and again the calculated points lie on the experimental curve except just at the peak. Thus the full curve is formed by the superposition of four simple curves, each alike in all respects but that of height; and the inference is that the four groups of α particles are alike in every respect but that of initial velocity. The differences of the ranges are in this way given with more accuracy than that with which the actual ranges can be found, for it is hard to avoid all uncertainty about the latter because the gauze of the ionization-chamber is not easily made quite flat. If the curve fg is produced to meet the curve hg also produced, then the meeting-point is at a height 3.4 cm. This point should represent the arrival of the Ra rays at the middle of the ionization-chamber, which is 1 mm. higher than the gauze. Hence the range of the particles from Ra is 3.50 cm.; and the ranges of the other three groups are 4.23, 4.83, and 7.06. These must be correct to 0.5 mm.

Since the ionization curve can now be drawn with some accuracy, it becomes reasonable to make an attempt to discover the relation between the velocity of the α particle and the rate at which it spends its energy.

Let v be the velocity of the particle, and s the distance it has yet to run. Suppose that the energy spent per cm. = $k.v^{-n}$, where k and n are constants. We seek a value for n .

Then

$$\frac{1}{2}m \frac{dv^2}{ds} = kv^{-n}.$$

$$\therefore mv^{n+1}dv = kds$$

$$\frac{mv^{n+2}}{n+2} = ks.$$

Hence the rate of expenditure of energy on ionization

$$= \frac{k}{v^n}$$

$$\propto \frac{1}{s^{\frac{n}{n+2}}}.$$

If the strip of air (supposed thin) in which the ionization is measured is at such a height above the radium layer that the α rays from the top of the layer pass it by a distance x , and those from the bottom by a distance $x-d$, then the ionization produced in this strip by the α particles from all parts of the layer

$$\propto \int_{x-d}^x x^{-\frac{n}{n+2}} dx$$

$$= A \left(x^{\frac{2}{n+2}} - (x-d)^{\frac{2}{n+2}} \right),$$

where A is a constant. If the rays from the top of the layer pass the strip by x , and those from the bottom do not reach it, then the ionization

$$= Ax^{\frac{2}{n+2}}.$$

In the curves as found experimentally the effect is further complicated by the fact that the ionization-chamber has an appreciable depth of 2 mm., and that the cones of rays have an appreciable vertical angle. We need not, however, take these factors into account in the formula. We can give n different values, plot the corresponding curves as found from the formula, and round off the corners in accordance with the effect which the two disturbing factors must have.

When this is done, it is found that $n = \frac{1}{2}$ gives a curve which is very near to the actual form. Choosing the value 5 mm. for d , which seems proper from an inspection of the curve and from trial, the ionization is equal to $Ax^{\frac{2}{3}}$ from $x=0$ to $x=5$ mm., and thereafter to $A\{x^{\frac{2}{3}} - (x-5)^{\frac{2}{3}}\}$. The curve obtained from this formula is plotted in fig. 1, P; and the corners are rounded off by the dotted line. The value of A is chosen so as to make the curve more easily comparable with the experimental curve E (portion $d a b c$).

Curves Q and R are derived from the formula when n is put equal to 1 and to $1/3$ respectively. It will be seen that they do not approach the experimental form so well, and err in opposite directions.

The conclusion is that the α particle spends its energy at a rate which is approximately inversely proportional to the square root of its velocity.

§ III.

From the results described above as well as from those described in the previous papers (Phil. Mag., Dec. 1904) it will be clear that the law of the absorption of the α rays is considerably different from that which has been generally assumed hitherto. It has been supposed that a stream of radiation gradually diminishes in power as it penetrates any material substance, the rate of loss being approximately expressible by an "absorption coefficient," and the law being therefore nearly exponential. It is true that it has been pointed out that this coefficient is not constant for any one stream in any one gas, but varies with the distance of the stream from the source. If this variation were small, there might be some ground for considering that the law was fundamentally dependent on an exponential form, but varied from it on account of some secondary cause. As a matter of fact, however, the variations are far too great to permit of such a supposition.

The true state of affairs is very different. There is no such absorption coefficient, nor any approach to an exponential law.

Each α particle pursues a rectilinear course, no matter what it encounters: it passes through all the atoms it meets, whether they form part of a solid or a gas (or in all probability of a liquid), suffering no deflexion on account of any encounter until, at any rate, it is very near the end of its course. It loses energy as it goes, a portion of this at least being spent in producing ions. This drain on its energy, at least, robs it of its extraordinary powers of penetration and ionization.

From this it will be clear that there are two quite independent characteristics of a stream of α radiation: one, the number of α particles in it, the other, the energy of their motion. The latter may more or less be evenly distributed amongst the various particles. It is possible for the latter of these characteristics to vary considerably whilst the first does not change at all. For example, in the curves described above, the α particles from Ra C remain unchanged in number up to nearly 6.5 cms. from the commencement of their motion; but their energy has almost vanished at the longer distance. Again, as will be seen later, a thin metal plate

may be placed in the way of the stream, and so rob every particle of some of its energy, but not a single one is brought to rest by collision with the atoms of metal, and the number of particles in the stream remains unchanged.

In our experiments two different quantities are measured, one the range of the α particles, the other, the ionization which they produce. The range of the α particle in a given substance is the distance it can penetrate before losing its power of making ions in that substance, which can be collected and measured by electrical means. We assume that when it ceases to have this power, the energy has fallen to an amount which is very small compared to its initial energy (say $1/500$), so that even if it varies in different substances, such a variation cannot affect the range appreciably. As a matter of fact, it is probable that no such variation exists.

The range of an α particle in any substance is therefore dependent on the original energy of projection. It is not quite proportional to it, because, as we have pointed out already, the α particle does not spend energy at exactly the same rate at all periods of its flight; towards the end of its range its rate of expenditure is somewhat increased. If the particle is made to pass through some material other than air, its range may be much altered. For example, if it passes through a thin silver film whose thickness is $\cdot 001$ cm. its range in air, supposing it to be allowed to complete its course in that medium, is diminished by more than 3 cms. Or again, the α particle which has a range of 7.0 cms. in air has only a range of 3.3 cms. in methyl bromide at the same pressure and temperature. Consequently, if we measure the range of the α particles in different gases, or after their passage through thin films of matter, we obtain a measure of the comparative rates at which energy is expended by a particle in passing through these gases and materials.

The second quantity which is measured by us is the ionization produced by the particle at various distances from its source. From this measurement we are able to discover, amongst other things, whether the stream has suffered in any other respect than that of loss of range when it has passed through thin material films, supposing that the rest of the flight is in ordinary air; and if we compare the quantities as measured in different gases, we are enabled to compare the conductivities which are imparted to these gases by α particles moving through them under conditions otherwise similar.

These two quantities are independent of each other. It is true that if the conductivity of a gas were proportional to the

number of ions produced by the particle, and that if the total number of ions produced by an α particle were the same, no matter the gas in which it completed its course, then the product of the ranges and the ionization per cm. of path would be a constant. Now Rutherford has made measurements of the total ionization produced by the particle in some substances, and found it the same in each case. But there are other substances for which this is not true; *e. g.*, ether, ethyl chloride, and methyl bromide; and in consequence the second of these suppositions is not justified. It is even possible that the first may also prove incorrect. It is conceivable that the α particle may spend energy on ionization whilst passing through a molecule, especially a complex molecule, and that the products may never get away from the molecule so as to be separated by the electrical field, and measured by the electrometer. That this actually occurs certainly seems a natural inference from some of the following results.

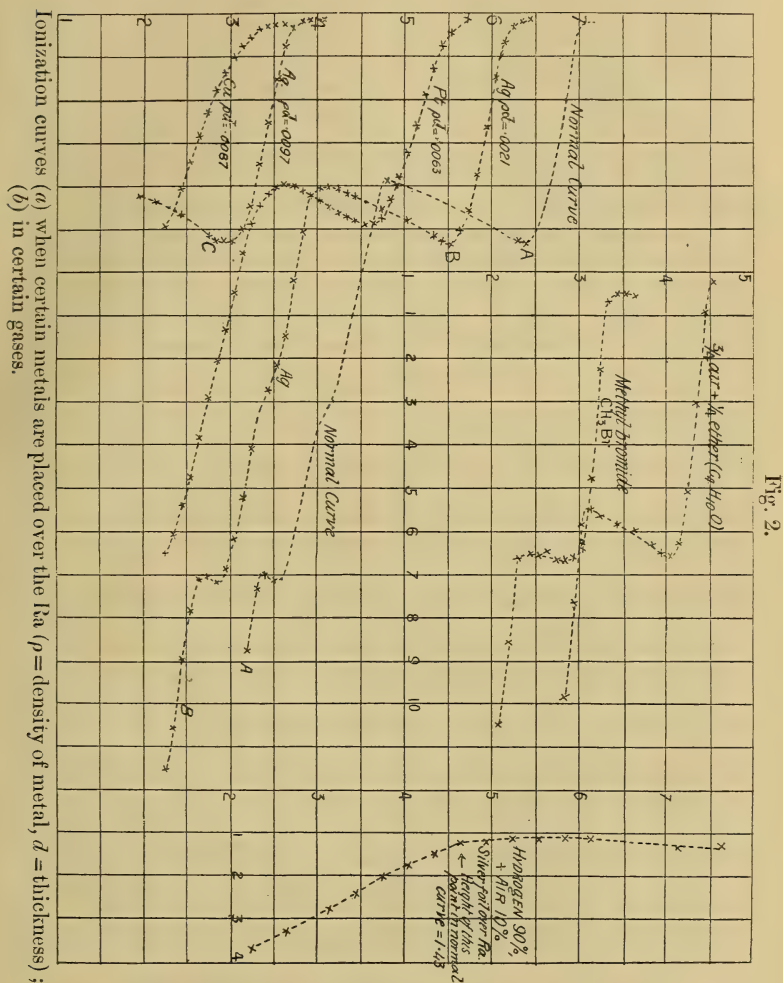
Let us now consider the experimental evidence. In fig. 2 are drawn three curves, A, B, and C. The first of these represents the ionization curve of radium in air. Curve B shows the result of placing a number of thin sheets of silver-leaf over the radium, so that the rays had to traverse these before reaching the ionization-chamber. Curve C shows the effect of substituting thin silver-foil for the leaf. The product of the density and the thickness was in the case of the leaf $\cdot 00213$, and in the case of the foil $\cdot 00967$. These figures were readily obtained by weighing a measured area of the material in each case; the leaf was not easy to handle, and the loss of range in its case not very great, so that the foil experiment is the more reliable.

The curves A B C are clearly similar in shape, and differ only in their height above the zero line. The silver sheet has not in either case caused any modification of the α stream, except that it has cut off an equal amount from the range of each particle.

If some of the particles had been stopped by the metal, or if some had lost more energy than others, there would have been more or less distortion of the curve. But the former condition does not occur, as has been already explained, and the latter was also prevented in this case by the great uniformity of the foil, which was polished on both sides.

The loss of range in the case of the silver-foil is 33.5 mm. If we take the density of the air to be $\cdot 0012$, we may say that a silver stratum for which thickness $\times$ density = $\cdot 00967$ is equivalent to an air stratum for which the same product = $3.35 \times \cdot 0012 = \cdot 00402$. The ratio of these quantities is 2.41.

In fig. 2 are shown the results of some of our experiments of this kind on other metal films. All do not show the same regularity as the silver: for we have found great difficulty in obtaining uniform films in the majority of cases. The



rolls used by the jewellers are not fine enough to give perfect results, and we believe there is no goldbeater in Australia at present. Nevertheless the metal workers have managed to roll for us films of gold, platinum, and copper which have given us fair results; aluminium leaf was procurable and

thin tinfoil. When a film is not uniform, the fact is made evident by a distortion of the curve, *e. g.* in the case of copper. It will be seen that the top of it slopes too much, implying that the α rays do not come into the ionization-chamber as suddenly as they ought to do. Some have, in fact, been less checked than others because they have passed through thinner portions of the somewhat uneven film. We have measured, in such cases, the drop of the curve as shown by its amount at the middle point of the top of the Ra C portion. *E. g.*, in the case of platinum we have measured the depth of the middle point of the top of the curve below that of the middle point of the top of the normal curve.

The results of these experiments are shown in the following table. The first column of figures gives the product of the thickness of the metal film and its density, the second the corresponding drop of the curve, multiplied by the density of air, and the third the ratio of these two products.

	I.	II.	III.	IV.	Ratio IV./III.
Gold	·0121	·00396	3·05	14·2	4·65
Platinum	·00633	·00192	5·29	14·0	4·25
Tin *	·0051	·00212	2·41	10·85	4·50
Silver	·00967	·00402	2·41	10·4	4·30
Copper	·00873	·00492	1·78	7·96	4·45
Aluminium ...	·00258	·00209	1·23	5·15	4·20

* Tinfoil contains a certain proportion of lead; but this could not affect the result very appreciably.

It is remarkable that the numbers in the third column are nearly proportional to the square root of the atomic weights of the metals. To bring this out more clearly, the square roots are shown in a fourth column, and in a fifth the ratios of the numbers in the two previous columns.

Moreover, air itself falls approximately into line with the metals. Its ionization ratio should be entered in the third column as unity, and the average square root of its atomic weight as $(4\sqrt{14} + \sqrt{16})/5 = 3\cdot79$. The corresponding entry in the last column should be also 3·79.

Also it is easily seen that hydrogen is not far away from the others. For it has been shown by Strutt that its conductivity under ionization as compared with air is ·226. Assuming for the present that this means that a 1 cm. layer of hydrogen has the same effect as a ·226 cm. layer of air, then the ratio of the product of the thickness and the density in the case of the hydrogen to the similar product in the case of the air is $1/14\cdot4 \times \cdot226 = \cdot31$. The square root of its atomic weight is 1, and $1/\cdot31 = 3\cdot2$.

Since the atomic weight is of such consideration in this law, it is more direct to state the results in another way. Instead of comparing the stopping powers of strata of metal and air of equal weights, we can compare the stopping powers of strata containing equal numbers of atoms, and therefore the stopping powers of individual atoms. For example, a stratum of silver produces the same effect as a stratum of air whose weight is 2.41 times greater. Thus for equal weights silver stops $1/2.41$ times as much as air; but for equal numbers of atoms it stops $108/14.4 \times 2.41$ times as much as air. This ratio, which is equal to 3.11, may be called the "stopping power" of the silver atom, referred to the air atom as a standard. The latter is an imaginary atom having an atomic weight 14.4, a molecular weight 28.8, and an atomic square root 3.79.

The stopping powers of the various metals examined will be set out below, in conjunction with those of certain gases.

When we found that a simple law seemed to cover the behaviour of substances differing so widely in all their properties as those enumerated, we thought it advisable to examine such other substances as were available. We were unable to obtain other metal films; and we therefore turned our attention to gases. Now no striking evidence for the square-root law could be obtained from an examination of gases whose atomic weights were nearly the same as that of air, such as oxygen or nitrogen. Indeed, it had already been shown by Strutt that their behaviour was very much the same as that of air. Nevertheless, it was not to be forgotten that their proved properties were not against the square-root law.

We therefore made experiments with the following gases, which contained atoms whose weights were very different from those of air atoms:—

Methyl bromide, methyl iodide, ethyl chloride, carbon tetrachloride, ether, and hydrogen.

Methyl bromide was a most suitable gas for our purpose. The ratio of its molecular weight to that of air is 3.28. If, then, the loss of range in passing through this gas were proportional to the density, the range of the particle from Ra C would be only about $7/3.28$ or 2.3 cm., provided the gas were at atmospheric pressure. But if the square-root law were true, the range would be much greater, and could be calculated thus:—

The carbon atom should contribute a stopping power proportional to $\sqrt{12}$, the three hydrogen atoms to $3\sqrt{1}$, and the bromine atom to $\sqrt{80}$. Total $3.46 + 3 + 8.95 = 15.41$. In air the stopping power should be the average of that of

four molecules of nitrogen and one of oxygen. The stopping power of nitrogen should be proportional to $2\sqrt{14}$ or $7\cdot48$, that of oxygen to $2\sqrt{16}$ or 8. Now $(4 \times 7\cdot48 + 8) / 5 = 7\cdot58$. Therefore the stopping power of the CH_3Br molecule as compared to that of air should be $15\cdot41 / 7\cdot58 = 2\cdot03$; and the range of the α particle of Ra C should be $7 / 2\cdot03 = 3\cdot4$ cm.

A sufficient quantity of methyl bromide was prepared and kept liquid in a freezing-mixture. The vessel containing our apparatus was joined to an air-pump and exhausted till the remaining pressure was equal to about 6 cm. of mercury. The liquid was then allowed to pass in under atmospheric pressure until the vessel was full of the gas. The pressure was again reduced to 6 cm., and vapour introduced until the pressure in the vessel was once more equal to that of the atmosphere.

The ionization curve was then obtained. It is plotted in fig. 2, and shows that the range is nearly as calculated from the square-root law.

It was not advisable to take it for granted that the vessel contained vapour only; and after a little time we hit upon a method of determining this point satisfactorily. The method also proved particularly useful in the case of mixtures of air and vapours, as will be described presently. Immediately after measuring the range of the particle in the gas, we opened a communication between the vessel and an exhausted globe of about one litre capacity. The capacity of the vessel itself was 6 litres. The weight of the gas so drawn over was compared with the weight of the air that came over under similar conditions when the larger vessel contained air only; and from this ratio the proportion of air and vapour could be deduced. For example, the weight of the mixture of air and methyl bromide was 2·599 grams. The corresponding weight of air was ·800 gram. (It was not, of course, at atmospheric pressure.) Hence if x = the ratio of the number of air molecules to the number of molecules of CHBr ,

$$\frac{x + 3\cdot28}{x + 1} = \frac{2\cdot599}{\cdot800},$$

$$= 3\cdot25,$$

$$\therefore x = 1/75.$$

The extreme range of the α particle of Ra C in CH_3Br is shown by the curve to be 3·32. This is the distance from the radium to the gauze of the ionization-chamber. The

latter was 2 mm. deep, and half this, viz. 1 mm., should be added in order to get the true range, which may therefore be taken as 3.42. The corresponding range in air is 7.06.

Hence, if x = stopping power of the methyl-bromide molecule,

$$\frac{1+75x}{76} = \frac{7.06}{3.42} = 2.07,$$

$$\therefore x = 2.09,$$

which is very nearly the value already calculated on the square-root law. The errors of experiment were not so small that this almost absolute agreement can be looked on as more than accidental. It will be seen, however, that all the gases examined gave good results.

The curve of a gas is different in some important respects from that which is obtained in examining the loss of range in metal films. In the latter case, all the ordinates are reduced from those of the normal air-curve by nearly the same amount; in the former, they are reduced in nearly the same proportion. Again, in the case of the metal film (provided it is uniform) the abscissæ are unaltered; in the case of the gas, the abscissæ are all altered in the same proportion and show the amount of ionization produced by the α particle in traversing the gas. For example, the extreme abscissa of the Ra C group has the value 666 in the gas and 547 in air; thus the ionization per cm. is 666/547 or 1.27 times greater in the gas than in the air. Since, however, the range is 2.07 times shorter, the ratio of the total ionization in the gas to the total ionization in air is only $1.27/2.07 = .62$ nearly.

In most of the complex gases examined the α particle produces less ionization than in air. We propose to make a further examination of this point.

In the case of the other gases, the procedure was very similar. Carbon tetrachloride, methyl iodide, and ether are liquid at ordinary temperatures and pressures, and in their case we introduced the necessary quantity of liquid to give a vapour pressure of about two-thirds that of saturation, and then filled up with air. We found it better to do this than to attempt the experiment with low pressures in our vessel, for the latter had so many joints and stuffing-boxes that it would not keep a vacuum very well. It was, however, quite airtight enough to retain a mixture unchanged for hours provided it was at atmospheric pressure. The proportion of air to vapour was afterwards determined in the manner already described.

Ethyl chloride was tried before we used the method of weighing a definite quantity of the gas. But the vessel was emptied and filled three times, and there must have been very little air in it. It cannot give a decisive answer as to the validity of the square-root law, because, as it happens, the square-root law and the simple density law give nearly the same value. On that account we have not yet repeated the experiment.

Hydrogen presented some special difficulties. The range (on the square-root law) of the α particle of Ra C must be about 30 cm., and our apparatus only measures up to 8 cm. We therefore placed a piece of silver-foil over the radium, which was equivalent to 3.3 cm. of air. This cut down the range of the Ra C particles to 3.63, and the range of the swiftest of the others to 1.43. The former when multiplied by four would still be out of range, but the latter would be within it.

Fig. 2 (p. 331) shows that this was the case. With a mixture of hydrogen and air in the proportion (by pressure) of 9 to 1 it was at 4.6. With mixtures containing a greater proportion of hydrogen it rose somewhat, but a small proportion of air made little relative difference. The curve is of course very much drawn out vertically, and drawn in horizontally.

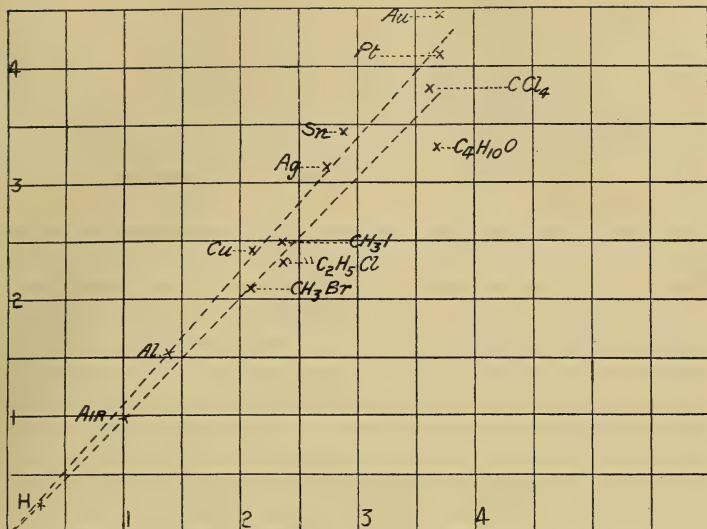
The results obtained for all the above substances are collected in the following table:—

TABLE, showing “stopping power” of various atoms and molecules as compared with that of air. The atomic weight of the supposititious atom of air is taken as 14.4, and the “atomic square root” as 3.79.

	Stopping power of atom or molecule.	Ratio of square roots.	Ratio of atomic or molecular weights.
Hydrogen	.246	.265	.069
Air	1	1	1
Aluminium	1.53	1.38	1.88
Copper	2.42	2.1	4.53
Silver	3.11	2.75	7.5
Tin	3.42	2.88	8.2
Platinum	4.12	3.7	13.5
Gold	4.45	3.7	13.7
Methyl bromide	2.09	2.09	3.28
Ethyl chloride	2.30	2.36	2.23
Methyl iodide	2.49	2.35	4.9
Ether	3.30	3.68	2.56
Carbon tetrachloride ...	3.8	3.61	5.41

These results are shown graphically in fig. 3, in which the ordinates and abscissæ represent the numbers in the first

Fig. 3.



and second columns respectively. It is curious that the metals and gases seem to lie on rather different lines. Possibly this is due to the difference between solid and gas; possibly again to the fact that the gases tested have such complex molecules. Further research may make the point clearer. Several other points arose in the course of the experiments, which deserve further inquiry. For instance, the metal films did not cause exactly the same drop throughout all the curve; the slower α particles were a little less affected than the swifter.

§ IV.

From these results it appears that the following law is true for certain elements, at least to a first approximation.

The energy spent by an α particle in its passage through an atom is proportional to the square root of the atomic weight. It can never be said that this is a law which is general to all the elements until all the elements have been experimented upon. In the case of many of them, the experimental difficulties will be very great. Since, however, the law holds with some accuracy in several cases, it becomes desirable to consider whether any physical interpretation can be placed upon it.

Now the energy which is spent by the α particle is certainly spent, in part, in producing ions. If the rate of the expenditure of the energy as a whole is related to the atomic weight by so simple a rule as that of the square root, it is probable that the part which is spent on ionization obeys the same rule. For if not, and if the expenditure on ionization follows some other law, then the remaining expenditure must follow such a law that the two together compound into the simple rule of the square root. It is proper to put aside this more complicated hypothesis until we have considered the simpler one.

We may therefore advance the law one stage and state it thus:—The energy spent on ionization by an α particle in its passage through an atom is proportional to the square root of the atomic weight. The question now arises: Is the variation from atom to atom due to a difference in the number of ions produced, or in the energy required to produce an ion, or in both these quantities? Because an oxygen atom absorbs four times as much energy as a hydrogen atom, are there four times as many ions produced from an oxygen atom, but the energy per ion the same; or is the act of ionization four times as difficult, but the number of ions the same; or are neither of these suppositions true, and is the difference between oxygen and hydrogen in this respect due to a variation in each of the two quantities?

Now the total conductivity imparted to a gas by the passage of a particle through it is very nearly the same for the simpler gases. This is true even though some of them differ widely from each other in their atomic weights, *e. g.* oxygen and hydrogen. From this it appears probable that the energy required to produce an ion is always the same. It is true that in some of the complex gases the total conductivity is less than in the simpler gases, but this can be explained on the hypothesis that the same number of ions is *made* in these heavy molecules, but that they find a difficulty in escaping from the molecule and are recombined, so that they add nothing to the conductivity of the gas. If this hypothesis is not entertained, we are driven to suppose that the energy required to make an ion is not the same in some of the complex molecules, and that the number made varies by just so much as to make the total loss of energy of the particle in each molecule follow the same simple law as holds for the simpler molecules. For the present at least this complicated supposition must also be set aside, and again we may make an advance in the statement of our law. It now stands:—

The number of ions made by an α particle in passing

through an atom is proportional to the square root of the atomic weight.

It should, perhaps, be pointed out that this does not mean that the α particle makes the same number of ions out of each atom of the gas through which it is passing ; but merely compares the average per atom. It asserts, for example, that in going through oxygen the α particle makes a number of ions which is four times as many as the number made by the particle moving with the same speed in passing through hydrogen at the same pressure and temperature.

It is possible that this rule implies that a particle must make more than one pair of ions in passing through any one atom, unless ionization is only rarely the consequence of the encounter. For if only one pair could be made in each atom, and if the α particle made one pair in crossing each atom of hydrogen, then in oxygen at the same pressure and temperature as the hydrogen, only the same number of ions would be made per centimetre, which is not the case. Nor does it appear possible to suppose that ionization does not occur at a large proportion of encounters ; the number of ions made is of the same order as the number of atoms traversed, according to Rutherford.

The most reasonable interpretation of these results seems therefore to be (1) that the α particle makes the same number of ions during its course no matter what the gas which it traverses ; (2) that the energy required to make a pair of ions is always the same ; and (3) that the observed variations in the conductivities in some cases are due to the failure of ions to get free from the molecule in which they are made.

Under these circumstances we must look for the origin of the square-root law in some disposition which limits the number of ions that can be made in an atom. It is not to depend on any variation of the energy required to make a pair of ions ; but must be due to some condition which only gives the α particle such an opportunity of making ions as is to be measured by the square-root of the atomic weight ; and, where it forbids the act of ionization, forbids also the corresponding expenditure of energy.

We can only make guesses as to what this condition may be. It has been maintained, on good evidence (see Meyer, 'Kinetic Theory of Gases,' § 112), that the atom has a disk-like form, and that the various atoms and molecules differ in two dimensions only. It is possible that an explanation of the square-root law may be found in the hypothesis that ions can only be formed on the circumference of the atom's disk, for the amount of that portion of the atom

which lies on the circumference is proportional to the square root of the whole. Under such circumstances an α particle, if it struck the atom anywhere except at the edge, would pass through without result.

Since the energy required to make a pair of ions is probably the same no matter what the atom or molecule may be in which they are made, and no matter whether it be solitary or a constituent of a complex molecule, we seem driven to the belief that ionization consists in breaking bonds which are always the same, both in character and in strength. The electron is torn from its union with something else which is always the same, and must form some constituent of all atoms. This hypothesis also makes reasonable the supposition that an act of ionization may be performed within a complex molecule, and yet no conductivity be imparted to the gas. For the act would then consist in the breaking of an electron from its attachment to this common constituent, and would not of necessity imply separation from the system of the molecule. The idea of a common constituent is, of course, an old one. It is certainly strengthened by the discovery that radium and its successive products emit α particles which are exactly alike.

We wish to express our gratitude to Mr. J. P. V. Madsen, B.Sc., for the generous assistance which he gave us during several weeks of full work, and to Professor Rennie, D.Sc., and Mr. A. J. Higgin for their kindness in making the chemical preparations.

XL. *On the Space occupied by Atoms: The Theories of Th. W. Richards and J. Traube.* By Prof. Dr. J. TRAUBE, Berlin*.

FOR several years † I have been busy with investigations regarding the space relations of different forms of matter, and have repeatedly pointed out the extraordinary importance of this much neglected domain of science.

To my satisfaction, so distinguished an investigator as Th. W. Richards has recently ‡ taken up the same problem,

* Communicated by the Author.

† See, especially, my *latest* work, Drude's *Ann. d. Phys.* Bd. v. p. 548 (1901), and Bd. viii. p. 267 (1902); further, the account in the *Boltzmann-Festschrift*, p. 430 (1904); also *Zeitschr. f. anorg. Chem.* Bd. xxxiv. p. 413 (1903), Bd. xxxviii. p. 399 (1904), Bd. xl. p. 372 (1904); and the older account: "Ueber den Raum der Atome, F. W. Ahrens, *Samml. chem. techn. Vorträge*, Bd. iv. p. 1 (1899), and my *Grundriss der Physikal. Chemie* (Enke, Stuttgart, 1904).

‡ Th. W. Richards, *Zeitschr. f. Physik. Chem.* Bd. xl. pp. 169 & 602 (1902); Bd. xlii. p. 129 (1903); and Bd. xlix. pp. 1 & 15 (1904).

and in a series of memoirs has arrived at conclusions and results which substantially agree with my own, which partly form extensions of them, but which partly also, in one essential point, present so characteristic a difference in the point of view that it may be worth while to compare the two theories.

More especially, however, do I wish, by a brief *résumé* of my work, to establish my *priority* in regard to *many* of the problems treated of by Richards. Although Mr. Richards is good enough, in a concluding note, *l. c.* vol. xlix. p. 17, to refer in terms of approval to my work, I am nevertheless of opinion that the note in question is liable to be misleading.

The space occupied by matter may be regarded as consisting of

- (1) The *true* volume of the atoms and molecules;
- and (2) The co-volume, or space within which the atoms are free to move.

Now while in the case of the "ideal" gases the true volume is negligible in comparison with the co-volume, Budde and especially J. D. van der Waals have shown how, from the behaviour of gases under high pressures and near the condensation-point, the coefficient *b* in van der Waals' equation

$$\left(p + \frac{a}{v^2}\right)(v-b) = RT$$

may be taken into consideration*, and how, by the aid of this well-known equation, the cohesion coefficient *a*, the true volume *b*, and the co-volume *v-b* may be calculated, as a first approximation, by taking into account the deviations of strongly compressed gases from the ideal gaseous laws.

It is sufficiently well known with what success van der Waals applied this equation to the transition from the gaseous to the liquid state, and further to the theory of corresponding states; and how, by means of the coefficient *b*, it became possible to determine the absolute dimensions of the atoms.

The proof that the equation of van der Waals also holds good for homogeneous liquids and even for solids—*so that the*

* I may take this opportunity of remarking that I regard the establishment of van der Waals' state equation as one of the greatest advances in physical chemistry. If we take into consideration the fact that *a* and *b* are not constants, then the equation holds with much greater accuracy than has hitherto been supposed; in any case, it is in general much to be preferred to those complicated equations which have been proposed in place of it, and which, perhaps, come nearer the truth, but are far less well adapted to purposes of calculation.

modified gaseous laws are applicable to all three states of matter—was first given by myself*.

Now the method which I first adopted in investigating the volume relations of liquids was to compare, according to Kopp's method, at the ordinary temperature, the molecular volumes (molecular weight $\div$ density) of such organic substances as had the same composition-difference, *e. g.* CH_2 , H_2 , &c. It appeared that—as a first approximation—corresponding to equal composition-difference there was equal volume-difference. On comparing, for a given compound, the atomic volumes so determined with the molecular volume, it appeared, contrary to Kopp's calculations, that the sum of the former was less than the molecular volume.

Thus the molecular volume of a liquid consists of a sum of atomic volumes ΣV_a , but in addition to this also of a second term, which obviously represents the co-volume. The values calculated by the above method of this co-volume, per gramme-molecule, mostly oscillated about 25 c.c. at 0° as a rough approximation, and only in the case of associated substances were the calculated values considerably less, for reasons which are easy to understand, when the simple molecular weight was made the basis of calculation.

The values v_1 and v_2 of the volumes corresponding to two neighbouring temperatures T_1 and T_2 , calculated by means of the coefficient of expansion of the liquid, were next substituted in the equation of van der Waals, and thus the values of b and $v-b$ calculated. The values of b so obtained were found to be in agreement† with the ΣV_a calculated according to Kopp's method—*i. e.* with the sum of the atomic volumes. Thus the equation of van der Waals was found to hold good for homogeneous liquids.

The same two methods led to the desired result in the case of solids. Kopp's method‡, as in the case of liquids, led to an equation of the form $\frac{m}{s} = \Sigma V_a + \Phi$, where $\frac{m}{s}$ denotes the molecular volume, ΣV_a the sum of the atomic volumes, and Φ the co-volume. When, on the other hand, by using the coefficients of expansion of the elements the volumes $b = \Sigma V_a$ and $\Phi = v - b$ were calculated by means of van der Waals' equation, the following result was obtained§:—

On the supposition that when a solid element is heated

* Drude's *Ann. d. Phys.* Bd. v. p. 548 (1901); and *Zeitschr. anorg. Chem.* Bd. xxxiv. p. 413 (1903).

† J. Traube, Drude's *Ann. Phys.* Bd. v. p. 552 (1901).

‡ See the author's *Grundriss der Physik. Chemie*, Enke, Stuttgart 1904, p. 207.

§ *Zeitschr. anorg. Chem.* Bd. xxxiv. p. 413 (1903).

only the co-volume expands—as in gases—and not the total volume, the coefficient of expansion was referred to the former instead of the latter, and it appeared *that this coefficient of expansion is equal to $\frac{1}{273}$ for all the elements* (halogens excepted). Therewith a proof was obtained that van der Waals' equation applies to all three states of aggregation, and that it enables us to calculate the true volume and the co-volume for all three states.

Now the separate consideration of each volume leads to remarkable consequences.

(a) *The true volumes (the quantities b).*

The atomic volumes as calculated by Kopp's method and by means of van der Waals' equation are only *mean values*, which may be used for purposes of calculation much in the same way as the mean molecular velocities are used in the kinetic theory of gases. The volume of an oxygen atom is, *e. g.*, least when it is in combination with hydrogen, it is appreciably larger in combination with carbon, and increases in homologous series, in combinations such as methyl, ethyl, &c. The contraction, *e. g.*, undergone by chlorine when combined with the atoms of various metals depends on the chemical affinity of the metal concerned for chlorine.

I was the first—*several years before* Mr. Richards*—to

* In the *Zeitschr. Physik. Chem.* Bd. xlix. p. 19, Richards remarks:—

"It is a pleasure to me here to draw attention to the entirely independent investigation of J. Traube in this direction. It is remarkable that quite simultaneously he published in Drude's *Ann.* Bd. v. p. 555 (20 June, 1901) the following proposition:—

'The atomic volume of an element changes from one compound to another; it decreases as its attraction for the neighbouring atoms increases'—while on June 15 of the same year my version of this relation appeared in the *Proc. of the Amer. Acad.* lvii. p. 17:

'The atomic volume is not constant, but is a function of pressure and temperature, and probably of electric strength.' "

Mr. Richards has evidently overlooked the fact that in my monograph, which appeared *already* in 1899, quoted by him and published in Ahrens, *Samml. chem. u. chem.-techn. Vorträge*, Bd. iv. p. 22, there is contained the following proposition:—

"The vibration space (the quantity *b*) changes, according to the nature of the atom, more or less from one compound to another, and in accordance with the mutual influence which the various atoms contained in the compound exert on each other. The less the attraction of neighbouring atoms, the more nearly does the vibration space approach its maximum value (maximum vibration volume), and, on the other hand, the vibration volumes decrease the more the greater the mutual attraction of the atoms."

I may also remark that I undoubtedly have the claim to priority in the matter of the doctrine of the compressibility of atoms (the quantities *b*) as against J. D. van der Waals (*cf.* my statements in *Zeitschr. anorg. Chem.* Bd. xxxvii. p. 242, 1903).

arrive, as a result of my researches, at the following fundamental proposition regarding atomic space:—

The space occupied by an atom changes from one compound to another; it is the smaller the greater the affinity of the atom in question for the neighbouring atoms with which it is combined.

The importance of this proposition lies, on the one hand, in the conception of atoms as *compressible spheres*, and, on the other, in the fact that the *contraction of the atom is regarded as a measure of chemical affinity*. Special attention must be drawn to the fact that the atomic contraction forms the *only* measure of chemical affinity; and although Richards cannot claim priority regarding the above and a few other propositions enunciated by him, he has rendered one of the most valuable services by showing* that in general the change in the free energy is just as little a measure of the chemical energy as the heat development.

In accordance with the above I have propounded the problem of calculating the atomic contractions from the results of chemical reactions, considering this to be the most important problem of *chemical dynamics*† (Mechano-chemie)—the sister science to thermo-chemistry.

The effect due to “affinity pressure” must be similar to that due to other pressures, and in particular to the intrinsic pressure of liquids, which in most cases amounts to about 1000 atmos. at 0°. From this it follows that the true atomic volumes corresponding to the liquid state must frequently be considerably less than those corresponding to the gaseous state, and that accordingly during evaporation not only the co-volume, but the true volume of the atoms as well, frequently undergo very considerable expansion.

A *gason* is greater than a *fluidon*. By means of this simple doctrine of the compressibility of atoms, among other things various difficulties of van der Waals’ theory may be explained, notably the third volume of the *isothermals* may be interpreted‡.

* *Zeitschr. Physik. Chem.* Bd. xlii. p. 129 (1903).

† Cf. my *Grundriss der Physik. Chemie*.

‡ Cf. my *Grundriss der Physik. Chem.* p. 107, 1904. I had also (cf. Drude’s *Ann. Phys.* (4) Bd. viii. p. 267, 1902, and *Zeitschr. anorg. Chem.* Bd. xxxvii. p. 226, 1903, and Bd. xxxviii. p. 399, 1904) appealed to the theory of gasons and fluidons in order to interpret the observations of de Heen, Galitzin, and especially also those made in my laboratory by Teichner, which apparently are not in accordance with Andrews’ theory of the critical state. I note that—as Teichner and Bakker have shown (*Zeitschr. Physik. Chem.* Bd. li. p. 345, 1905)—other suppositions are also possible. But when Verschaffelt (in the *Berichte der Akad. von Wetenschappen*, Amsterdam, 24, Dec. 1904) once more attempts to minimize Teichner’s experiments by ascribing them to the effect of slight

The determination of the true atomic volumes of an element in its elementary state and in its various combinations is also of considerable importance in other directions. Thus the determination of the space occupied by, for example, an oxygen or a nitrogen atom is helpful in arriving at the constitution. Ring compounds—such as the benzene ring—are characterized by so great a reduction in the volume of the carbon atom, that by means of the molecular volume it becomes possible to determine the number and even the nature and tension of the rings. The small space occupied by the carbon atom in its elementary state as compared with the large space which it occupies in compounds, is characteristic throughout of the behaviour of carbon in its elementary and combined states—and what holds for carbon is equally applicable to other elements.

Already in my first memoir* on chemical volumes I drew attention to the intimate connexions which primarily exist between the *molecular volume* and the *thermo-chemical constants*, especially the *heat of formation*. Now although Richards was not the first to point out the close relation between atomic volume contraction and heat of formation, he nevertheless first showed, in a large number of cases, that a far-reaching parallelism exists between heat of formation and atomic contraction, and that the *heat of reaction is, as a first approximation, equivalent to the work represented by the contraction of the atoms*.

Now when we consider that the electrical energy which a galvanic cell is capable of generating may, as a first approximation, be equated to the heat which under ordinary circumstances the reactions taking place in the cell are capable of developing, it necessarily follows from the relation of this heat development to the dynamical contraction-energy, that *the electric energy of a cell is to be attributed to the mechanical work accompanying atomic contraction*†.

In this respect my views are in complete agreement with

impurities, I beg to refer him to my own experiments, *Zeitschr. anorg. Chem.* Bd. xxxviii. p. 399, especially p. 402. When in using Teichner's method it is found that after the equalization of the densities during cooling these differences of density again appear above the critical temperature, then surely this cannot be due to admixtures! *It will be necessary to get accustomed to the supposition that Andrews' theory is in need of a very essential modification; that the temperature at which the meniscus vanishes is not the critical temperature; that this latter has, unfortunately, not been determined accurately in any case, but that in all cases it lies considerably higher than has hitherto been supposed.*

* *Zeitschr. anorg. Chem.* Bd. iii. p. 23 (1892).

† J. Traube, *Zeitschr. anorg. Chem.* Bd. xl. p. 382 (1904).

those of Richards, and we also agree in thinking that this way of regarding the electrical processes leads much further than the application of the electron theory to electrolytic processes, and especially as regards the conception of electrolytic solution pressure. While fully acknowledging the additions due to Mr. Richards in this respect, I regret that my earlier remarks in this connexion should have been overlooked by him. I have at a much earlier date * established a connexion between the solution pressure of a metal and the difference in the affinity of the solvents for the metal and the affinity of the metal for itself; and I have even repeatedly pointed out the very remarkable fact—which had not as yet been noticed by Richards—that *the atomic contraction of potassium, sodium, silver, &c. ions in water is the same, and is independent of the nature of the ion, and that this proposition is strikingly analogous to Faraday's electrolytic law* †. It is to be hoped, now that Richards also expresses similar views, that my former views will receive greater attention.

(b) *The Co-volumes* (the quantities $v-b$).

Not less significant are the conclusions to which we are led by a discussion of those volumes which we denote as co-volumes.

Since at ordinary temperatures the intrinsic pressure is in most cases not very different for different liquids, the molecular co-volume is also in general so nearly constant that frequently it may be used to determine the molecular weight as well as the approximate degree of association. Especially is this the case for substances soluble in water without ionization. The molecular volume in solution, or the *solution volume* ‡ of a dissolved substance is also equal to a sum of atomic volumes, increased by a co-volume, which in the case considered, owing to the contraction which takes place in water, is considerably less than for the undissolved substances, *but which for all substances not undergoing ionization was found to be approximately 12.4 c.c. per gramme-molecule as determined by Kopp's method.*

* J. Traube, *Ann. Phys.* Bd. lxii. p. 505 (1897); and Richards, *Zeitschr. Physik. Chem.* Bd. xl. pp. 179–181.

† J. Traube, "Raum der Atome," *Ahrens Samml. Chem. und Chem.-Techn. Vortr.* Bd. iv. p. 76 (1899), and *Ann. d. Phys.* Bd. lxii. p. 505 (1897).

‡ Cf. J. Traube, F. W. Ahrens *Samml. Chem. u. Chem. Tech. Vortr.* 1899, *l. c.*

This law—of *Avogadro*—is, as I yet hope to show, of fundamental interest in connexion with the notion of osmotic pressure. The above proposition also furnishes a very simple method for determining the molecular weight from the specific gravity of a solution.

The study of the co-volumes of *solids* led to values which were mostly not greater than half the molecular co-volumes of homogeneous liquids. Now since in the case of some such compounds as racemic acid &c. the molecular volume was undoubtedly doubled, but was accompanied by twice as great a co-volume as in the case of other solid compounds, the assumption did not appear too bold that in general the apparent halving of the co-volume during the passage from the liquid to the solid state was to be attributed to a doubling of the molecular weight—an assumption supported by the fact that during the passage from the solid to the liquid state the increase of volume is proportional to the decrease in the degree of association of the liquid *. *On this is based the only reliable method of determining the molecular weights of homogeneous solids.*

As regards the effect of temperature on the co-volume, the law of Charles-Gay-Lussac-Dalton applies to all three states of aggregation. Especially in the case of the solid elements (including most metalloids) did I supply the proof that the coefficient of expansion of the co-volume is $\frac{1}{273}$.

If the known co-volume per gramme-molecule for the gaseous state—22,400 c.c.—be divided by the molecular co-volume at 0° for the liquid state, we obtain the intrinsic pressure $\frac{a}{v^2}$. Using this method, I calculated the intrinsic pressure for most organic liquids to be between 800 and 1000 atmospheres; for gold in the solid state, 176,000 atmos.; and for the diamond, 5,460,000 atmos. These intrinsic pressures were, for an element obeying Dulong and Petit's law, equal to exactly three times the value $C \frac{dt}{dv}$, where C stands for the atomic heat and $\frac{dv}{dt}$ for the rate of change of volume with temperature †.

The intrinsic pressures of the metals were found to follow the

* Cf. my *Grundriss der Physik. Chem.* pp. 207 & 208.

† Richards has (*l. c.* Bd. xl. p. 174), unlike myself, calculated the intrinsic pressure not by the aid of van der Waals' equation, but by putting $Cdt = Kdv$ —an assumption which is not permissible, since the heat has to do work other than that involved in overcoming the internal pressure.

changes in the hardness, elastic modulus, and coefficient of friction †.*

It was further found ‡ that the length of free path of the metallic atoms as calculated from the difference between the diameter of the total volume and that of the true volume (the quantity b) was in agreement with that found from the diffusion-coefficient; also, the atomic *coefficient of compressibility* of the metals was the greater the co-volume.

Van der Waals' equation leads to the value $\frac{a}{v}$ for the heat of vaporization §, if, as in the case of monatomic substances, it is permissible to neglect the work connected with the passage from a fluidon to a gason. I have shown that in the case of mercury, observation and calculation are in agreement. In the case of poly-atomic substances, the heat of vaporization came out to be $2\frac{a}{v}$. Since according to Deprez-Trouton's rule the heat of vaporization is proportional to the absolute temperature of ebullition for non-associated substances, it follows that the *boiling-point* also becomes a simple calculable function of the volume, and finally I showed that in the case of metals *the absolute boiling-point* is inversely proportional to the *expansion-coefficient* ||.

(c) *Internal and External Atomic Volumes.*

So far we have only considered two volumes: the true volume and the co-volume, or the quantities b and $v - b$.

According, however, to the calculations of van der Waals, the quantity b is by no means the space which is completely filled by the mass of the atoms, but the four-fold multiple of this space. We may, following Clausius, picture to ourselves the quantity b as the space occupied by the atom together with the æther envelope into which no other atom can penetrate. The *internal* atomic volume would thus be proportional to the *external* volume, or to the space occupied by the envelope of "bound" æther. Now the theories of Clausius, Mossotti, and Exner lead to the conclusion that an approximate measure of the internal volume is furnished by

* J. Traube, *Zeitschr. anorg. Chem.* Bd. xxxiv. p. 413 (1903).

† J. Traube, *ibid.* Bd. xl. p. 377 (1905).

‡ J. Traube, *ibid.* Bd. xxxiv. p. 425 (1903).

§ J. Traube, *Ann. der Phys.* Bd. viii. p. 298 (1902); and *Zeitschr. anorg. Chem.* Bd. xxxiv. p. 423.

|| J. Traube, *Zeitschr. anorg. Chem.* Bd. xxxiv. p. 422 (1903).

the molecular refraction $\frac{n^2-1}{n^2+2} \cdot \frac{m}{d}$; and it has, as a matter of fact, been shown by me* that, in accordance with this assumption and with the theory of van der Waals, the quantity b is, at the critical temperature, in general about 3.5 or 4 times as great as the molecular refraction. According to this, the refractive index also becomes a simple volume-function.

The molecular refraction is also dependent in a similar manner on pressure—i. e., is compressible like the quantity b . Although the influence of the internal pressure is only demonstrable in the case of the quantities b , the considerably stronger affinity pressure acts also on the refraction. From this it follows that we have to consider not only the surrounding æther envelope, but also the atom proper, as elastic. I do not, however, go so far as T. W. Richards, who concludes from this result that the distinction between the internal and external volumes is unjustifiable; but I conclude, from the above compressibility, as well as from the multiple relations between the internal volume, the external one and the co-volume (at corresponding temperatures), that ultimately æther and chemical matter are identical. The consideration of volume thus leads to views similar to those which are developed in the theory of vortex atoms, and even the more modern views regarding electrons are by no means so far removed from those here developed as would at first sight appear to be the case†. It might be specially pointed out, that the doctrine of the three volumes is not only in accordance with the theory of van der Waals, but also with that of Fresnel, who distinguishes the æther bound up with the atoms from free æther. The difference between the external and internal volumes corresponds to the space occupied by the bound æther, while the co-volume is filled with free æther.

I have pointed out‡ that the various properties of matter are related to the atomic volumes much more simply and directly than to the atomic weights (e. g., hardness, elasticity, viscosity, length of free path and velocity of diffusion, boiling-point, melting-point, heat of vaporization, surface-tension, refraction, isomorphie, valency, compressibility, cohesion, affinity, heat of formation, electromotive force, &c.). I have further drawn attention to the fact that the numerical relations among the atomic spaces are likewise simpler than those among the atomic weights, especially when the effects due to intrinsic pressure

* J. Traube, *Ann. der Phys.* [4] Bd. v. p. 552 (1904).

† Cf. J. J. Thomson, 'Electricity and Matter.'

‡ J. Traube, *Zeitschr. anorg. Chem.* Bd. xl. p. 372 (1904).

are eliminated and the elements compared in their gaseous state*; I have further shown that† *to the changes in the properties which an element undergoes as it passes from the elementary state to that of a definite class of compound, there correspond throughout simultaneous volume changes.* All these facts, combined with the well-known defects of the periodic systems, have led me to enunciate the proposition—*The properties of the elements and their compounds are primarily functions of the atomic and molecular volumes.* A satisfactory systematic presentation must primarily be based on the relations connecting the volumes (and possibly also the shapes) of the atoms.

The fact that, according to the well-known volume curve of Lothar Meyer, the atomic volumes are periodic functions of the atomic weight, and that thereby all other properties become functions of a function, is best interpreted as follows:

When the elements are arranged according to their atomic weights, there are frequently found at periodic intervals elements whose atomic volumes and atomic shapes stand in simple numerical relationship; only when such is the case are the elements also related to each other in their remaining properties.

The periodic interval is thus by no means the essential determining factor; the determining factors are volume and shape of atom.

I would particularly draw attention to the fact that these views of mine regarding the periodic system are in excellent agreement with those of J. J. Thomson‡ regarding the structure of atoms—views which, though hypothetical, are strikingly original and worthy of all attention. J. J. Thomson shows, by building up the atoms out of negatively charged corpuscles, that at definite periodic intervals there recur atoms of similar constitution, and hence also similar space relations, and that it is these forms of matter whose properties are related. *It seems to me very remarkable that the views which I have for a long time advocated solely on the ground of my studies of volume-relations, should have been arrived at in a totally different way by so competent an authority, on the basis of the electron theory.*

I now wish to return to the investigations of Mr. Richards, and say a few words about them.

Starting from the compressibility of matter and the contraction of the atoms, Richards arrives, although considerably

* L. c. p. 375.

† "Raum der Atome," F. W. Ahrens *Samml. Chem. und Chem.-Techn. Vortr.* p. 14.

‡ J. J. Thomson, *Phil. Mag.* [6] vol. vii. p. 237 (1904); also 'Electricity and Matter.'

later than myself, yet independently, at the important relation between contraction and affinity. By means of the equation $Cdt = Kdv$ he calculates the "energy-quotient" K as a quantity which is proportional to the intrinsic pressure calculated by me from van der Waals' equation. In accord with myself, Richards points out that hardness, elasticity, and other properties vary in accordance with this intrinsic pressure. To Richards is, as already pointed out, due the great credit of having verified for the first time, in a large number of cases, the relations, among others, between the heats of formation and the atomic contractions already recognized by myself. As regards the relations connecting electromotive force, solution-pressure, and atomic contraction, Richards has arrived at results which accord with my own. Of fundamental interest is the already quoted memoir on the change of free energy, as well as the recently commenced experimental investigations on the coefficient of compressibility. Although I regret that Mr. Richards did not make himself acquainted with my work *before* commencing his own, yet, on the other hand, it is satisfactory to find that he has, on the whole, independently arrived at similar conclusions.

On one very important point only do we differ.

Richards remarks, *l. c.* vol. xlix. p. 17: "The bulk of Traube's reasoning is rendered difficult to follow by his hypothetical assumptions of 'co-volume,' 'core-volume,' 'bound' and 'free' æther, but nevertheless to him is due the credit of having recognized the importance of many of the facts."

In my opinion, the value of my views lies precisely in what Richards reproaches me with.

While I distinguish three volumes: internal, external, and co-volume, Richards assumes for the liquid and solid states only a single volume. He regards the atoms as something in the nature of elastic spheres, which are in contact and undergo contraction, without any intervention of a free space between them. The effect of heat manifests itself in condensations and rarefactions within the atom.

While I have proved the generality of the gaseous laws as expressed by van der Waals' equation, and their applicability to all three states of aggregation, from the assumptions made by Richards it would follow that this equation is not applicable to the liquid and solid states, since Richards equates the co-volume $v - b$ for both these states to zero, not only at the absolute zero of temperature, but at all temperatures.

But if Richards is right, how are we to account for the fact that the coefficient of expansion of the solid elements $= \frac{1}{273}$, provided the total expansion be referred not to the

total volume, but to the co-volume as calculated by myself? This result alone is enough to show the advantage which my views possess over those of Richards. The co-volume is, in fact, that part of the total volume which changes with temperature, while the true volume can only be altered by pressure.

Further, I would refer to all the other consequences which are rendered possible by the separate calculation of the co-volume, such as the calculation of the intrinsic pressure (this is proportional to Richards' "energy-quotient"), and the relation of the co-volume to the most varied properties. Had Richards separated the notions of co-volume and true volume, many of his investigations—especially that in *Zeitschr. Physik. Chem.* xlix. pp. 16 *et seq.*—would have taken a much simpler form.

Not only, however, the notion of co-volume, but also the consideration of the mutual relationship between external and internal volume must I defend against the views of Mr. Richards. Now I am not only in agreement with Mr. Richards, but may even claim priority regarding the view that not only the envelope but also the core of the atom is compressible; but the two ideas must be maintained separate—this is confirmed by the theories of van der Waals, and of Clausius, Mossotti, Exner, as well as by my verification of both theories.

Finally, I have already pointed out the agreement of my volume-theory with Fresnel's theory of a bound and free æther; and it also appears to me that the various æther theories, such as those of Lord Kelvin, are in agreement with my views, but not with those of Mr. Richards.

Charlottenburg, Technische Hochschule.

XLI. *The Electrical Resistance of a Conductor the Measure of the Current passing.* By W. A. PRICE, *Fellow Phys. Soc.**

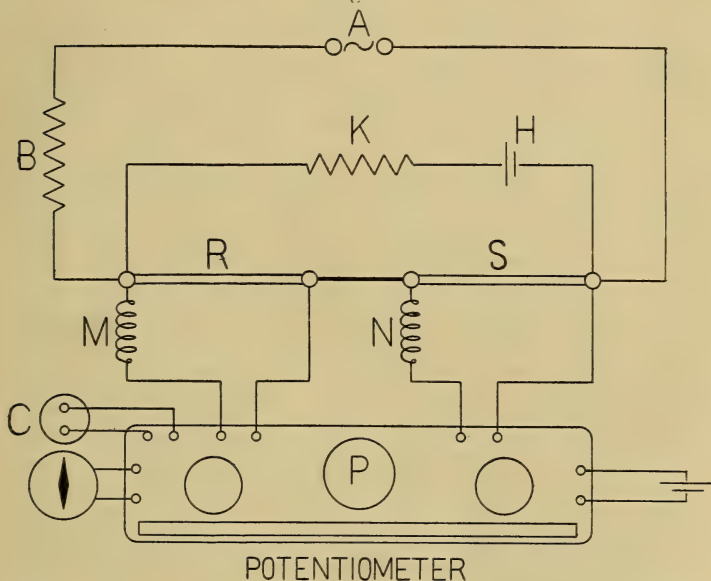
1. **C**HANGE in the length of a platinum wire is a convenient measure of the current, but the ordinary hot-wire instrument is not sufficiently precise for some purposes. The experiments in this paper were made by Mr. C. H. Wright, in Crompton & Co.'s works at Chelmsford, to ascertain whether the resistance of a conductor would be a better measure of the current than its length where considerable accuracy is required, the practical object in view being the calibration of alternating-current instruments. It was hoped to retain the advantage possessed by the dynamometer of calibration by direct currents, and thus of direct

* Communicated by the Physical Society : read May 12, 1905.

reference to the Clark cell and standard resistance ; to get the precision of other resistance measurements ; and to avoid the use of apparatus which is easily put out of adjustment.

2. To try whether the resistance of a conductor carrying alternating current could be easily measured, the connexions were arranged as in fig. 1. The mean fall of potential over R carrying a current from the alternating supply A was compared with that over S, a standard ; a small direct current being passed through the system from the accumulator H. The potential-differences of R and S were compared on the potentiometer. B, K were regulating resistances, and highly inductive coils at M, N the primaries of

Fig. 1.



small high-voltage transformers reduced the alternating currents in circuits where only direct-current effects were required. If the alternating current in R be I , and the direct current i , the heat developed $\propto I^2 + i^2$; so that if I be large compared with i , say 100 times as great, the heating effect of i is negligible. The experiments of fig. 1, in which I was several hundred milliamperes, and i was 1 milliampere, were designed to show whether the resistance of R carrying alternating current could be measured by a superimposed direct current so small that it would not affect the heat developed. It was found that using the Chelmsford town-supply for the alternating source observations could be made

without any difficulty, the widening of the galvanometer-line by the alternating current producing no appreciable inconvenience in reading. With the workshop supply readings were impossible, as the line was given a violent jerk every time a motor was started or stopped.

3. It was assumed that the temperature of R, a thin conductor, will be the same for a direct current as for an alternating current of the same root mean square; so that if R can be constructed to measure direct currents, it can be calibrated with those, and used to measure alternating currents. All the subsequent experiments were accordingly made to ascertain the conditions for its use with direct currents.

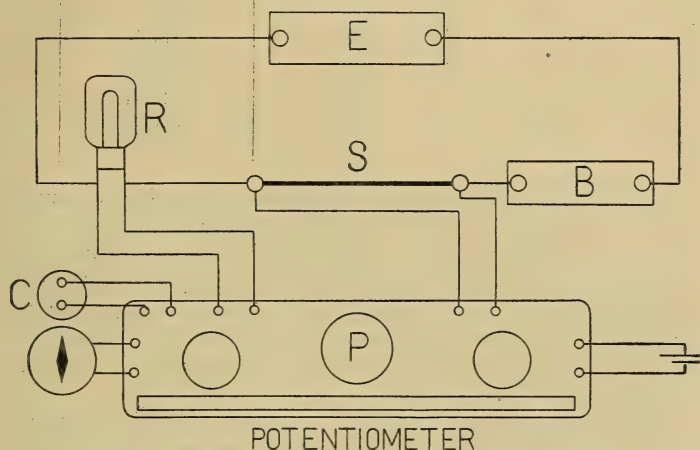
4. Silver was chosen for trial. Being a pure metal, its resistance changes rapidly with temperature, and its molecular condition is unlikely to change with repeated heating and cooling as might be the case with an alloy. Being a good conductor, it is heated rapidly by a low potential-difference. It was available as foil .5 mil thick, in which form it has a large radiating surface for a small heat capacity. In the end we found this foil gave irregular results, probably from being too fragile, and platinum-foil of 1 mil in thickness was used instead. A few preliminary experiments showed the results to be exceedingly sensitive to draughts; and a great many different arrangements were tried to protect conductors from irregular cooling by currents of air. Conductors of thin foil were enclosed in large vessels and in small, packed in mica slips and in slag-wool. The containing vessels were protected in various ways, or immersed in water or paraffin-oil. The net results were clear. If any considerable free body of air is near to the conductor, currents of air and irregular cooling are produced when the conductor is heated, and no consistent measurements are obtained. If, on the other hand, the containing vessel or any solid packing is near to the conductor, its heat capacity affects the temperature, which only slowly reaches a permanent value. These results indicate that the conductor must be contained in a relatively large vessel, exhausted of air, and suggest that an incandescent lamp may be suitable for small currents and large pressures, and a slip of metal foil for large currents and small potential-differences.

5. Experiments on the above lines were started on—

- A. Carbon-filament incandescent lamps.
- B. A platinum ligament in a metal box, exhausted by a Fleuss pump.
- C. A platinum ligament sealed into an exhausted glass bulb.

All these experiments were directed to the establishment of a relation between the resistance of the conductor and the direct current passing. The connexions are shown in fig. 2, where R is the conductor under test, shown, *e. g.*, as an incandescent lamp, S is a standard resistance of known value, B is a regulating resistance, E a storage-cell, and C a standard cell. The potential-differences of R, S, and the E.M.F. of C were compared on the potentiometer P for a series of different currents obtained by regulating B; and from the three readings the current through and the resistance of R are calculated. In each case there are four leads to the resistance, two carrying the current, and two which carry no current connected to the potential points, and the resistance measured is in every

Fig. 2.



case that between the potential points. The potentiometer lends itself admirably to experiments of this class, since all the data are obtained from adjustments on one calibrated wire, while the whole of the conductor whose resistance is measured may be inside the exhausted vessel, so that the leading-in wires are not involved. With a bridge the current could not be measured with the resistance, while the resistance of leading-in wires would have to be taken into account.

6. The resistance of a 50 volt, 8 c.p. Stearn lamp was measured with a small testing-current immersed in a paraffin-bath warmed up to 56° C. to obtain an idea of the change of resistance with temperature. The mean change over a range of 40° C. was -0.0224 per cent. per degree. In the resistance was included in this case that of the leading-in wires; and

it may be mentioned as a curiosity that a particular 4-volt lamp tested at the same time showed a very nearly constant resistance over the same range, the increase of the platinum and the decrease of the carbon resistance just balancing one another. The combination of a carbon lamp and a metallic conductor might be useful for some purposes as having a small or negligible temperature-coefficient.

7. To determine the effect of external temperature a 50-volt 8 c.p. lamp was tested in a paraffin-bath, with the following results:—

Current in milliamperes.	Resistance in ohms.	
	Bath at 7° C.	Bath at 50° C.
10	160	159
30	150·35	149·97
50	142·84	142·6
70	137·14	136·83
89·98	131·96	131·82

As would be expected, the effect of the external temperature diminishes as the current employed increases. A current of 110–120 milliamperes just made the above lamp incandescent, the above measurements being taken with the filament black. Observations on the lamp in air, quite unprotected, gave closely similar and consistent results, showing that air-draughts outside an exhausted bulb are of no appreciable importance.

8. To ascertain how far the resistance of the lamp is affected by use, the following results were obtained, on a lamp when new, and after it had been run for 6 hours and 17 hours respectively at its full voltage of 50 volts.

Current in milliamperes.	Resistance in ohms.		
	New in air at 11° C.	6 hrs. run, air 15° C.	17 hrs. run, air 15° C.
10	163	164·7	164·25
30	153·6	155·5	155
50	146·2	147·7	146·45
70	140·2	141·4	140·5
90	135·2		135·26

These experiments seem to indicate that these lamps might serve very usefully for measuring alternating currents from 50 up to a few hundred milliamperes, or even a thousand, but would not be instruments of much precision. The reason for the discrepancies was not then apparent.

9. Curves of three lamps of the same manufacture, taken to see how far they followed the same form, are given in

Fig. 3.

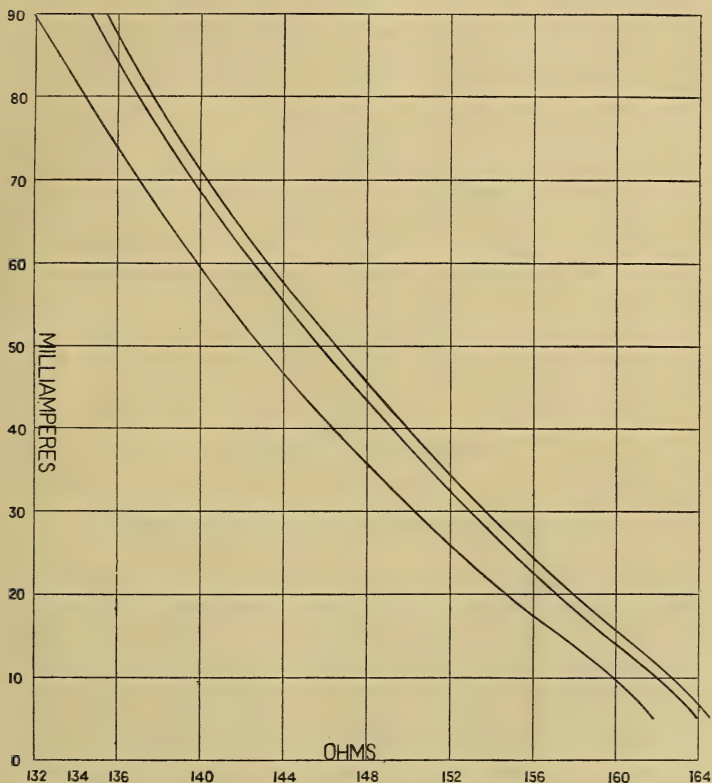
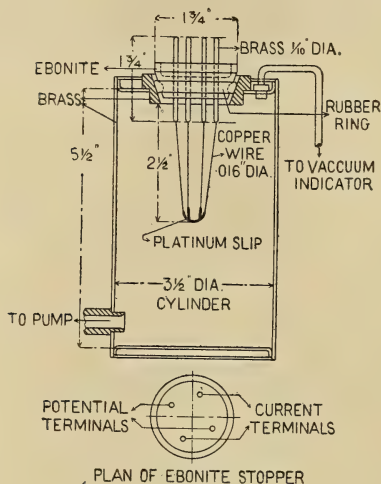


fig. 3, which indicates that possibly one formula connecting current and resistance might be employed for all lamps of one method of manufacture.

10. A platinum-foil ligament in a circular box, exhausted by a Fleuss pump, was then tried. The form of the ligament

and the arrangement of the box are shown in fig. 4. The joints between the rubber packing and the box, and also

Fig. 4.



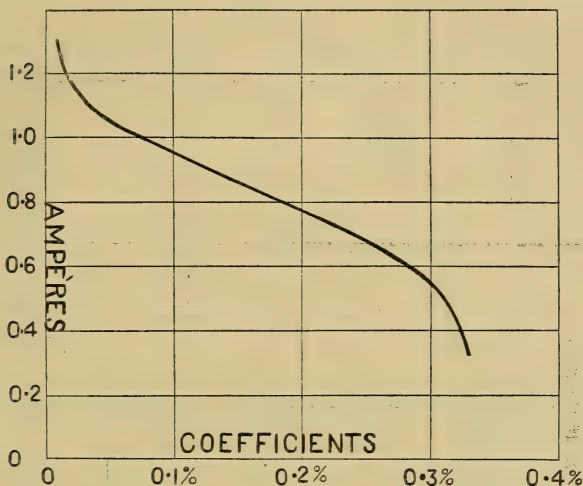
Enlarged Plan of Platinum Strip .001 inch thick.

between the brass wires and the ebonite cap, were sealed with stiff machine-oil before exhausting. Before this box was used a smaller and thicker one had been tried, of 2 inches diameter, which was found to give an appreciable lag, owing to the containing box being too near the ligament. In this the resistance of the filament continued to rise for 20 minutes under a steady current, and had not then attained its maximum. With the vessel of fig. 4 no change in the resistance was observed after the current had been applied for the shortest time necessary to obtain the first set of readings, which was about $\frac{3}{4}$ minute, so that there was no appreciable lag.

11. The results of experiments on the effect of outside air temperature on the conductor of fig. 4 are given in fig. 5. The results were not very consistent, only a mercurial thermometer being used, and the exact position of the curve is a

little uncertain; but its character is well defined. In the diagram the abscissæ are the percentage corrections to be applied to the resistance for each degree by which the air temperature differs from 15° . The resistance thus corrected is the value for an air temperature of 15° .

Fig. 5.



12. The results of experiments on the degree of exhaustion of the vessel are given in the diagram of fig. 6. It will be noticed how rapidly the resistance for a given current, 1.2 amperes, air temperature 15° C., increases with the exhaustion when the latter is fairly perfect. This might form the basis of a useful gauge for high vacua. In the next set of experiments the vacuum was actually gauged in this way, the pumping being started before each set of readings and continued until the resistance of the strip for a given current, 1 ampere, corrected for the air temperature from the figures of fig. 5, took the same value as before. The curve at normal atmospheric pressures is too steep to be of any use as a barometer.

13. On the diagram of fig. 7 are plotted a number of observations made on four different occasions of the resistance of the platinum strip of fig. 4; the vessel being exhausted to about .2 inch mercury, the exhaustion being fixed by the resistance at 1 ampere.

Fig. 6.

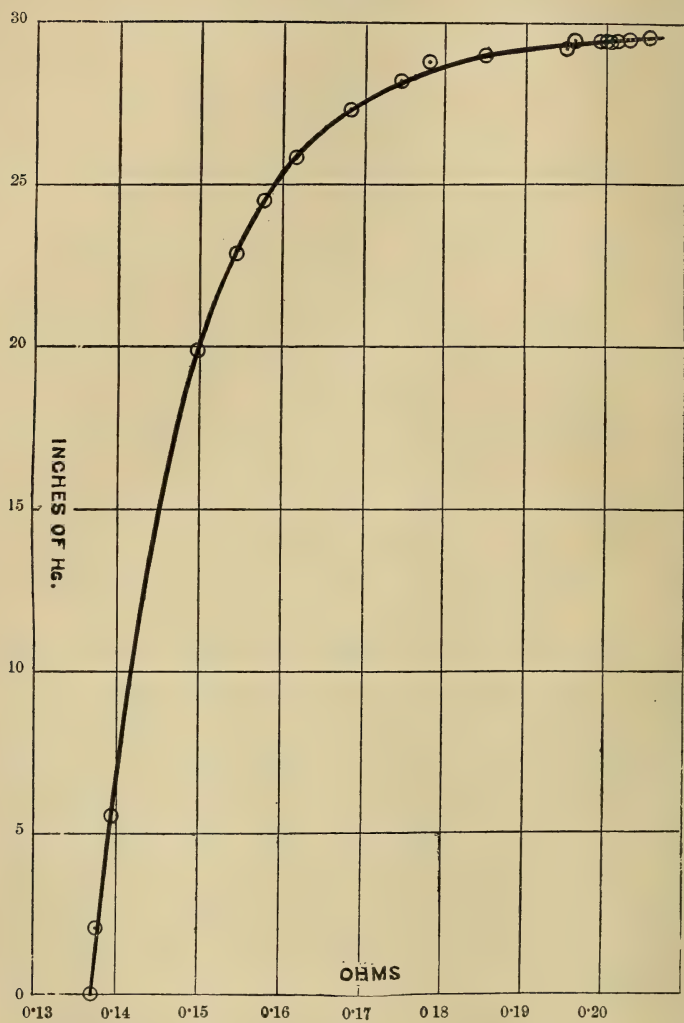
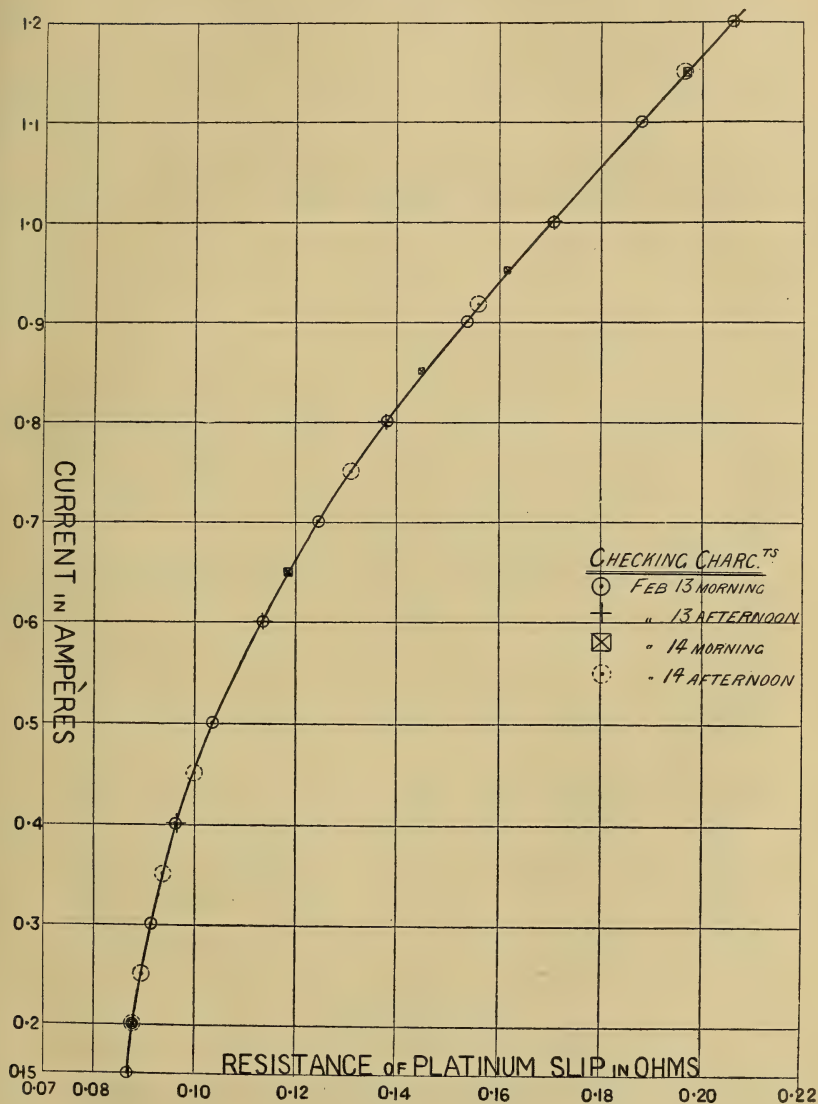
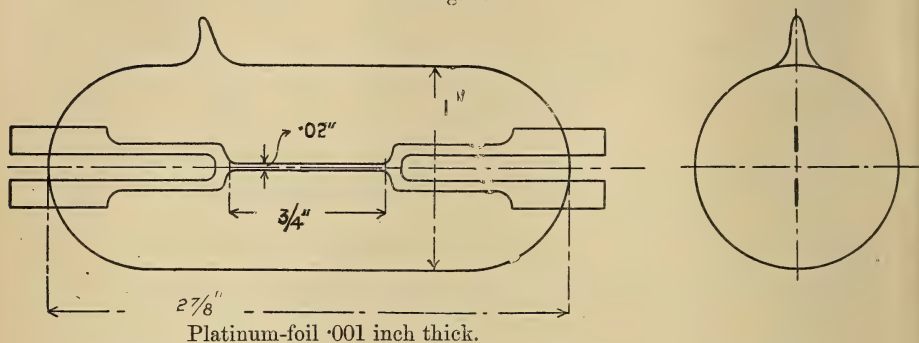


Fig. 7.



14. Experiments were then carried out on a platinum-foil ligament sealed into an exhausted glass vessel shown in fig. 8.

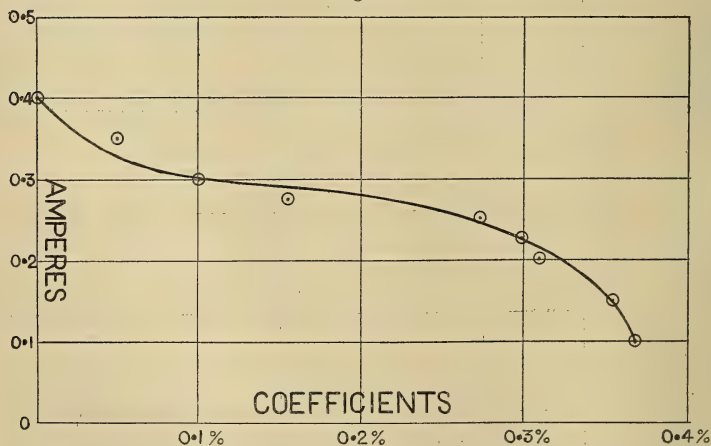
Fig. 8.



As before, two of the connexions sealed through the glass carry the current, and two are potential-leads and carry no current. The degree of exhaustion of the vessel was not known; but it was a high vacuum from a good mercury-pump, and much better than that obtained in the vessel of fig. 4 from a mechanical hand-pump.

15. Fig. 9 gives the results of experiments on the effect of

Fig. 9.



the air temperature on the resistance of the strip, the abscissæ being as in fig. 5 percentage corrections to reduce to an air temperature of 15° C. Two points are noticeable in this diagram. The effect of the air temperature becomes negligible for much lower current-densities in the highly exhausted

glass vessel than in the imperfectly exhausted brass one. The second point is that the extension of the curve to the x axis appears to indicate a temperature-coefficient of resistance for the metal of which the slip was composed, which was believed to be pure platinum, of about $\cdot 375$, which is higher than the accepted figure; so that the exact position of this curve, though it was very carefully taken, could not be relied on without confirmation.

16. The figures of the following table are different sets of measurements of the resistance of the slip of fig. 8, at various currents and after different treatment.

Current in milliamperes.	Resistance in ohms.			
	A.	B.	C.	D.
$\cdot 3$	$\cdot 32$	$\cdot 319$	$\cdot 318$	$\cdot 3155$
$\cdot 35$	$\cdot 368$	$\cdot 368$	$\cdot 363$	$\cdot 361$
$\cdot 4$	$\cdot 421$	$\cdot 420$	$\cdot 416$	$\cdot 413$
$\cdot 45$	$\cdot 479$	$\cdot 479$	$\cdot 474$	$\cdot 471$
$\cdot 5$	$\cdot 531$	$\cdot 534$	$\cdot 5295$	$\cdot 529$

The figures A were taken as the bulb was received from the maker; B were taken ten days afterwards, a good many experiments with currents not exceeding $\cdot 5$ ampere, alternating and direct, having been made in that time; C were taken shortly after B, the slip having been heated previously for one hour to a bright red by a current of $\cdot 9$ ampere; D were taken after a further heating for $\frac{3}{4}$ hour of the same current, $\cdot 9$ ampere. The resistance of the slip when cold was unchanged throughout. It had been raised to a bright-red heat before originally sealing into the glass bulb. It seems clear that the explanation of the changes between A, B, C, D must be looked for in the small quantities of gas occluded by the platinum before sealing in, and given off when subsequently heated *in vacuo*; and this effect may also explain the changes in the resistance of a carbon filament noticed in § 8 above. If a conductor of this kind is to be used as a gauge for the measurement of current, either, as Prof. Callendar has suggested to me, the exhaustion of the vessel must be poor, so that the absorption or occlusion of gas by the platinum does not make any appreciable change in the amount in the vessel, giving the case of the results of § 13; or else the exhaustion and sealing must be performed when the platinum is at a bright-red heat, so that no occluded gases are left. Either course seems to promise a satisfactory result.

XLII. On the Momentum and Pressure of Gaseous Vibrations, and on the Connexion with the Virial Theorem. By Lord RAYLEIGH, O.M., F.R.S.*

IN a paper on the Pressure of Vibrations (Phil. Mag. iii. p. 338, 1902) I considered the case of a gas obeying Boyle's law and vibrating within a cylinder in one dimension. It appeared that in consequence of the vibrations a piston closing the cylinder is subject to an additional pressure whose amount is measured by the volume-density of the total energy of vibration. More recently, in an interesting paper (Phil. Mag. ix. p. 393, 1905) Prof. Poynting has treated certain aspects of the question, especially the momentum associated with the propagation of progressive waves. Thus prompted, I have returned to the consideration of the subject, and have arrived at some more general results, which however do not in all respects fulfil the anticipations of Prof. Poynting. I commence with a calculation similar to that before given, but applicable to a gas in which the pressure is any arbitrary function of the density.

By the general hydrodynamical equation (Theory of Sound, § 253 *a*),

$$\varpi = \int \frac{dp}{\rho} = - \frac{d\phi}{dt} - \frac{1}{2}U^2 \quad . \quad . \quad . \quad (1),$$

where p denotes the pressure, ρ the density, ϕ the velocity-potential, and U the resultant velocity at any point. If we integrate over a long period of time, ϕ disappears, and we see that

$$\int \varpi dt + \frac{1}{2} \int U^2 dt \quad . \quad . \quad . \quad . \quad (2)$$

retains a constant value at all points of the cylinder. The value at the piston is accordingly the same as the mean value taken over the length of the cylinder.

If p_1, ρ_1 denote the pressure and density at the piston, and p_0, ρ_0 the pressure and density that would prevail throughout were there no vibrations, we have

$$p = f(\rho) = f(\rho_0 + \rho - \rho_0) \quad . \quad . \quad . \quad (3),$$

and approximately

$$\begin{aligned} \varpi &= \int \rho^{-1} f'(\rho) d\rho = \int \frac{f'(\rho_0 + \rho - \rho_0)}{\rho_0 + \rho - \rho_0} d(\rho - \rho_0) \\ &= \frac{\rho - \rho_0}{\rho_0} f'(\rho_0) + \frac{(\rho - \rho_0)^2}{2\rho_0^2} \{ \rho_0 f''(\rho_0) - f'(\rho_0) \} \quad . \quad (4). \end{aligned}$$

* Communicated by the Author.

For the mean value of ϖ at the piston we have only to write ρ_1 for ρ in (4) and integrate with respect to t . And at the piston $U=0$.

For the mean of the whole length l of the cylinder (parallel to x), we have to integrate with respect to x as well as with respect to t . And in the integration with respect to x the first term of (4) disappears, inasmuch as the mean density remains the same as if there were no vibrations. Accordingly

$$f'(\rho_0) \int \frac{\rho_1 - \rho_0}{\rho_0} dt = \frac{1}{2} \iint \frac{U^2 dx dt}{l} + \{ \rho_0 f''(\rho_0) - f'(\rho_0) \} \left\{ \iint \frac{(\rho - \rho_0)^2 dx dt}{2\rho_0^2 l} - \int \frac{(\rho_1 - \rho_0)^2}{2\rho_0^2} dt \right\} \quad (5),$$

the terms on the right being of the second order in the quantities which express the vibration.

Again,

$$\begin{aligned} \int (p_1 - p_0) dt &= \int \{ f(\rho_1) - f(\rho_0) \} dt \\ &= \rho_0 f'(\rho_0) \int \frac{\rho_1 - \rho_0}{\rho_0} dt + \rho_0^2 f''(\rho_0) \int \frac{(\rho_1 - \rho_0)^2}{2\rho_0^2} dt; \end{aligned}$$

so that by (5)

$$\begin{aligned} \int (p_1 - p_0) dt &= \frac{\rho_0}{2} \iint \frac{U^2 dx dt}{l} \\ &+ \{ \rho_0^2 f''(\rho_0) - \rho_0 f'(\rho_0) \} \iint \frac{(\rho - \rho_0)^2 dx dt}{2\rho_0^2 l} + \rho_0 f'(\rho_0) \int \frac{(\rho_1 - \rho_0)^2}{2\rho_0^2} dt \\ &\quad \dots \dots \dots (6). \end{aligned}$$

The three integrals on the right in (6) are related in a way which we may deduce from the theory of infinitely small vibrations. If the velocity of propagation of such vibrations be denoted by a , then $f'(\rho_0) = a^2$. By the usual theory we have

$$U = \frac{d\phi}{dx}, \quad \frac{\rho - \rho_0}{\rho_0} = -\frac{1}{a^2} \frac{d\phi}{dt} \quad \dots \dots (7).$$

If we suppose that the cylinder is closed at $x=0$ and at $x=l$, a normal vibration is expressed by

$$\phi = \cos \frac{s\pi x}{l} \cdot \cos \frac{s\pi at}{l} \quad \dots \dots (8),$$

where s is any integer, giving

$$\frac{a^2}{2} \int \frac{(\rho_1 - \rho_0)^2}{\rho_0^2} dt = a^2 \iint \frac{(\rho - \rho_0)^2}{\rho_0^2} \frac{dx dt}{l} = \iint \frac{U^2 dx dt}{l} \quad (9),$$

the integrations with respect to x in (9) being taken from 0 to l , that is over the length of the cylinder.

The same conclusions (9) follow in the general case where ϕ is expressed by a sum of terms derived from (8) by attributing an integral value to s . The latter part expresses the equality of the mean potential and kinetic energies.

Introducing the relations (9) into (6), so as to express the mean pressure upon the piston in terms of the mean kinetic energy, we get as the final formula

$$\int (p_1 - p_0) dt = \left\{ \rho_0 + \frac{\rho_0^2 f''(\rho_0)}{2 f'(\rho_0)} \right\} \iint \frac{U^2 dx dt}{l} \quad (10).$$

Among special cases let us first take that of Boyle's law, where $p = a^2 \rho$, so that

$$f'(\rho_0) = a^2, \quad f''(\rho_0) = 0.$$

We have at once

$$\int (p_1 - p_0) dt = \rho_0 \iint \frac{U^2 dx dt}{l} \quad (11)$$

The expression on the right represents double the volume-density of the kinetic energy, or the volume-density of the whole energy, and we recover the result of the former investigation.

According to the adiabatic law

$$p/p_0 = (\rho/\rho_0)^\gamma \quad (12);$$

so that

$$f'(\rho_0) = \frac{p_0 \gamma}{\rho_0}, \quad f''(\rho_0) = \frac{p_0 \gamma (\gamma - 1)}{\rho_0^2} \quad (13).$$

Hence from (10)

$$\int (p_1 - p_0) dt = \frac{1}{2}(\gamma + 1) \rho_0 \iint \frac{U^2 dx dt}{l} \quad (14).$$

The mean pressure upon the piston is now $\frac{1}{2}(\gamma + 1)$ of the volume-density of the total energy. We fall back on Boyle's law by taking $\gamma = 1$.

It appears therefore that the result is altered when Boyle's law is departed from. Still more striking is the alteration when we take the case treated in 'Theory of Sound' § 250

of the law of pressure

$$p = \text{Const.} - a^2 \frac{\rho_0^2}{\rho} \quad . \quad . \quad . \quad . \quad . \quad (15).$$

According to this

$$f'(\rho_0) = a^2, \quad f''(\rho_0) = -2a^2/\rho_0 \quad . \quad . \quad . \quad (16),$$

and (10) gives

$$\int (p_1 - p_0) dt = 0 \quad . \quad . \quad . \quad . \quad (17).$$

The law of pressure (15) is that under which waves of finite condensation can be propagated without change of type.

In (17) the mean additional pressure vanishes, and the question arises whether it can be negative. It would appear so. If, for example,

$$p = \text{Const.} - \frac{a^2 \rho_0^3}{2\rho^2} \quad . \quad . \quad . \quad . \quad (18),$$

$$f'(\rho_0) = a^2, \quad f''(\rho_0) = -3a^2/\rho_0,$$

and

$$\int (p_1 - p_0) dt = -\frac{1}{2}\rho_0 \iint \frac{U^2 dx dt}{l} \quad . \quad . \quad . \quad (19).$$

I now pass on to the question of the *momentum* of a progressive train of waves. This question is connected with that already considered; for, as Prof. Poynting explains, if the reflexion of a train of waves exercises a pressure upon the reflector, it can only be because the train of waves itself involves momentum. From this argument we may infer already that momentum is not a necessary accompaniment of a train of waves. If the law were that of (15), no pressure would be exercised in reflexion. But it may be convenient to give a direct calculation of the momentum.

For this purpose we must know the relation which obtains in a progressive wave between the forward particle velocity u (not distinguished in one-dimensional motion from U) and the condensation $(\rho - \rho_0)/\rho_0$, usually denoted by s . When the disturbance is infinitely small, this relation is well known to be $u = as$, in the case of a positive wave. Thus

$$u : s = \sqrt{dp/d\rho} \quad . \quad . \quad . \quad . \quad (20).$$

The following is the method adopted in 'Theory of Sound,' § 351:—"If the above solution be violated at any point a wave will emerge, travelling in the negative direction. Let us now picture to ourselves the case of a positive progressive wave in which the changes of velocity and density are very gradual but become important by accumulation, and let us

inquire what conditions must be satisfied in order to prevent the formation of a negative wave. It is clear that the answer to the question whether, or not, a negative wave will be generated at any point will depend upon the state of things in the immediate neighbourhood of the point, and not upon the state of things at a distance from it, and will therefore be determined by the criterion applicable to small disturbances. In applying this criterion we are to consider the velocities and condensations not absolutely, but relatively, to those prevailing in the neighbouring parts of the medium, so that the form of (20) proper for the present purpose is

$$du = \sqrt{\left(\frac{dp}{d\rho}\right)} \cdot \frac{d\rho}{\rho} \quad . \quad . \quad . \quad . \quad . \quad (21),$$

whence

$$u = \int \sqrt{\left(\frac{dp}{d\rho}\right)} \cdot \frac{d\rho}{\rho} \quad . \quad . \quad . \quad . \quad . \quad (22),$$

which is the relation between u and ρ necessary for a positive progressive wave. Equation (22) was obtained analytically by Earnshaw (Phil. Trans. 1859, p. 146).

In the case of Boyle's law, $\sqrt{(dp/d\rho)}$ is constant, and the relation between velocity and density, given first, I believe, by Helmholtz, is

$$u = a \log (\rho/\rho_0),$$

if ρ_0 be the density corresponding to $u=0$."

In our previous notation

$$dp/d\rho = f'(\rho) = a^2 + f''(\rho_0) \cdot (\rho - \rho_0),$$

a being the velocity of infinitely small waves, equal to $\sqrt{\{f'(\rho_0)\}}$; and by (22)

$$u = a \frac{\rho - \rho_0}{\rho_0} + \frac{a}{\rho_0} \left(\frac{f''(\rho_0)}{2a^2} - \frac{1}{\rho_0} \right) \frac{(\rho - \rho_0)^2}{2} \quad . \quad (23),$$

the first term giving the usual approximate formula.

The momentum, reckoned per unit area of cross section,

$$= \int \rho u \, dx = \rho_0 \int \left(1 + \frac{\rho - \rho_0}{\rho_0} \right) u \, dx.$$

Introducing the value of u from (23) and assuming that the mean density is unaltered by the vibrations, we get

$$\rho_0 \left\{ \frac{\rho_0 f''(\rho_0)}{4a} + \frac{a}{2} \right\} \int \frac{(\rho - \rho_0)^2}{\rho_0^2} \, dx \quad . \quad . \quad (24),$$

or, if we prefer it,

$$\frac{\rho_0}{a^2} \left\{ \frac{\rho_0 f''(\rho_0)}{4a} + \frac{a}{2} \right\} \int u^2 dx \quad . \quad . \quad . \quad (25).$$

The total energy of the length considered is

$$\rho_0 \int u^2 dx ;$$

and the result may be thus stated

$$\text{momentum} = \left\{ \frac{\rho_0 f''(\rho_0)}{4a^3} + \frac{1}{2a} \right\} \times \text{total energy} \quad (26).$$

This may be compared with (10). If we suppose the long cylinder of length l to be occupied by a train of progressive waves moving towards the piston, the integrated pressure upon the piston during a time t , equal to l/a , should be equal to twice the momentum of the whole initial motion. The two formulæ are thus in accordance, and it is unnecessary to discuss (26) at length. It may suffice to call attention to Boyle's law, where $f''(\rho_0)=0$, and to the law of pressure (15) under which progressive waves have *no momentum*. It would seem that pressure and momentum are here associated with the tendency of waves to alter their form as they proceed on their course.

The above reasoning is perhaps as simple as could be expected; but an argument to be given later, relating to the kinetic theory of gases, led me to recognize, what is indeed tolerably obvious when once remarked, that there is here a close relation with the virial theorem of Clausius. If x, y, z be the coordinates; v_x, v_y, v_z the component velocities of a material particle of mass m , then

$$\frac{1}{2} \Sigma m v_x^2 = -\frac{1}{2} \Sigma Xx + \frac{1}{4} \frac{d^2 \Sigma m x^2}{dt^2}$$

with two similar equations, X being the impressed force in the direction of x operative upon m . If the motion be what is called stationary, and if we understand the symbols to represent always the mean values with respect to time, the last term disappears, and

$$\frac{1}{2} \Sigma m v_x^2 = -\frac{1}{2} \Sigma Xx \quad . \quad . \quad . \quad (27).$$

The mean kinetic energy of the system relative to any direction is equal to the virial relative to the same direction.

Let us apply (27) to our problem of the one-dimensional motion of a gas within a cylinder provided with closed ends.

As in other applications of the virial theorem, the forces X are divided into two groups, internal and external. The latter reduces to the forces between the ends (pistons) and the gas. If p_1 be the pressure on the pistons—it will be the same on the average at both ends—the external virial is per unit of area $\frac{1}{2}p_1l$ simply. As regards the internal virial, I do not remember to have seen its value stated, probably because in hydrodynamics the mechanical properties of a fluid are not usually traced to forces acting between the particles. There can be no doubt, however, what the value is. If we suppose that the whole mass of gas in (27) is at rest, the left-hand member vanishes, so that the sum of the internal and external virial must vanish. Under a uniform pressure p_0 , the internal virial is therefore $\frac{1}{2}p_0l$. In an actual gas the virial for any part can depend only on the local density, so that whether the gas be in motion or not, the value of the internal virial is

$$-\frac{1}{2}\int_0^l p \, dx \quad . \quad . \quad . \quad . \quad . \quad (28).$$

Hence (27) gives

$$\begin{aligned} \text{kinetic energy} &= \frac{1}{2}p_1l - \frac{1}{2}\int_0^l p \, dx \\ &= \frac{1}{2}(p_1 - p_0)l - \frac{1}{2}\int_0^l (p - p_0) \, dx \quad . \quad (29). \end{aligned}$$

If the gas be subject to Boyle's law, pressure is proportional to density, and the last term in (29) disappears. The additional pressure on the ends ($p_1 - p_0$) is thus equal to twice the density of the kinetic energy.

In general,

$$p - p_0 = a^2(\rho - \rho_0) + \frac{1}{2}f''(\rho_0) \cdot (\rho - \rho_0)^2,$$

and

$$\int_0^l (p - p_0) \, dx = \frac{1}{2}f''(\rho_0) \int (\rho - \rho_0)^2 \, dx.$$

If we introduce expressly the integration with respect to t already implied, (29) gives

$$\begin{aligned} l \int (p_1 - p_0) \, dt &= \rho_0 \iint U^2 \, dx \, dt + \frac{1}{2}f'''(\rho_0) \iint (\rho - \rho_0)^2 \, dx \, dt \\ &= \left\{ \rho_0 + \frac{\rho_0^2 f'''(\rho_0)}{2a^2} \right\} \iint U^2 \, dx \, dt, \end{aligned}$$

regard being paid to (9). Equation (10) is thus derived very simply from the virial.

In all that precedes, the motion of the gas has been in one dimension, and even when we supposed the gas to be confined in a cylinder, we were able to avoid the consideration of lateral pressures upon the walls of the cylinder by applying the virial equation in its one-dimensional form. We now pass on to the case of three dimensions, and the first question which arises is as to the value of the virial. In place of (27) we have now

$$\frac{1}{2} \sum m U^2 = -\frac{1}{2} \sum (Xx + Yy + Zz) \quad . \quad . \quad (30),$$

U being the resultant velocity, Y, Z impressed forces parallel to the axes of y and z . Let us first apply this to a gas at rest under pressure p_0 . The total virial, represented by the right-hand member of (30), is now zero; that is, the internal and external virial balance one another. As is well known and as we may verify at once by considering the case of a rectangular chamber, the external virial is $\frac{3}{2}p_0v$, v denoting the volume of gas. The internal virial is accordingly $-\frac{3}{2}p_0v$; and from this we may infer that whether the pressure be uniform or not, the internal virial is expressed by

$$-\frac{3}{2} \iiint p \, dx \, dy \, dz, \quad . \quad . \quad . \quad (31).$$

The difference between the internal virial of the gas in motion and in equilibrium is

$$-\frac{3}{2} \iiint (p - p_0) \, dx \, dy \, dz \quad . \quad . \quad . \quad (31^*).$$

According to the law of Boyle, (31^{*}) must vanish, since the mean density of the whole mass cannot be altered. The internal virial is therefore the same whether the gas be at rest or in motion.

A question arises here as to whether a particular law of pressure may not be fundamentally inconsistent with the statical Boscovitchian theory of the constitution of a gas upon which the application of the virial theorem is based. If, indeed, we assume Boyle's law in its integrity, the inconsistency does exist. For Maxwell has shown (Scientific Papers, vol. ii. p. 422) that on a statical theory Boyle's law involves between the molecules of a gas a repulsion inversely as the distance. This makes the internal virial for any pair of molecules independent of their mutual distance, and thus the virial for the whole mass independent of the distribution of the parts. But such an explanation of Boyle's law violates the principle upon which (31) was deduced, making the pressure dependent upon the total quantity of the mass and not merely upon the local density; from which

Maxwell concluded that all statical theories are to be rejected. It is to be remarked, however, that our calculations involve the law of pressure only as far as the term involving the square of the variation of density, and that a law agreeing with Boyle's to this degree of approximation may perhaps not be inconsistent with a statical Boscovitchian theory*.

Passing over this point, we find in general from (30)

$$\frac{1}{2} \Sigma m U^2 = \frac{3}{2} (p_1 - p_0) v - \frac{3}{2} \iiint (p - p_0) dx dy dz \quad (32),$$

whenever the character of the motion is such that the mean pressure (p_1) is the same at all points of the walls of the chamber. Further, as before,

$$\iiint (p - p_0) dx dy dz = \frac{1}{2} f''(\rho_0) \iiint (\rho - \rho_0)^2 dx dy dz,$$

and finally, regard being paid to (9) as extended to three dimensions,

$$(p_1 - p_0) v = \left(\frac{1}{3} + \frac{\rho_0 f''(\rho_0)}{2\alpha^2} \right) \times \text{total energy} \quad (33).$$

In the case of Boyle's law $f''=0$, and we see that the mean pressure upon the walls of the chamber is measured by one-third of the volume-density of the total energy.

For the adiabatic law (12), (13) gives

$$(p_1 - p_0) v = \left(\frac{1}{3} + \frac{\gamma - 1}{2} \right) \times \text{total energy} \quad (34).$$

In the case of certain gases called *monatomic*, $\gamma = 1\frac{2}{3}$, and (34) becomes

$$(p_1 - p_0) v = \frac{2}{3} \times \text{total energy} \quad (35).$$

Thirdly, in the case of the law (15) for the relation between pressure and density,

$$(p_1 - p_0) v = -\frac{2}{3} \times \text{total energy} \quad (36),$$

the mean pressure upon the walls being *less* than if there were no motion.

So far we have treated the question on the usual hydrodynamical basis, reckoning the energy of compression or

* I think the difficulty may be turned by supposing the force, inversely as the distance, to operate only between particles whose mutual distance is small, and that outside a certain small distance the force is zero. All that is necessary is that a pair of particles once within the range of the force should always remain within it—a condition easily satisfied so long as small disturbances alone are considered.

rarefaction as *potential*. It was, however, on the lines of the kinetic theory that I first applied the virial theorem to the question of the pressure of vibrations. In the form of this theory which regards the collisions of molecules as instantaneous, there is practically no potential, but only kinetic, energy. And if the gas be monatomic, the whole of this energy is translational. If V be the resultant velocity of the molecule whose mass is m , the virial equation gives

$$\frac{3}{2}p_1v = \frac{1}{2}\Sigma mV^2 \quad . \quad . \quad . \quad . \quad (37),$$

p_1 denoting, as before, the pressure upon the walls, assumed to be the same over the whole area. If necessary, p_1 and ΣmV^2 are to be averaged with respect to time.

It is usually to a gas in equilibrium that (37) is applied, but this restriction is not necessary. Whether there be vibrations or not, p_1 is equal to $\frac{2}{3}$ of the volume-density of the whole energy of the molecules. Consider a given chamber whose walls are perfectly reflecting, and let it be occupied by a gas in equilibrium. The pressure is given by (37). Suppose now that additional energy (which can only be kinetic) is communicated. We learn from (37) that the additional pressure is measured by $\frac{2}{3}$ of the volume-density of the additional energy, whether this additional energy be in the form of heat, equally or unequally distributed, or whether it take the form of mechanical vibrations, *i. e.* of coordinated velocities and density differences. Under the influence of heat-conduction and viscosity the mechanical vibrations gradually die down, but the pressure undergoes no change.

The above is the case of the adiabatic law with $\gamma=1\frac{2}{3}$ already considered in (35), and a comparison of the two methods of treatment, in one of which potential energy plays a large part, while in the other all the energy is regarded as kinetic, suggests interesting reflexions as to what is really involved in the distinction of the two kinds of energy.

If we abandon the restriction to monatomic molecules, the question naturally becomes more complicated. We have first to consider in what form the virial equation should be stated. In the case of a diatomic molecule we have, in the first instance, not only the kinetic energy of the molecule as a whole, but also the kinetic energy of rotation, and in addition the internal virial of the force by which the union of the two atoms is maintained. It is easy to see, however, that the two latter terms balance one another, so that we are left with the kinetic energy of the molecule as a whole. For

general purposes a theorem is required of which I have not met a complete statement. For any part of a wider system for which we wish to form the virial equation, we may omit the kinetic energy of the motion relative to the centre of gravity of the part, if at the same time we omit the virial of the internal forces operative in this part and treat the forces acting from outside upon the part, whether from the remainder of the system or wholly from outside, as acting at the centre of gravity of the part. In applying (37) to a gas regarded as composed of molecules, we are therefore to include on the right only the kinetic energy of translation of the molecules. If a gas originally at rest be set into vibration, we have

$$\frac{3}{2}(p_1 - p_0)v = \text{additional energy of translation.} \quad (38).$$

The pressure p_1 does not now, as in the case of monatomic gases, remain constant. Under the influence of viscosity and heat-conduction, part of the energy at first translational becomes converted into other forms.

A complete discussion here would carry us into the inner shrine of the kinetic theory. We will only pursue the subject so far as to consider briefly the case of rigid molecules for which the energy is still entirely kinetic—partly that of the translatory motion of the molecules as wholes and partly rotatory. Of the additional energy E representing the vibrations, half may be regarded as wholly translational. Of the other half, the fraction which is translational is $3/m$, where m is the whole number of modes. The translational part of E is therefore $\frac{1}{2}E(1 + 3/m)$; so that

$$(p_1 - p_0)v = E\left(\frac{1}{3} + \frac{1}{m}\right) \quad . \quad . \quad . \quad . \quad (39).$$

If $m=3$, as for monatomic molecules, we recover the former result; otherwise $p_1 - p_0$ is less. In terms of γ we have

$$\gamma = 1 + 2/m \quad . \quad . \quad . \quad . \quad (40),$$

and accordingly

$$(p_1 - p_0)v = E\left(\frac{1}{3} - \frac{\gamma - 1}{2}\right) \quad . \quad . \quad . \quad . \quad (41),$$

in agreement with (34) where what was there called the total energy is now regarded as the additional energy of vibration. In the case of a diatomic gas, $m=5$, $\gamma=1\frac{2}{5}$.

XLIII. *On the Absorption of the β and γ Rays of Actinium.*
 By T. GODLEWSKI, Ph.D. (Cracow)*.

THE numerous investigations of the absorption of β rays emitted by thorium and radium have established the fact that the β rays of these elements are complex in their character and consist of electrons projected with a wide range of velocities. It has been shown that their penetrating power does not remain constant but increases with the thickness of matter traversed. Consequently, the absorption does not follow an exponential law, such as is shown by the more homogeneous rays of uranium. In the case of radium and thorium, the value of the absorption constant is dependent on the thickness of the matter traversed. For instance, the following values of the absorption constant λ for aluminium were found by Eve† for the β rays from radium:—

Thickness in mms.	λ cm. ⁻¹ .
0 to .85	6.5
.85 „ 2.5	5.7
2.5 „ 4.1	1.6

As we see, the value of λ varies in a marked manner with the thickness of matter previously traversed.

In my previous paper on actinium, I pointed out that the activity of actinium, measured by the β rays, decreases accurately according to an exponential law with the thickness of traversed matter. Thus, in this respect, the β rays of actinium are distinguished from the β rays of other elements.

The measurements were made in the following way:—About 2 grams of emanating substance of Giesel (of activity about 300 times that of uranium) were spread uniformly on the bottom of a dish (7 cms. in diameter). The dish was covered with a very thin plate of mica, which was sealed with wax, in order to prevent the escape of the emanation.

The measurements of activity were made by means of sensitive electroscope of the type of C. T. R. Wilson. The bottom of the electroscope was removed and replaced by an aluminium plate, 0.08 mm. thick, which absorbed all the α rays. The dish, containing the actinium, was placed under the electroscope and covered with the plates of metal whose absorption power was to be examined.

* Communicated by Prof. E. Rutherford, F.R.S.

† Phil. Mag. Dec. 1904.

The results are graphically represented in fig. 1, where the ordinates give the logarithms of the activity expressed in

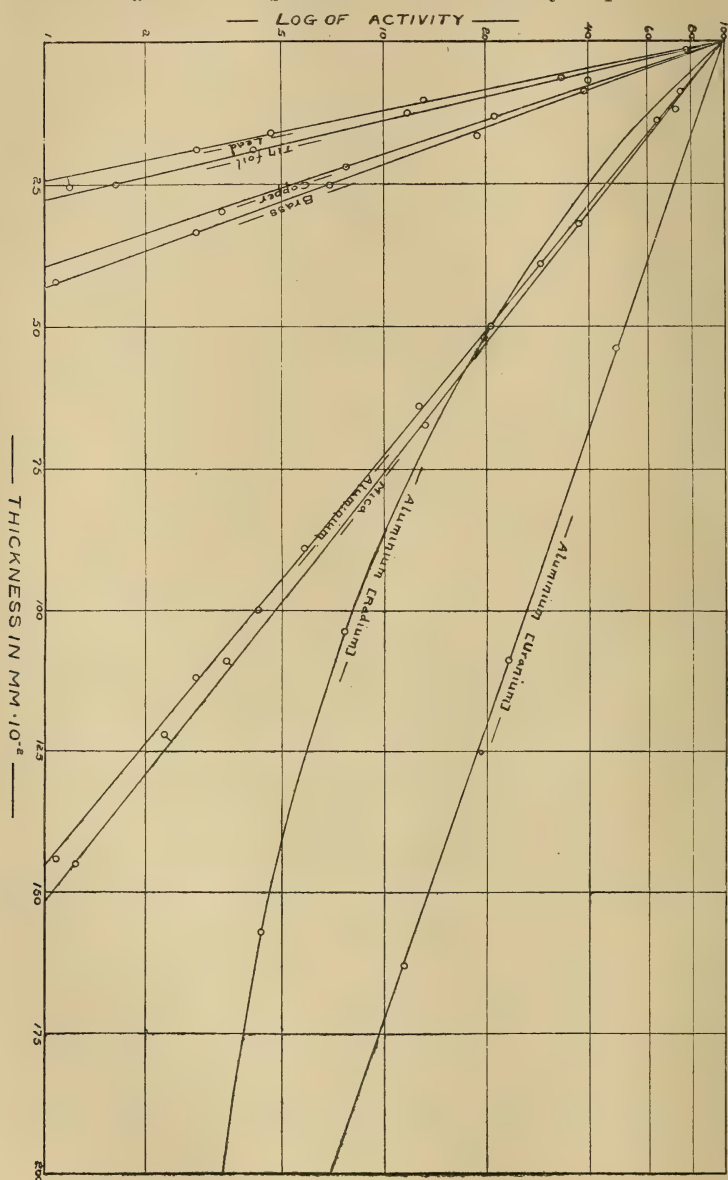


Fig. 1.

percentages of total activity, the abscissas the thickness of the absorbing matter. For comparison, the corresponding

curves of absorption for the β rays of radium and uranium are also given.

The experimental points obtained for the β rays of actinium lie on a straight line. The equation $I = I_0 e^{-\lambda x}$, where x is the thickness, is applicable even when I has a value smaller than one per cent. of I_0 . This indicates that the β rays from actinium are homogeneous in character, and that their penetrating power does not change with the thickness of matter traversed.

The second characteristic property of the β rays of actinium is their relatively small penetrating power. While the β rays from uranium are half absorbed in passing through the thickness of 0.5 mm. of aluminium, the β rays of actinium are reduced to half value in 0.21 mm. Thus the β rays of actinium have only 40 per cent. of the penetrating power of those of uranium. A still greater difference is observed when we compare the penetrating power of the β rays of actinium and radium. The average penetrating power of the β rays from radium is more than three times as great as for actinium. It must be pointed out, however, that the value of λ for radium rays changes very considerably with the thickness traversed. From fig. 1 we see that the initial portion of the absorption curves for radium and actinium rays are nearly the same. This shows that some of the β rays of radium are of the same penetrating power as those from actinium.

TABLE II.

Substance.	ACTINIUM.			URANIUM.		RADIUM.	
	Thickness of metal in mms. $\times 10^{-2}$ to absorb half the rays.	λ cm. $^{-1}$.	$\frac{\lambda}{d}$.	λ .	$\frac{\lambda}{d}$.	λ .	$\frac{\lambda}{d}$.
Aluminium .	21.2	32.7	12.9	14.0	5.4	11.6	4.30
Mica	21.0	33.0	12.0	17.2	5.1	10.8	3.94
Brass	6.5	108	13.1	...	...	...	...
Copper	6.3	139	15.9	60	7.0	49.2	5.50
Tinfoil	4.5	154	15.7	...	...	...	...
Lead	4.25	163	14.1	122	10.8	62.5	5.48

A reference to the absorption constants λ given in Table II. for different substances traversed by the β rays of actinium shows that the absorption is nearly proportional to the

density d . For comparison, the values of λ and $\frac{\lambda}{d}$ for uranium given by Rutherford *, and for radium given by Strutt †, are added.

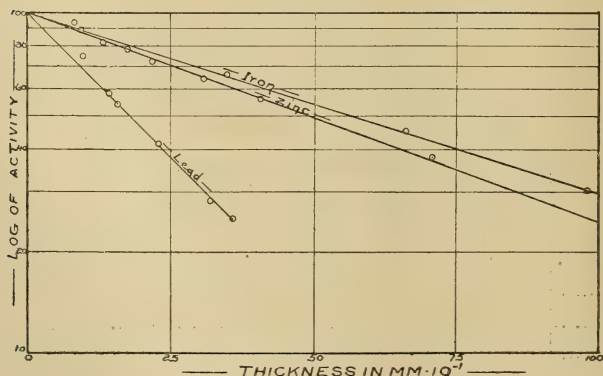
We see that although the value of $\frac{\lambda}{d}$ is not constant, its variations are considerably smaller than in the case of the other radio-elements. Thus the absorption law of density holds better for the β rays of actinium than for those of radium and uranium.

γ Rays of Actinium.

The absorption of the γ rays was also investigated in a similar way. The ratio of the ionization produced by the β rays in the electroscope employed to that produced by the γ rays of actinium was found to be about 300. The absorption of the γ rays could not be investigated over so wide a range as that of the β rays, owing to the smallness of the effect produced.

The results are graphically represented in fig. 2, where the ordinates give the logarithms of the γ activity, expressed in per cent. of the total γ activity, and the abscissæ the thickness of absorbing matter. The initial value of the γ activity was obtained by graphical extrapolation of the curves.

Fig. 2.



It is seen that the activity measured by the γ rays decreases according to an exponential law with the thickness traversed. This law is applicable over the range examined, viz., to

* Rutherford, 'Radioactivity,' p. 114.

† Strutt, Nature, 1900, p. 439.

30 per cent. of the initial value. In order to show whether the γ rays of actinium are entirely homogeneous, it would be necessary to repeat the experiment, using a more active preparation of this substance.

The γ rays of actinium differ also very considerably from the γ rays emitted by other radioactive elements in having a very small penetrating power. The following table gives the constants of absorption, and the values of $\frac{\lambda}{d}$ for the metals investigated; for comparison the corresponding values for radium* are also given.

TABLE III.

Metal.	ACTINIUM.			RADIUM.			
	Thickness of metal in mms. to absorb half of the rays.	λ cm. $^{-1}$.	$\frac{\lambda}{d}$.	Less penetrating.		More penetrating.	
				λ .	$\frac{\lambda}{d}$.	λ .	$\frac{\lambda}{d}$.
Iron ...	5.70 mms.	1.23	0.16				
Zinc ...	5.60 „	1.24	0.17	0.28	0.039	0.24	0.033
Lead ...	1.92 „	4.54	0.40	0.64	0.056	0.44	0.037

We see from this table that the γ rays of actinium have a penetrating power of only about one-tenth of that of more penetrating rays of radium. This unusually small penetrating power of the γ rays of actinium may perhaps be the cause of the great divergence from the density law for these rays†. Further experiments are in progress to examine the behaviour of the β and γ rays of actinium in a magnetic field. An investigation of this character may perhaps throw some light on the peculiarities of these rays.

In conclusion the writer has pleasure in acknowledging the kind advice received from Prof. Rutherford.

McGill University, Montreal.

April 7, 1905.

* See McClelland, Phil. Mag. July 1904, p. 67.

† See J. J. Thomson, 'Conduction of Electricity through Gases,' p. 312; also McClelland, Phil. Mag. July 1904, p. 67.

XLIV. *A Fundamental Experiment in Electricity.* By Prof. A. M. WORTHINGTON, C.B., F.R.S., Royal Naval Engineering College, Devonport*.

THE object of the experiment which I desire to record was to ascertain whether any difference could be detected between space at a high electrical potential and space at a low potential, quite irrespective of the existence of any electric field in the space in question.

Any easily noticeable difference was not to be expected. Such would probably have been long since discovered; for, if it exists, it can only correspond to some small residue of energy not accounted for in the well-established relations of the electric field.

To most people the question may seem to have been settled once for all by the experiments recorded by Faraday in his 'Experimental Researches,' vol. i. p. 1174, in which he describes how he constructed a hollow, insulated, conducting cube, inside which he went, "and lived in it, and using lighted candles, electrometers, and all other tests of electrical states, could not find the least influence upon them, or indication of anything particular given by them, though all the time, the outside of the cube was powerfully charged, and large sparks and brushes were darting off from every part of its outer surface."

It must be observed, however, that what Faraday was looking for was some sign of what he called an absolute charge—i. e., as he explains, a charge existing by itself and not connected by lines of induction with an equal charge of opposite sign; and it will be noticed that the tests he mentions were tests of "electrical states." He does not seem to have employed tests of other physical conditions.

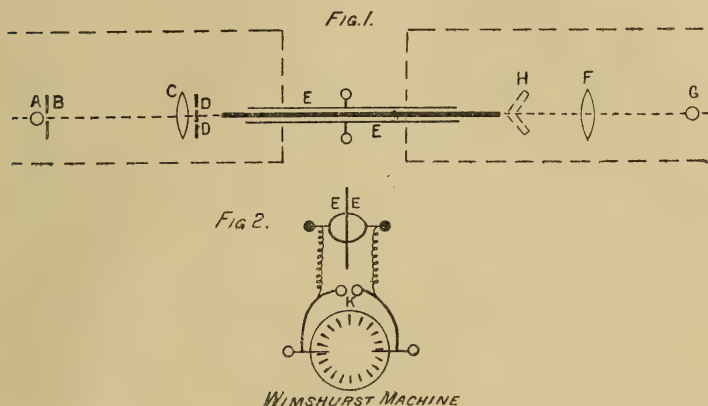
It therefore seemed to me worth while to examine whether any difference could be detected in the velocities of light passing through two tubes, the interior of one of which was at a high, and of the other at a low potential.

The arrangement employed was that of a Rayleigh interferometer, and is shown in the diagram.

The light of a Cooper-Hewitt mercury-vapour lamp A passes through a fine vertical slit B, at the focus of a collimating lens C. (This lens was an achromatic object-glass of 126 cm. focal length.) The light then passes, as a beam of parallel rays, to the two fine vertical slits D D, and thence through the two open brass tubes E E, each 5 feet (152·4 cm.) long, and separated by a partition of ebonite about 2 mm.

* Communicated by the Author.

thick, 183 cm. long, and 30 cm. wide. The two beams are then received by the telescope-lens F (a Steinheil achromatic) 28.4 in. (72.1 cm.) focal length, and produce interference-bands in the focal plane of a cylindrical eye-lens G, made of a well-worked glass rod about 4 mm. in diameter.



The brass tubes were half-cylinders, obtained by splitting a complete tube about 5 cm. in diameter and 3 mm. thick into two equal halves, by sawing it from end to end. The sawn edges were then planed flat, and between the two halves was placed the sheet of ebonite, along the middle of either side of which was pasted a strip of tinfoil a little wider and a little longer than the tubes. The edges of the tubes were pressed firmly against the tinfoil, and the tubes were held in position by a series of stout insulating ebonite bars carried by a light wooden frame, not shown in the diagram. In this way the separating wall of ebonite was prevented from warping and was kept pretty accurately plane, at any rate along its middle part, which was necessary on account of the nearness to each other of the two slits D D'.

From the middle of the outside of each tube projected a brass rod with a knob, and these knobs could be connected with a Wimshurst induction-machine (see fig. 2). When the machine was worked a potential-difference was established between the two tubes, and this increased until a spark passed between adjustable knobs at K, which were kept about $1\frac{1}{4}$ inches apart (30 mm.). The optical apparatus at one end, and one end of the frame carrying the tubes and partition, rested on a table standing on a base of concrete, the other end of the frame and the remainder of the optical parts on a similar table; the

electrical machine was placed below the middle of the tubes between these two tables.

The experiment consisted in watching the interference-bands as the potential-difference rose and then suddenly fell on the passage of the spark, and looking for a shift of the bands. To aid in the detection of the shift, the edge of a fine silk fibre, fixed in the middle of the field of the cylindrical eyepiece, served as a fiducial line. When care was taken not to touch either table, no shift whatever could be detected either when the spark occurred or while the potential-difference was accumulating.

With the slits D D about 4 mm. apart the bands as viewed were just about 1 degree of arc apart, and a shift of $\frac{1}{20}$ of this distance could have been easily detected. Sometimes the slits D D were placed wider apart and the beams brought together by means of a parallel plate Jamin separator shown at H in fig. 1. With this arrangement the separation of the middle of the bands was about $\frac{2}{3}$ of a degree, but the definition was not so good. Sometimes the slits D D were traversed by the light *after* it had passed through the tubes, instead of before; but the result was always the same.

The difference of potential when the spark passed was ascertained to be about 60,000 volts. The wave-length of the light to which the predominating part of the illumination was due may be taken as 57×10^{-6} cm., and the experiment therefore shows that a difference of potential of 60,000 volts does not, in a length of 152 cm. of air, produce a relative retardation of as much as $\frac{1}{20}$ of 57×10^{-6} cm.; so that, if there is any difference in the velocities through the tubes, it is less than $\frac{1}{152} \times \frac{1}{20} \times \frac{57}{10^6}$ of the whole, *i. e.* less than $\frac{1}{53}$ of one-millionth of the whole.

Observations on the Experiment.

It is satisfactory to notice that the observations were not appreciably disturbed by any secondary effects due to displacement of the optical parts through the action of electric forces. Even if such effects had, however, been observed, they could have been distinguished from the effect sought, through their remaining unaltered when the charges of the tubes were reversed in sign.

The sensitiveness of the experiment could probably be considerably increased by extending the length of the tubes, for this should not increase any secondary disturbance due to electrification. There might be some difficulty in procuring

very long sheets of ebonite: indeed that which I used had to be specially rolled. But it is not necessary for the tubes and sheets to be continuous; a succession of short lengths could be employed. I think, however, that a practical limit must exist to the total length of tube that can be advantageously employed, for the definition was always better when the tubes were removed, and was better with tubes 1 foot long than with tubes 5 feet long, a fact which must, in part at any rate, be attributed to reflexion of diffracted and scattered light, at grazing incidence from the partition into the field of the eyepiece. It was to diminish the effect of such reflexion that in some experiments the slits D D were set wider apart and the beams subsequently brought nearer together by the parallel-plate Jamin device. But the gain was apparently more than compensated by the loss due to the dispersion, by refraction, of each beam in traversing the Jamin plates, since the light was not homogeneous. With a homogeneous source, if one bright enough can be found, this would not happen.

July 22, 1905.

XLV. The *Æther* "Drift" and Rotary Polarization.

By D. B. BRACE*.

MASCART†, some thirty-five years ago, first examined the effect of the motion of the earth on double circular refraction, using R- and L-quartz, from which he concluded that the rotary power of quartz is not altered by the $\frac{1}{20000}$ part when the ray is *reversed* in the direction of motion of the earth.

Lord Rayleigh‡ recently repeated this experiment, also using quartz, in which he found that such a *reversal* does not alter the rotation by $\frac{1}{100000}$ part.

By using an active oil—the oil of caraway—and reversing the ray so as to compensate for rotary dispersion, I have been able by means of a sensitive-strip analyser, giving a much higher accuracy, to carry the limit certainly twenty-five and probably fifty times as far, so that we may conclude that the effect of the motion of the earth on the rotation in active substances is certainly less than $\frac{1}{5000000}$ and probably less than $\frac{1}{10000000}$ of the total rotation.

Lorentz§, in his early analysis, gives two first order terms,

* Communicated by the Author.

† *Ann. de l'Ecole Normale*, vol. i. p. 157 (1872).

‡ *Phil. Mag.* Aug. 1902, p. 215.

§ *Versuch einer Theorie*, Leiden (1895), p. 118.

in one of which this coefficient is the ratio of the velocities of light in quartz and in space, or approximately two-thirds. The presence, however, of two first order terms might imply still a zero first order effect if these terms completely compensated one another. It is difficult, however, to see how this could be; and hence, from such a mode of reasoning, we might still expect to find a residual first order effect which would be represented by the aberration term with a small coefficient, and which even the refined experiment of Rayleigh could not show. In a later paper, Lorentz*, in a reply to a criticism of Larmor†, gives the relation of the action between two elements arising from the electric forces which must obtain in order that the earth's motion may not influence the rotation of the plane of polarization to the first order. As this is not the only possible mode of action, and as further it is only true when second order terms are neglected, a definite conclusion can only be reached by direct experimental examination.

While the following test does not fully attain a second order sensibility, it does establish the absence of first order terms, or, at least, a compensation up to one part in five hundred and probably to one part in one thousand. Instead of quartz, which both Mascart and Rayleigh used, I employed finally an active liquid, the oil of caraway-seed ($\alpha_D = +103^\circ 33'$ per decimetre). Although its rotary power is much less than quartz ($\alpha_D = 21^\circ 67$ per millimetre), I found it preferable in the arrangement used. Both Mascart and Rayleigh point out some of the difficulties in such an experiment, involving as it does such enormous rotations. Thus, with the five pieces of quartz which Rayleigh used, the total rotation for sodium light was more than 5500° , and this would give a difference of rotation for the two D-lines of 11° , thus making the use of such a source impossible. He actually used the yellow helium line. Even then the field in his half-shade plane polarizer was "decidedly inferior to that obtainable when the quartzes were removed." This inferiority due to residual light in the field seems to have originated more largely in imperfections in the quartzes themselves. We should infer from this that, even if the dispersion due to the actual rotation were compensated for, the field would, on account of such imperfections, be far less dark than that attainable with a perfectly homogeneous substance. This was borne out by

* Amsterdam *Akad. v. Wet.* March 29, 1902, p. 669.

† 'Æther and Matter,' Cambridge, 1900, pp. 214-215; also *Phil. Mag.* Sept. 1902, p. 367.

my own experience in attempting to use quartz successfully in the experiment.

It is well known that if the direction of propagation of a ray in an active substance is changed by a single reflexion, the rotation is completely compensated for after passing over the same distance. If such a compensation could be brought about and still allow the influence of the earth-motion to be unaffected, we could use white light, and hence approximate to the normal polariscopic sensibility of say $0^{\circ}01$ or less, a sensibility usually some ten times that attainable with homogeneous light from the same radiant under like conditions. My first plan was to mount two equal lengths of the active substance (in this case, quartz cylinders) at right angles to each other, with one in the direction of drift, and then rotate the system through 180° . This arrangement would give complete compensation if the details of the system could be realized. As this plan involved a change in the direction of the ray of 90° , reflecting surfaces were necessary; and since, with white light, the azimuths of the various vibrations would be at different incidence on the reflecting surface, the rotations produced by this surface would, in general, follow a different law from that of quartz, so that, to this extent, compensation would be prevented. If, however, these rays should all strike a second surface at the same incidence, but in azimuths which are the complements of those at the first surface, the effect due to reflexion would be completely compensated for. This of course may be realized by making the planes of incidence at right angles to one another. Thus, if the plane of the paths within the active media is horizontal, the plane of incidence of the first mirror may be vertical, for example, and the ray be sent upward and then horizontally from the second mirror, the plane of incidence of the latter being also vertical but in an azimuth of 90° to the incident plane of the first. Since now a single reflexion reverses an axial displacement, *i. e.*, a right-handed to a left-handed one, a second reflexion would reverse this to a right-handed one again, thus restoring the relative order, and so on. An odd number of reflexions therefore reverses the relative axial displacement, while an even number restores it* This

* Mascart, in his experiment, realized a reversal by means of a half-wave plate, following the method of Fizeau and Foucault, who used the two parallelepipeds of Fresnel instead. He was in this way able to use consecutively R- and L-quartz, their effects being added together. The total rotation which he thus obtained was approximately 6300° for the green thallium line. This rotation is somewhat greater than that obtained by Rayleigh, but the former's polariscopic sensibility was several times less, depending, as it did, on the appearance or disappearance of the thallium line as seen with a spectroscope between crossed nicols.

principle of reversal is often lost sight of in connexion with rotary polarization problems. Thus it is true that by a single reflexion the rotation of a ray is neutralized in returning through an active substance and doubled in a magnetic one. If, however, two reflexions were used, as may sometimes occur, the relative rotation for the different colours would be doubled in active substances and destroyed in a magnetic field. The total rotation of any one ray, however, would depend on its azimuth, which changes sign with respect to the normal to the incident and reflected rays, at each reflexion. The resultant rotation would be the sum of the initial and final azimuths of the vibrations on entering and leaving the reflecting system. White light, returned through an active substance by two mutually perpendicular surfaces, would not show a recomposition, as it does with a single reflexion. Thus, in the arrangement proposed (fig. 1), equal lengths of right- and left-handed substances, *e.g.* quartz, would give perfect colour neutralization, as well as plane-polarized light, if the double-mirror compensation, described above, were used. As a length of one metre for each cylinder was contemplated, the question of the realization of the normal sensibility of a half-shade system had to be answered. This requires, for its application, a uniform field of sensible area, a condition apparently unrealizable with paths of such great lengths in double-refracting substances. In this case, we should have the well-known spirals resulting from the passage of white light through equal lengths of R- and L-quartz, since the number of reflexions is even in this case and they do not enter. However, these spirals, at the centre, intersect each other at right angles, and a partial compensation at this point of the field might be realized by means of a crystal of opposite sign placed so as to form a "crossed" system with the quartz cylinder, the cross in the former being tangent to the spiral of the latter for a short distance from the optical axis. It was proposed to use Iceland spar of the proper thickness to give the best result, if this mode of compensation should prove effective. Quartz is positive and the difference in index between its two rays is only about one-twentieth that of spar, which is negative, so that a cylinder of the latter of much less length would be sufficient. As the area available in the field would necessarily be small, the amount of material for cutting could be reduced by using a small cross-section. Samples of both R- and L-quartz crystals of various sizes were secured, mainly from the Hot Springs region of Arkansas. These were cut into prisms (something over a total length of one metre for each kind) approximately parallel to the

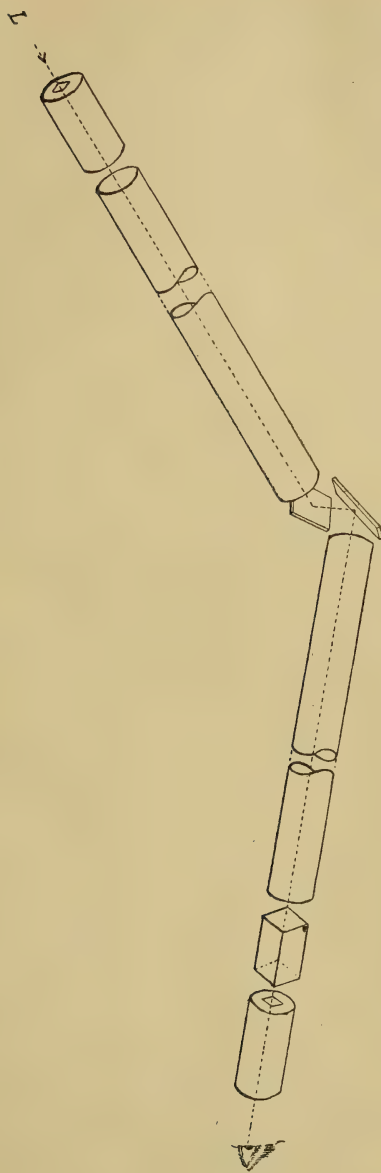


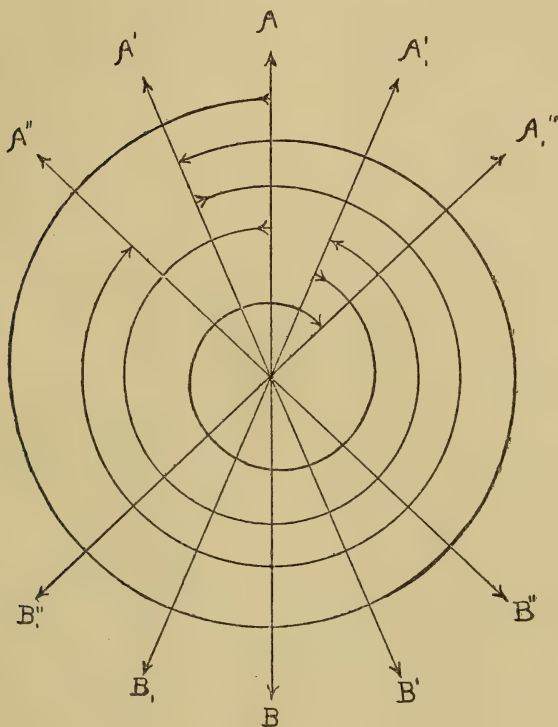
Fig. 1.

optic axis, and their ends polished in order to make a preliminary examination of their optical qualities. Many of these showed traces of the double spiral over various points of their end faces, indicating the presence of both R- and L-formations to a greater or less extent, so that perhaps less than a third of the cuttings could be used. The entire lot was finally sent to an expert cutter, who declined to undertake, with this material, the making of cylinders which should give the optical conditions necessary for realizing the normal polariscopic sensibility. Cylinders one-fourth as long, built up from sections some 5 cm. in length of purer material from Swiss sources, were proposed. Even then the final outcome as to the polariscopic sensibility was much in doubt, while the importance of the extension of this test did not seem to me to warrant the great expense which the proposal contemplated. The use of quartz was finally abandoned for other available active materials. These I sought among the active oils.

The success of the same plan, as proposed for quartz, seemed somewhat doubtful, since we should need two R- and L-substances which for suitable lengths—not necessarily equal—would give identical rotary dispersions. With the great rotations proposed, any slight relative irrationality would at once make itself evident by the corresponding increase of the residual light in the field of the half-shade, which would present the same order of difficulty as that from impure quartz. Such oils would also have to be very clear indeed to allow the passage of sufficient light through the much greater distances necessary to produce the same total rotation as in quartz. Several of the commercial oils fulfil these conditions to a greater or less extent. Two of these, caraway oil, $\alpha_D = +103^\circ 33'$, and eucalyptus oil, $\alpha_D = -52' 22''$, as prepared by Schimmel & Co., Miltitz-Leipzig, are quite colourless and suited to the above arrangement. The expense of the latter (8 marks per pound) precluded its use, but the former (1 mark per pound) was entirely available, if the optical conditions referred to above could be met by a single substance. It did not at first occur to me that an arrangement for testing the "drift" was possible with a single substance which would allow the use of white light, thus giving the normal polariscopic sensibility. Both of the previous experimenters had failed to make use of this idea. If the motion of the earth produces an effect on the rotation of the plane of polarization, we should of course expect this effect to be reversed on changing the direction of motion. Thus the plane of a ray propagated along the drift would show a

slight increment, say ; then, if it be reflected back, there would be a corresponding decrement over what would occur if either there were no motion or the direction of propagation were across the drift. Thus in fig. 2, if AB is the initial direction of vibration, and if the active substance rotate it through a whole number of circumferences without motion, it would be

Fig. 2.



slightly increased, if propagated along the drift say, and the emergent vibration be in the direction of $A'B'$. If now this ray is reflected back by a single reflector against the drift, it would be rotated in the opposite direction (to a fixed observer) by the same number of circumferences less the angle between AB and $A'B'$, vibrating on emerging in the direction of $A''B''$. Hence we should have a recombination of all the colours to this extent. If now the optical system be turned

about through 180° , the first passage would make the emergent vibration along $A_1'B_1'$, since now the propagation is against the drift. On reflexion the final vibration would be along $A_1''B_1''$. Thus, on reversing the optical system, we double the effect. Hence we are entitled to reflect the ray backward and forward, compensation being attained when the total distance in the liquid in one direction is equal to that in the other. Such an arrangement contains at once the possibilities of far greater sensibilities than any previous attempts.

The same mounting for the optical system was used as in my former experiment "on the Double Refraction of Matter moving through the *Æther*"*. The optical system, however, was somewhat different.

Instead of using plane-reflecting mirrors, as before, which allowed the beam of light to spread too much, with the considerable distances gone over, concave reflectors mounted with adjusting screws were used instead; their centres of curvature being approximately at the axis of rotation of the mount (fig. 3). The distance between the two sets of mirrors at the end of the trough was 410 cm., and it required some 60 to 70 pounds of the oil to fill the trough. This arrangement conserved the light without allowing it to spread out continuously, which would have required a much larger trough and consequent amount of liquid. This would have produced greater distortion in the ray by its passage through larger portions of the liquid. A brass trough whose section was like the lower half of an octagonal cylinder was mounted within the wooden trough, used in the experiment referred to. This was just large enough to carry at its ends three mirrors of two inches aperture arranged together with their centres at the points of an equilateral triangle with its vertex down. In order to avoid depolarization from reflexion and double refraction, the polarizing and analysing systems were mounted in the bottom of brass L tubes, which could be partly immersed in the oil. The vertical axis of the first L was coincident with the axis of rotation of the trough. A circular beam of sunlight from a heliostat H, converging at the axis of rotation E, was reflected downward by the prism R, mounted on an universal carrier fixed to an arm I, coaxial with the axis of rotation. The prism T reflected the ray horizontally through the polarizing nicol P, thence through the thin cover-glass C out into the liquid to the first concave

* Phil. Mag. April 1904, p. 317.

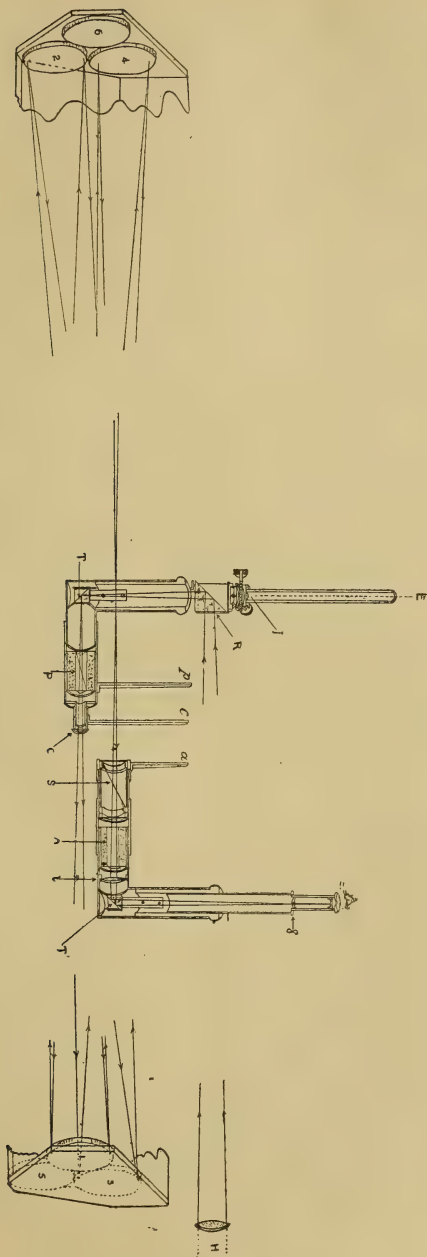


Fig. 3.

mirror 1. This reflected it back, through a convergence point in the axis of rotation below the brass L, to the second concave mirror 2, and so on; the last mirror reflecting it, through its point of convergence, successively into the sensitive-strip cell S, the analysing nicol A, the condensing lens l , the total reflecting prism T' and the focus f of the lens l , and, finally, either into the eye or the observing telescope. The tube carrying the thin cover-glass C could be adjusted by the arm c so that the latter was exactly in the plane of the front face of S, thus giving complete rotary compensation for the two opposite paths of the rays within the liquid. The polarizing nicol P could be rotated by means of an arm p , attached to its retaining tube, so that it might be crossed with the analyser A or varied at will. An extension arm, some 73 cm. long and carrying an index, could be fastened to this, and its rotation measured on a scale fixed upon the trough. The strip S, upon which my sensibility depended, I have already described*. This particular strip, which was mounted so as to cover half the field of view, was 15 mm. wide and 45 mm. long and 0.1 mm. thick, and was remarkable in the sharpness of its edge. It was cut so that its plane of greatest length made an angle of 70° with the optic axis, and placed in the cell in such a way that the ray traversed the strip at right angles to its optic axis. Instead of using α -monobromonaphthaline, whose index is exactly that of the ordinary ray, I used carbon disulphide, which had been carefully clarified. The index of the latter is only slightly below the ordinary ray, but not enough to cause total reflexion for any of the ordinary rays. This liquid is much clearer than the former, and gives superior definition and a better vanishing line. The end plates, which were of the thinnest cover-glass, were carefully tested for double refraction, after being cemented on, and the cell filled with liquid. To this cell was fastened an arm, a (fig. 3) with which it could be rotated so as to vary the azimuth made by the principal plane of the strip with that of the analysing nicol, and thus the intensity of the field of view, settings for a match being made with the arm p of the polarizer. The submerged joints were cemented with a mixture of glycerine and gelatine to prevent ingress of the oil to the nicols and prisms. Preliminary adjustments of the beam of light were usually made before filling the trough with liquid, after which the ray had to be readjusted in order to bring it back into the field of view again. Much care had to be exercised in

* Phil. Mag. Jan. 1903, p. 161.

the axial adjustment of the system and the beam of sunlight, as, if the latter were not perfectly uniform in intensity over its section, a rotation of the trough would show a spurious effect in the half-shade, destroying the match. This difficulty was many times greater than would have been experienced with an artificial source which could have moved with the system; but the increased light allowed a much smaller azimuth of the strip and a consequent increase in the sensibility. It was found that the light, when it reached the analyser, was slightly depolarized; and I sought to correct this with a thin mica compensator, in the way I have already described*, placed over the end of the cell S; but this did not eliminate it, showing that the light was irregularly depolarized. The compensator was therefore dispensed with. A portion of this residual light came from diffusion, by the liquid, of the intense initial beam of sunlight within the same. The rest was due to imperfect compensation of all the rays arising from differences in density. The disturbing effect on the field of this residual light was, however, small, and did not interfere seriously with the settings for a match. At the beginning of a set of observations, the two halves of the field might show complementary colours on rotating the polarizer, indicating inequality between the combined paths. The arm *c* was then shifted until the complementary colours disappeared on rotating P in both directions, the entire field having a bright orange hue.

To avoid magnetic effects due to the earth's field, the trough was placed normal to the mean magnetic meridian. A compass, mounted in a slide upon the edge of the trough, could be set at equal intervals along its length and the position of the needle noted. This was found to vary by several degrees. The mean for the various positions was taken and found to be 21° N.E., so that the effect of the drift could be reduced only a few per cent. The trough was then set at right angles and always reversed in this direction.

Much difficulty was experienced on account of the ingress of the oil to the nicols and other optical pieces. On this account, and also because the oil, if left in contact with the air for some time, turns a yellowish colour, the trough was always filled and emptied at each observation. The residue on the mirrors and optical pieces also produced a scattering of the light, reducing the final sensibility perhaps by a half; so that all pieces had to be dismounted, cleaned, and re-cemented, before remounting and refilling the trough for a

* *Phil. Mag. L. c.*

second set of observations. Notwithstanding these precautions, the mirrors had to be resilvered and the oil gained access to the nicols and the strip and attacked the cement. This showed itself by a thin capillary line gradually encroaching on the field of view of the nicol and the final loosening of the sensitive-strip within its cell. This placed a definite limit to the number of observations which I was able to make and also to the extent of the paths within the trough, on account of the time necessary to make adjustments with the entire number of mirrors.

In most of the observations, I used a small telescope of some two or three diameters. I found that my eye was most sensitive if removed from the telescope during a reversal of the trough, thus relieving it from fatigue during the interval. On account of local disturbances within the liquid, the trough was turned very slowly, the time occupied in a reversal being usually a minute or more. It was then noted whether the beam of light followed exactly the same path as before and adjustments made, if necessary. This could be determined by its position with reference to a definite mark. The eye was then placed at the telescope, and the field carefully examined for any change in the two halves at some selected point in its centre, on the dividing or vanishing line. Settings for a match were also made by means of the arm p , and the index read by an assistant. While the edges of the field might show a slight contrast in the two halves, the central portion was very uniform and the bounding line, practically, a vanishing one, so that settings could be made to a high degree of accuracy. The strip used was the finest I have ever seen, and gave a dividing line far superior to any half-shade plane-polarizing system I have had the opportunity of working with. A half-shade "Lippich," placed in the same position, was entirely inadequate to the sensibility required. This was partly due to the fact that such a system, to give a vanishing line, must have in general a much larger source than a point radiant. It might also be mentioned that, if the half-shade system had been next to the polarizer, instead, definition would have been impossible through such a length of liquid. The use of such a thin strip, *e. g.* 0.1 mm., in a liquid of the same index, makes it possible to invert the order with respect to the active substance and still obtain a vanishing line as well as perfect definition; just as was done in my previous experiment with a half-shade elliptical polarizing strip placed after the substance examined. In the preliminary observations made during April, light was sent

only twice through the trough. Although the liquid appears perfectly clear and colourless, the absorption through this length was very marked and the intensity much reduced. That a large part of the light was due to internal scattering, was made evident by the bright shaft of light, seen in the liquid between the polarizer and first mirror, showing along its length the various orders of colour corresponding to the reflexion of plane-polarized light in successive azimuths from small particles.

The light, however, was sufficient to make successive settings to $1/120^\circ$, and the quality of the rotatory compensation of the liquid indicated that greater lengths were practicable. Such observations were taken on different days between 11 A.M. and 1 P.M. and at 4.30 P.M., the entire apparatus having been previously dismantled and cleaned; as with all the precautions the surfaces became fogged and the quality of the definition reduced.

In the final test I used better mountings for my mirrors, and examined the light after it had passed four times through the liquid. The adjustments in this case were much more tedious, but I finally succeeded in getting a field with a sufficient intensity and uniformity to attain a high degree of sensibility. Under conditions which seemed to be freest from residual disturbances, I could observe no change on reversing the trough. With an unusually brilliant sun, I was able to detect a change in the plane of polarization, by rotating P, of less than $0^\circ 01$, or one tenth of a division of my scale $= 1/120^\circ$. I also made several successive settings in the two positions, but did not note them down, as I treated them as preliminary to more extended systematic observations at another time. I am not therefore able to reproduce them definitely, but the differences between the means of the averages of individual sets of four or five, which I calculated mentally as the positions of the index were read off by my assistant, were perhaps one half of this. The observations upon which I based my conclusions were taken on May 10th at the noon hours 11-1 and at 4.30 P.M., which was as late as I could obtain the sun. I did note occasionally, while observing with the trough at rest, a gradual change in the field amounting in one or two instances to $0^\circ 05$, apparently due to internal motions of the liquid and relative changes of temperature. Observations at reversal were taken, however, when no gradual change could be detected. Slight changes were also noted due to a shift in the reflected beam from the heliostat. This disappeared when the proper adjustments were made to bring

the beams back to the reference mark. Similar effects were noted late in the afternoon.

My next attempt was to obtain observations with light which had traversed the liquid the full number of times,—six, which I had provided for. To do this, I took precautions to exclude extraneous light, in order to obtain the full optical sensibility, by immersing screens made of tubes, within which the successive rays should pass. These adjustments were very tedious, and before I succeeded in getting the light from the last mirror into my field, the noon hour had passed. On dismounting my half-shade system, I found that the nicol A was seriously impaired by the presence of a capillary film of oil and the strip of spar had become detached. On removing the latter, I found it broken. In attempts to remount the remainder I destroyed it still more, beyond hope of further use. These strips are very fragile, ground as they are to 0.1 mm. thick, so as to give as near a vanishing line as possible. Mr. Halle of Steglitz-Berlin has, however, succeeded in making strips with square ground edges, in lengths of 45 mm., as thin as this. As it was quite doubtful whether I could obtain a proportionately greater sensibility with six as with four passages, even if I should be fortunate enough to secure again so fine a strip, and as it was probable that I would have to renew all the optical pieces as well as redistill the oil, I concluded to discontinue the experiment, although I had not obtained the final sets of systematic observations as originally planned. The likelihood of attaining a second order sensibility in this test seems very remote indeed, although these observations have carried the test probably a thousand times beyond the first order and within a factor of ten of the second order.

Thus assuming white light and a rotation varying as the inverse square of the wave-length, we may take 130° per decimetre as the mean rotation of the oil. Considering the total length to be $410 \text{ cm.} \times 4 = 1640 \text{ cm.}$, we have a (compensated) rotation of 21300° . As I should have been able to detect a direct change of $1/120^\circ$ on reversal, or one half this, if the mean readings mentioned be allowed, the total effect of the motion of the earth in space on the rotation of the plane of polarization is certainly less than $1/5000000$, and probably less than $1/10000000$.

Physical Laboratory, University of Nebraska,
June 7, 1905.

XLVI. *On the Mutual Solubilities of Diethylamine and Water.*

By ROBERT TABOR LATTEY, B.A.*

PAIRS of liquids which exhibit partial miscibility are usually divided into three classes:—(1) Those which are completely miscible above a certain temperature and only partially miscible at lower temperatures; (2) Those which are only partially miscible at all observed temperatures; and (3) Those which are completely miscible below a certain temperature above which they become partially miscible.

The only cases of the third class which have been at all fully studied are: sym-trimethylpyridine and water, triethylamine and water (Rothmund, *Zeit. Phys. Chem.* xxvi. p. 433, 1898), diethylamine and water (Guthrie, *Phil. Mag.* xviii. p. 500, 1884) †.

The case of nicotine and water recently studied by Hudson (*Zeit. Phys. Chem.* xlvii. p. 113, 1904) forms an interesting combination of cases (1) and (3) since these liquids are completely miscible below 64° or above 205°, but between these temperatures their mutual solubilities are only partial; the solubility-temperature curve representing these phenomena is a closed elliptical figure.

This work was originally undertaken with the idea of extending Guthrie's curve showing the relation between temperature and the miscibility of diethylamine and water. It was soon found that either Guthrie's results were incorrect or that some unexpected influence was raising the temperature of separation. The results obtained at first were not only in disagreement with Guthrie's, but were not in concordance with one another. At length the behaviour of one of the tubes (which happened to contain 58·6 per cent. of the amine) furnished a clue to the difficulty; the contents of this tube separated into two layers at 140°·2, after being cooled to about 130° and mixed they separated at 138°; a second and third repetition of the observation gave 137° and 135° respectively. Obviously some change was taking place in the contents of the tube: this might either be due to an action of water on diethylamine at high temperature or to the solution of glass. Experiment showed that about 1 mgrm. of solid had been taken up by the liquid. Experiments were therefore begun in tubes of Jena glass; the results so obtained were concordant, and measurements made on individual tubes did not differ from one another more than observations of a melting-point usually differ.

* Communicated by the Author.

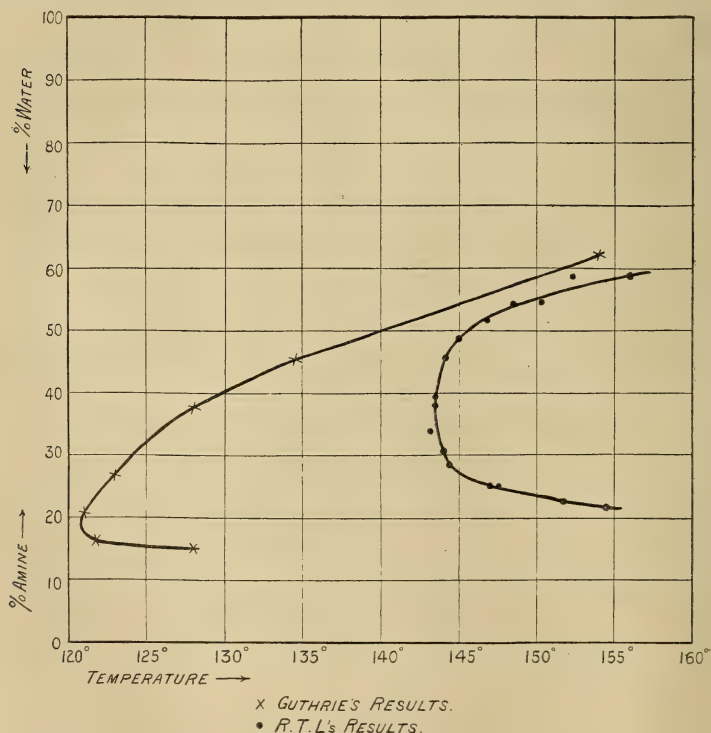
† The case of dimethylamine and water, given by Prof. van't Hoff in his "Lectures on Theoretical and Physical Chemistry," is a misprint. (Private communication.)

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The results obtained were as follows :—

Per cent. amine.	Temperature of separation.	Per cent. amine.	Temperature of separation.
21.73	154.5	39.63	143.5
22.65	151.7	45.78	144.15
25.04	147.5	48.89	145
25.06	147	51.71	146.8
28.60	144.4	54.24	148.5
30.93	144	54.78	150.3
34.03	143.2	58.59	152.3
38.18	143.5	58.99	156

The “critical” temperature so found is $143^{\circ}5$ and the “critical” concentration 37.4 per cent. of amine ; whereas Guthrie’s results lead to 121° and 19 per cent. respectively. That this divergence is attributable to the glass seems not



unreasonable when we read Guthrie’s statement that “the 62.35-per-cent. solution caused the surface of the glass (soft

German) to peel off in visible scales when heated to 150°–160°."

An experiment with Müller's new "Resistance" glass showed that even the slightly greater solubility of this glass as compared with Jena glass caused a lowering of the temperature of over 2°.

It seems therefore probable that the present results are slightly vitiated by the solution of glass, but the extreme insolubility of Jena glass makes it likely that the inaccuracy due to its solution is but slight.

It is interesting to note that the form of the curve is more that of the curve for trimethylpyridine (Rothmund, *Zeit. Phys. Chem.* xxvi. p. 462, 1898) than of that for triethylamine (*ibid.* p. 461).

An attempt was made to determine the densities of saturated solutions of the amine and water at various temperatures by heating a solution containing 40·5 per cent. of amine in a graduated tube and observing the volumes of the layers at various temperatures; the weights of the layers being calculated from the percentage composition of the saturated solutions as read off from the solubility curve. The attempt proved abortive since the values obtained are so largely affected by small errors in the data used; it was, however, obvious that there is (1) practically no sudden change in total volume on separation, and (2) very little difference in the densities of the two saturated solutions which are in equilibrium at temperatures between 143°·5 and 153°.

Experimental Details.

Suitable quantities of water and diethylamine were weighed out from "weighing pipettes" into a small stoppered bottle. The mixture so made was transferred as soon as possible to a tube sealed at one end and drawn to a capillary in the middle; by changes of pressure the liquid was got into the lower part of the tube, which was then sealed off at the capillary.

A tube so prepared was heated in a paraffin bath so arranged that the filament of an incandescent electric lamp could be viewed through the tube. The appearance of a fog was very easily seen in this way, and consequently the temperature at which the mixture under observation separated into two layers could be ascertained with a fair degree of accuracy. The tube was supported by a simple mechanical device whereby the contents could be remixed between observations without removing the tube from the bath.

XLVII. *Notices respecting New Books.*

Leçons sur l'Électricité. Par ERIC GERARD. Tome Second. Paris : Gauthier-Villars, 1905. Pp. viii + 888.

THE publication of the second volume of the seventh edition of this work has not long been delayed after the appearance of the first volume, and the high terms in which we spoke of the latter must also be extended to the former. A perusal of the work cannot fail to produce the impression that it is a wonderfully complete and well-balanced account of the present state of electro-technology. There is hardly a single aspect of the subject left unnoticed by the author, and yet the book is extremely readable and not overloaded with detail. It is in every respect an ideal student's book.

The present volume deals with alternate-current transformers, their theory, construction, design and testing; induction-coils and their applications; systems of distribution, overhead lines, underground conductors, submarine cables, telegraphy, telephony, electric lighting and lamps, photometry, motors—continuous current, polyphase, single-phase, induction and synchronous—rotary converters, power transmission, electric traction, electro-metallurgy, and electro-chemistry.

The diagrams and illustrations are characterized by simplicity, but their reproduction is far from perfect, and leaves a good deal to be desired. This is about the only feature of the book which calls for adverse criticism, and to which the publishers would do well to give some attention.

Mathematical and Physical Papers. Vol. V. By the late Sir GEORGE GABRIEL STOKES. Reprinted from the Original Journals and Transactions, with brief Historical Notes and References. Cambridge: at the University Press. 1905. Pp. xxv + 370.

DR. LARMOR's task of editing those papers of the late Sir G. G. Stokes which had not been included in the first three volumes—the third being the last which their illustrious author was able personally to prepare for publication—has now been completed. The present volume contains various short papers and notes, very frequently having reference to the work of other investigators who asked for that advice and guidance which were always so generously and freely given. In order to render the volume self-contained, contributions by other authors are included where necessary to the complete understanding of Sir G. G. Stokes's criticisms or suggestions. We notice a paper "On the Maximum Wave of Uniform Propagation," which has never been published before. An appendix contains Mathematical Tripos and Smith's Prize Examination papers set by Sir G. G. Stokes. The volume opens with the masterly Obituary Notice by Lord Rayleigh, reprinted from the Proceedings of the Royal Society, and it is adorned by an excellent portrait of Sir G. G. Stokes in his later years.

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JOURNAL OF SCIENCE.

[SIXTH SERIES.]

OCTOBER 1905.

XLVIII. *The Origin of the Prismatic Colours.*

By Lord RAYLEIGH, O.M., F.R.S.*

THE fact that by the aid of a spectroscopic interference may be observed with light originally white used to be regarded as a proof of the existence of periodicities in the original radiation; but it seems now to be generally agreed that these periodicities are due to the spectroscopic. When a pulse strikes a grating, it is obvious that the periodicity and its variation in different directions are the work of the grating. The assertion that Newton's experiments prove the colours to be already existent in white light, is usually made in too unqualified a form.

When a prism, which has no periodicities of figure, is substituted for a grating, the *modus operandi* is much less obvious. This question has been especially considered by Schuster (*Phil. Mag.* xxxvii. p. 509, 1894; vii. p. 1, 1904), and quite recently Ames has given an "Elementary Discussion of the Action of a Prism upon White Light" (*Astrophysical Journal*, July 1905). The aim of the present note is merely to illustrate the matter further.

I commence by remarking that, so far as I see, there is nothing faulty or specially obscure in the traditional treatment founded upon the consideration of simple, and accordingly infinite, trains of waves. By Fourier's theorem any arbitrary disturbance may be thus compounded; and the method suffices to answer any question that may be raised, so long at least as we are content to take for granted the

* Communicated by the Author.

character of the dispersive medium—the relation of velocity to wave-length—without enquiring further as to its constitution. For example, we find the resolving-power of a prism to be given by

$$\frac{\lambda}{d\lambda} = T \frac{d\mu}{d\lambda} \cdot \cdot \cdot \cdot \cdot \cdot (1),$$

in which λ denotes the wave-length *in vacuo*, T the “thickness” of the prism, μ the refractive index, and $d\lambda$ the smallest difference of wave-length that can be resolved. A comparison with the corresponding formula for a grating shows that (1) gives the number of waves (λ) which travel in the prescribed direction as the result of the action of the prism upon an incident pulse.

But, although reasoning on the above lines may be quite conclusive, a desire is naturally felt for a better understanding of the genesis of the sequence of waves, which seems often to be regarded as paradoxical. Probably I have been less sensible of this difficulty from my familiarity with the analogous phenomena described by Scott Russel and Kelvin, of which I have given a calculation*. “When a small obstacle, such as fishing-line, is moved forward slowly through still water, or (which, of course, comes to the same thing) is held stationary in moving water, the surface is covered with a beautiful wave-pattern, fixed relatively to the obstacle. On the up-stream side the wave-length is short, and, as Thomson has shown, the force governing the vibrations is principally cohesion. On the down-stream side the waves are longer and are governed principally by gravity. Both sets of waves move with the same velocity relatively to the water, namely, that required in order that they may maintain a fixed position relatively to the obstacle. The same condition governs the velocity, and therefore the wave-length, of those parts of the wave-pattern where the fronts are oblique to the direction of motion. If the angle between this direction and the normal to the wave-front be called θ , the velocity of propagation of the waves must be equal to $v_0 \cos \theta$, where v_0 represents the velocity of the water relatively to the (fixed) obstacle.” In the laboratory the experiment may be made upon water contained in a large sponge-bath and mounted upon a revolving turn-table. The fishing-line is represented by the impact of a small jet of wind. In this phenomenon the action of a prism is somewhat closely imitated. Not only are there sequences of waves, unrepresented (as would appear)

* “The Form of Standing Waves on the Surface of Running Water,” *Proc. Lond. Math. Soc.* xv. p. 69 (1883); *Scientific Papers*, ii. p. 258.

either in the structure of the medium or in the character of the force, but the wave-length and velocity are variable according to the direction considered.

For the purposes of Scott Russel's phenomenon the localized pressure is regarded as permanent; but here it will be more instructive if we suppose it applied for a finite time only. Although the method is general, we may fix our ideas upon deep water, subject to gravity (cohesion neglected), upon which operates a pressure localized in a line and moving transversely with velocity V . In the general two-dimensional problem thus presented, the effect of the travelling pressure is insignificant unless V is a possible wave-velocity; but where this condition is satisfied, a corresponding train of waves is generated. In the case of deep water under gravity the condition is always satisfied, for the wave-velocities vary from zero to infinity.

The limitation to a wave-train of velocity V is complete only when the time of application of the pressure is infinitely extended. Otherwise, besides the train of velocity V we have to deal with other trains, of velocities differing so little from V that during the time in question they remain sensibly in step with the first. As is known*, the behaviour of such aggregates is largely a matter of the *group-velocity* U , whose value is given by

$$U = \frac{d(kV)}{dk} \quad . \quad . \quad . \quad . \quad . \quad (2),$$

k being proportional to the reciprocal of the wave-length in the medium. In the particular case of deep-water waves $U = \frac{1}{2}V$.

From this point of view it is easy to recognize that the total length of the train of waves generated in time t' is $\pm(V-U)t'$. If τ be the periodic time of these waves, the wave-length in the medium is $V\tau$, and the number of waves is therefore

$$\pm \frac{V-U}{V} \frac{t'}{\tau} \quad . \quad . \quad . \quad . \quad . \quad (3).$$

But for our present purpose of establishing an analogy with prisms and their resolving-power, what we are concerned with is not the number of waves at any time in the dispersive medium itself, but rather the number after emergence of the train into a medium which is non-dispersive; and here a curious modification ensues. During the emergence the

* See, for example, 'Nature,' xxv. p. 51 (1881); 'Scientific Papers,' i. p. 540.

relative motion of the waves and of the group still continues, and thus we have to introduce the factor V/U , obtaining for the number N of waves outside

$$N = \frac{V-U}{U} \frac{t'}{\tau} \quad \dots \quad (4).$$

If X be the distance through which the pressure travels, $X = Vt'$; and if V_0 be the (constant) velocity outside and λ the wave-length outside, $\lambda = V_0\tau$. Thus

$$N = \left(\frac{V_0}{U} - \frac{V_0}{V} \right) \frac{X}{\lambda} \quad \dots \quad (5).$$

To introduce optical notation, let $\mu = V_0/V$, so that μ is the refractive index. In terms of μ

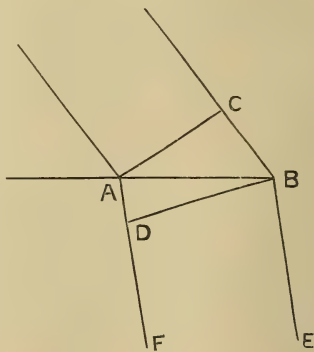
$$\frac{V_0}{U} = \mu - \lambda \frac{d\mu}{d\lambda} \quad \dots \quad (6),$$

so that finally

$$N = -X \frac{d\mu}{d\lambda} \quad \dots \quad (7),$$

in close correspondence with (1). A very simple formula thus expresses the number of waves (after emergence) generated by the travel of the pressure over a distance X of a dispersive medium.

The above calculation has the advantage of being clear of the complication due to obliquity; but a very little modification will adapt it to the case of a prism, especially if we suppose that the waves considered are emergent at the second face of the prism without refraction. In the figure, AC represents an incident plane pulse whose trace runs along the first face of the prism from A to B . AF , BE is the direction of propagation of the refracted waves under consideration, to which the second face of the prism is perpendicular. As before, if τ be the period, V the wave-velocity of the waves propagated in direction BE , U the corresponding group-velocity, t' the time of travel of the pulse from A to B , the number of waves within the medium is



$$\frac{V-U}{V} \frac{t'}{\tau},$$

giving on emergence the number of waves expressed in (4). If V_0 be the velocity in vacuum, $\tau = \lambda/V_0$, and

$$t' = \frac{BC}{V_0} = \frac{AD}{V};$$

so that

$$\frac{t'}{\tau} = \frac{AD}{\lambda} \frac{V_0}{V}.$$

Thus, as in (5), (6), (7),

$$N = \left(\frac{V_0}{U} - \frac{V_0}{V} \right) \frac{AD}{\lambda} = -AD \frac{d\mu}{d\lambda} \quad . \quad . \quad . \quad (8),$$

in agreement with (1).

Although the process is less easy to follow, the construction of a train of waves from an incident pulse is as definite in the case of a prism as is that of a grating; and its essential features are presented to the eye in Scott Russel's phenomenon.

The above treatment suffices for a general view, but it may be instructive to give an analytical statement; and this I am the more inclined to do as affording an opportunity of calling attention to a rather neglected paper by Lord Kelvin entitled "On the Waves produced by a Single Impulse in Water of any Depth, or in a Dispersive Medium"* . When we know the effect of an impulse, that of a uniform force applied for a finite time can be deduced by integration. It may be convenient to recite the leading steps of Kelvin's investigation.

Let $f(k)$ denote the velocity of propagation corresponding to wave-length (in the medium) $2\pi/k$. The Fourier-Cauchy-Poisson synthesis gives

$$u = \int_0^\infty dk \cos k[x - tf(k)] \quad . \quad . \quad . \quad (9)$$

for the effect at place and time (x, t) of an infinitely intense disturbance at place and time $(0, 0)$. When $x - tf(k)$ is very large, the parts of the integral (9) which lie on the two sides of a small range, $\kappa - \alpha$ to $\kappa + \alpha$, vanish by annulling interference; κ being a value, or the value, of k , which makes

$$\frac{d}{dk} \{k[x - tf(k)]\} = 0 \quad . \quad . \quad . \quad (10)$$

$$\text{or} \quad x = t\{f(\kappa) + \kappa f'(\kappa)\} = Ut \quad . \quad . \quad . \quad (11),$$

* Proc. Roy. Soc. vol. xlii. p. 80 (1887).

U being the group-velocity. By Taylor's theorem when $k - \kappa$ is very small,

$$k[x - t f(k)] = \kappa^2 t f'(\kappa) + \frac{1}{2} t (k - \kappa)^2 \{-\kappa f''(\kappa) - 2f'(\kappa)\}.$$

Using this in (9) and integrating with the aid of

$$\int_{-\infty}^{+\infty} d\sigma \cos \sigma^2 = \int_{-\infty}^{+\infty} d\sigma \sin \sigma^2 = \sqrt{\left(\frac{1}{2}\pi\right)},$$

we find as an approximate value

$$u = \frac{\sqrt{(2\pi)} \cdot \cos \left\{ t \kappa^2 f'(\kappa) + \frac{1}{4}\pi \right\}}{\sqrt{t} \cdot \sqrt{\{-\kappa f''(\kappa) - 2f'(\kappa)\}}} \quad \dots \quad (12).$$

As a particular case, for deep-water gravity waves

$$f(k) = \sqrt{(g/k)}, \quad f'(k) = -\frac{1}{2} g^{\frac{1}{2}} k^{-\frac{3}{2}}, \\ -k f''(k) - 2f'(k) = \frac{1}{4} g^{\frac{1}{2}} k^{-\frac{3}{2}},$$

and finally with use of (11)

$$u = \pi^{\frac{1}{2}} g^{\frac{1}{2}} t x^{-\frac{3}{2}} \cos \left(\frac{gt^2}{4x} - \frac{\pi}{4} \right) \quad \dots \quad (13)*.$$

This gives the effect of the impulse at $(0, 0)$. If the impulse be at x', t' , we are to write $x - x'$ for x and $t - t'$ for t . For our purpose of finding the effect of a travelling force, we are to make $x' = Vt'$ and integrate with respect to t' from 0 to t , t' being the duration of the force. The integral will depend mainly upon the part where

$$\frac{(t - t')^2}{x - Vt'},$$

under the cosine, is stationary. This occurs when

$$2x = V(t + t') \quad \dots \quad (14),$$

and then

$$\frac{g(t - t')^2}{4(x - Vt')} = \frac{g(Vt - x)}{V^2} \quad \dots \quad (15).$$

Omitting the variation of the other factors as less important, we see that, when sensible, the effect is proportional to

$$\cos \left\{ \frac{g(Vt - x)}{V^2} - \frac{\pi}{4} \right\} \quad \dots \quad (16),$$

representing simple waves of velocity V . But this is limited to such values of x and t as make t' in (14) lie between 0 and t . Thus if t be given, the range of x is from $\frac{1}{2}Vt$ to $\frac{1}{2}Vt + \frac{1}{2}Vt'$; so that the train of waves covers a length $\frac{1}{2}Vt'$,

* An almost equally simple formula applies when more generally $f(k) \propto k^n$.

agreeing with the general value given before, since here $U = \frac{1}{2}V$. If, as would be more convenient in order to find the length of the train after emergence into a non-dispersive medium, we regard x as given, we find that t ranges from $2x/V$ to $2x/V + t'$.

I have taken the particular case first, as the reasoning is rather simpler when we have, as in (13), an explicit expression in terms of x and t^* . In general κ cannot be eliminated between (11) and (12), and we must proceed rather differently. The question is when will

$$k[x - tf(k)] \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

with

$$x = t\{f(k) + kf'(k)\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

be stationary with respect to t' , $x - Vt'$ being substituted for x and $t - t'$ for t in (17), (18)? Now

$$\left(V \frac{d}{dx} + \frac{d}{dt}\right)(17) = k\{V - f(k)\} + \left(V \frac{dk}{dx} + \frac{dk}{dt}\right)\left(x - t \frac{d\{kf(k)\}}{dk}\right),$$

of which the second term on the right vanishes by (18). The variation of (17) vanishes when $V = f(k)$. Accordingly

$$k[x - Vt' - (t - t')f(k)]$$

with

$$x - Vt' = (t - t')\{f(k) + kf'(k)\}$$

is stationary with respect to t' , if $V = f(k)$, and then assumes the form

$$k[x - Vt].$$

Here t' must lie between 0 and t' . Thus if t be constant, x has a range

$$Vt' - t'\{f(k) + kf'(k)\} = (V - U)t'.$$

And if x be given, t has a range

$$t' - \frac{Vt'}{f(k) + kf'(k)} = \frac{t'(U - V)}{U} \quad . \quad . \quad . \quad (19).$$

These are the limits over which the waves of velocity V extend. And (19) shows that the number of waves which pass a fixed point, either within the dispersive medium or on emergence from it, has the expression

$$\frac{t'}{\tau} \frac{U - V}{U},$$

where τ is the periodic time, in agreement with (4).

* For an admirable discussion of the general problem of deep-water waves arising from a localized disturbance, see Lamb, Proc. Lond. Math. Soc. vol. ii. p. 371 (1904).

XLIX. *The Magneto-Optics of Sodium Vapour and the Rotatory Dispersion Formula.* By R. W. WOOD, *Professor of Experimental Physics, Johns Hopkins University* *.

[Plate V.]

IT has been shown in a previous paper† that the vapour of metallic sodium is an ideal substance for investigating the effect of a strong absorption-band on the magnetic rotation of the plane of polarization. The preliminary work was not very satisfactory, however, as the method employed did not admit of very accurate determinations of the wave-lengths, and the verification of the rotatory dispersion formula could only be considered as approximate. Improvements in the methods of observation and design of the apparatus have been accompanied by an increase in accuracy, which could scarcely have been hoped for, as will be seen by the following comparisons.

In the preliminary work no readings of the rotation for wave-lengths between the sodium lines could be obtained, while in the present case accurate readings have been made for as many as *nine* different values of λ between D_1 and D_2 . Rotations as great as 1440° (four complete revolutions) have actually been observed, and this with a 10 cm. column of not very dense vapour, in a field of only 2000 C.G.S. units. This was for a wave-length midway between D_1 and D_2 . In the earlier work it was only with the greatest difficulty that the bright lines which appeared in the region of the red and green channelled absorption spectrum could be seen. They have now been photographed to the number of about 160 with the 14-foot concave grating. Comparatively few of the absorption-lines show any trace of magnetic rotation, scarcely one per cent.; and the fact seems to have been established that these lines coincide with the bright lines of the fluorescent spectrum of the vapour, which has been found to be by no means the exact complement of the absorption spectrum as was at first supposed.

In the present paper the magneto-optics of the vapour for light travelling along the lines of force will be discussed. The double-refraction of the vapour, which occurs when the rays are perpendicular to the field, has also been studied, and will be reported in a subsequent paper. An exhaustive study of the fluorescence of the vapour has been made, and the lines of the spectrum to the number of several hundred

* Communicated by the Physical Society: read June 30, 1905.

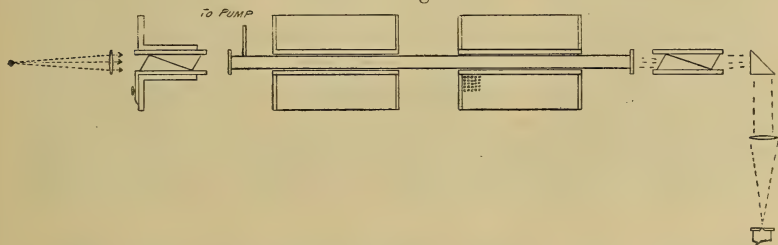
† Wood and Springsteen, "The Magnetic Rotation of Sodium Vapour," Phys. Review, July 1905.

accurately measured. Very remarkable effects have been observed when the vapour is illuminated with a *very* narrow band of approximately homogeneous light, the lines in the fluorescent spectrum changing their position and appearing to dance about in the liveliest manner with the slightest change in the wave-length of the exciting light. The motion is of course only an illusion, lines disappearing and others reappearing, like the sparks of a spinthariscopes. Stokes's law is violated in a most flagrant manner, bright lines coming out on both sides of the excited region. The behaviour of the spectrum indicates that we are dealing with a number of groups of electrons, each group containing a large number of vibrators. The excitation of one of the vibrators sets the whole group agoing, but does not start disturbances in the other groups. These and other remarkable phenomena will be fully discussed in the following Number of this Journal; they are mentioned here only on account of the apparent relation between magnetic rotation and the fluorescence.

In the earlier experiments referred to above, the sodium was heated in a tube of hard glass, the ends of which were closed with thin plate glass. The tubes lasted but a short time, owing to the chemical action of the metal, and in fact usually cracked on a second heating. In the present work a tube of thin steel has been used, the ends of which projected from the helices of the magnet. It was found that the field-strength within the steel tube did not differ greatly from that obtained when glass tubes were used.

The arrangement of the apparatus is shown in fig. 1. A piece of thin seamless steel tubing of such a diameter as to

Fig. 1.



slip easily through the hollow cores of the electromagnet, from which the conical pole-pieces have been removed, is procured. A short piece of small brass tubing is brazed into one end, through which the tube is exhausted. It has been found that a good vacuum is essential, all trace of the rotation disappearing in hydrogen or nitrogen at atmospheric pressure. In the earlier work this fact was not known, and the tubes

were exhausted and then sealed off from the pump. The hydrogen liberated from the sodium must have raised the pressure to at least 15 cms. in all of these experiments. In the light of what is now known, it is surprising that any results at all were obtained under these conditions.

A lump of sodium the size of a walnut is melted in an iron crucible, and poured out into a V-shaped trough made of thin sheet iron. As soon as the bar is solid it is placed in the iron tube, one end of which has been previously closed with a small piece of plate-glass cemented on with sealing-wax. The tube is introduced into the magnet, the sodium bar pushed to a position midway between the helices, and the other end closed with a piece of glass in a similar manner. The ends of the tube should be coated while hot with sealing-wax before the introduction of the sodium. One has then only to wave a Bunsen flame over them and press on the piece of glass, previously heated; the sealing-wax should come into optical contact with the glass to insure an air-tight joint. The tube is now connected with an air-pump which will produce a vacuum of a millimetre or two. If the air-pump leaks, it is a good plan to place a glass stopcock between the pump and tube to prevent the entrance of traces of air after exhaustion. For purposes of demonstration it is sufficient to heat the tube gradually with a Bunsen burner turned down low. In the present work, however, where constancy of temperature was essential, electrical heating was invariably used; the tube was wrapped with a thin sheet of asbestos board, around which was wound a spiral of iron wire, and the whole subsequently covered with an asbestos jacket.

The light from an arc-lamp, made parallel by a lens, is passed through a Nicol prism, the steel tube, and a second nicol, after which it is brought to a focus by means of a second lens upon the slit of a spectroscope. In the present case, the instrument in question was a concave grating of 14-foot radius, the observations being made both visually and by means of photography.

We will now consider briefly the phenomena which are presented when the sodium vapour is formed in the magnetic field. The nicols are crossed and the spectrum vanishes completely. The magnet is now excited and the Bunsen burner placed under the tube, the tip of the flame barely touching it. In a few minutes we see two bright yellow lines exactly in the position of the D lines. The light constituting these lines comes, however, from the crater of the arc, as we can easily see by intercepting the beam. The lines are in reality double, though they appear single with low resolving powers, while even with the 14-foot grating their duplicity

cannot be made out when they first appear. As the density of the vapour increases, the components separate, four lines being distinctly visible. This condition is shown in Plate V. fig. 1 *a*. The lines are formed by the constituents of the white light bordering the two absorption-lines, which having suffered a rotation of 90° in the magnetic field, is enabled to pass the second nicol. The lines continue to separate, becoming broader, and presently we see between them a second pair of lines, for which the rotation is 270° , the two dark regions between representing rotations of 180° . This stage is shown in fig. 1 *b* and *c*, Plate V. In fig. 1 *b*, the two inner 90° lines are beginning to fuse together, the centre being partially dark however. In fig. 1 *c*, the fusion is complete and the centre of the system of lines is bright. With a further increase in the vapour-density, the outer lines (90°) separate still further, and widen out into broad flares of light, and other lines appear between them, corresponding to larger rotations, the system resembling a set of diffraction fringes, as shown in fig. 2. The centre of the system, as I

Fig. 2.



shall designate a point midway between D_1 and D_2 , becomes bright and dark in succession, as is shown in the succeeding figures of Plate V. fig. 1. Only a few of the inner lines show in the photographs, as they are very close together, and the vibration of the building prevented their resolution in the photograph. They could be distinctly seen with the eyepiece, and accurate settings of the cross-hair could be made. Their position with respect to the D lines was very accurately determined by means of a filar micrometer, settings being

made on the dark lines between as well. A little practice was necessary before a complete series of readings could be taken without error, but consistent results were finally obtained. The method adopted was as follows. A current of given strength was turned into the iron spiral, and as the vapour-density increased the alternations of brightness and darkness at the centre were counted. No measurements were commenced until a steady state was reached, which was indicated by a fixed condition at the centre. The positions of the bright lines and dark spaces were then measured, the readings being taken down by an assistant. If at the end of the series any change was found to have occurred at the centre, it was rejected. As soon as the series was completed, the heating current was shut off, and the tube allowed to cool. During this process the alternations of brightness at the centre were counted, and if the number of changes differed from that originally recorded the series was thrown out. The rapid changes which occur as the tube cools, especially an unjacketed tube, are most interesting; the centre of the system changes from bright to dark with great rapidity. The largest number of complete alternations observed was eight, corresponding to a rotation of 1440 degrees, one alternation (*i. e.* from bright to bright) corresponding to 180. The whole thing lasts but a few seconds, the bright band "winking" almost as fast as one can count. We can, as it were, see the plane of polarization actually turning around and around, for four complete revolutions. Beyond this point the absorption becomes too strong to admit of further observations between the D lines, but readings may be taken above and below them in the spectrum, with vapours of very great density.

When the vapour acquires a considerable density, a most magnificent bright-line spectrum appears in the red and green-blue region. Each bright line corresponds to a dark line in the absorption spectrum, but only a small percentage of the dark lines appear to exercise a rotatory power. Some of the strongest absorption-lines are absolutely unrepresented in the magnetic rotation spectrum, which indicates that there is some radical difference in the absorbing mechanism.

Much time has been spent in a further photographic study of the channeled absorption spectrum, a piece of work which was commenced three years ago by the writer in collaboration with Mr. J. H. Moore. It has since been found that the presence of hydrogen, nitrogen, or carbonic acid modifies the appearance of the absorption spectrum. The photographs previously obtained were made by passing the light through

a steel tube containing sodium vapour in an atmosphere of hydrogen, under which condition the absorption spectrum has a most beautiful shaded appearance, being made up of fluted bands, each band containing a very large number of fine lines. If the hydrogen is removed, and the sodium vapour formed *in vacuo*, the fluted appearance disappears almost entirely, although the individual lines remain. Many new lines appear, however, which are broader and more distinct than the others, and these lines appear to coincide with the bright lines in the rotation spectrum. This point will be more fully investigated in the future, the work on the absorption spectrum not having been completed at the present time.

Returning now to the measurements obtained in the vicinity of the D lines, we will discuss them in their bearing on the theory of magnetic rotation.

The rotation has been measured over a considerable range of wave-lengths in the case of very dense vapours, by the method described in the previous paper. In brief this method consisted in rotating one of the nicols through various angles and measuring the position of the two dark bands which move, the one up, the other down the spectrum. The dispersion of the 14-foot grating was too great for this part of the work, and a large plane grating was accordingly combined with a pair of telescope objectives of six-foot focal length. A very brilliant spectrum was obtained in this way, and the dark bands were not so broad but that the cross hairs of the micrometer could be set on the centre of the band with a fair degree of accuracy. Readings of the rotation were obtained throughout the region comprised between $\lambda=5840$, and $\lambda=5932$. The dark bands cannot be pursued with accuracy to greater distances from the D lines, on account of their increased breadth. It will be understood from what has been said in the previous paper, that these dark bands retreat from the D lines when one of the nicols (originally parallel) is rotated towards the position of extinction. The bands enter the red and green regions when the nicols are very near the crossed position, under which circumstances the bright lines appear, and completely obliterate all trace of the dark bands, which by this time have become very broad. The rotation for the wave-lengths corresponding to the dark bands, is the angle through which one of the nicols has been rotated, measured from the crossed position.

The values found with vapours of various density are recorded in the following table. The tables are designated by the magnitude of the rotation midway between the D lines.

CENTRE 30°.	CENTRE 90° <i>con.</i>	CENTRE 450°.	CENTRE 540° <i>con.</i>
5897.1 90	5894.5 270	5902.1 30	5888.0 630
5896.8 180	5894.4 180	5902.2 40	5887.8 540
5896.5 270	5893.34 90	5900.3 60	5887.4 450
5895.4 270	5891.9 180	5899.4 90	5887.1 360
5895.3 180	5891.5 270	5898.7 180	5886.7 270
5894.9 90	5891.3 360	5898.0 270	5886.1 180
5893.2 30	5891.1 450	5897.7 360	5884.6 90
5891.4 90	5890.86 640	5897.5 450	5883.2 60
5890.9 180	5889.0 450	5897.3 540	5881.5 40
5890.6 270	5888.8 360	5894.8 900	
5889.3 270	5888.5 270	5894.7 810	
5889.0 180	5888.2 180	5894.5 720	CENTRE 720°.
5888.7 90	5887.5 90	5894.2 630	5875.0 20
	5886.96 60	5894.0 540	5876.3 25
	5886.5 40	5893.2 450	5880.5 50
CENTRE 50°.	5886.0 30	5892.3 540	5881.4 60
5898.4 30	5884.8 20	5892.1 630	5882.6 80
5897.8 45		5891.8 720	5883.2 95
5897.6 60	CENTRE 180°.	5891.5 810	5885.7 180
5897.3 90		5888.3 720	5887.0 270
5897.0 180	5901.3 20	5888.1 630	5893.5 720
5896.7 270	5900.3 30	5887.9 540	5899.0 180
5895.3 270	5899.4 40	5887.7 450	5900.8 97
5894.9 180	5899.0 60	5887.4 360	5901.8 80
5894.6 90	5898.2 90	5886.9 270	5902.8 61
5893.2 50	5897.7 180	5886.1 180	5903.6 50
5891.9 90	5897.3 270	5884.7 90	5908.0 25
5891.4 180	5897.1 360	5883.2 60	5909.7 20
5891.0 270	5896.9 450	5881.7 40	
5889.1 270	5894.9 540	5880.3 30	
5888.7 180	5894.7 450		CENTRE 960°.
5888.3 90	5894.5 360	CENTRE 540°.	5903.9 60
5887.8 60	5894.1 270	5902.8 40	5902.0 90
5887.6 45	5893.2 180	5901.5 60	5900.2 180
5886.9 30	5891.9 270	5901.1 90	5899.6 270
	5891.6 360	5898.8 180	5899.2 360
CENTRE 90°.	5891.3 450	5898.3 270	5898.8 450
5899.7 20	5891.2 540	5897.7 450	5893.4 960
5899.3 30	5888.6 540	5894.4 720	5886.6 540
5898.8 40	5888.4 450	5893.7 630	5886.3 450
5898.3 60	5888.2 360	5893.5 540	5885.9 360
5897.8 90	5887.9 270	5892.4 630	5885.1 270
5897.45 180	5887.5 186	5892.1 720	5884.5 180
5897.1 270	5886.8 90	5891.8 810	5881.8 90
5896.8 360	5886.2 60	5891.7 900	5880.0 60
5895.3 451	5885.4 40		
5895.0 360	5884.6 30		
	5883.2 20		

A number of photographs of the spectrum with the dark bands in different positions were made with a large three-prism spectrograph, recently constructed for the study of the fluorescence spectrum (Pl. V. fig. 2). It was at first thought that by measuring the positions of the bands on the negatives, more accurate results could be obtained than by visual observations; but the reverse proved to be true, though fairly good curves were obtained. The density of the vapour was

much greater than that employed for the visual observations, and the dark bands were found to be symmetrical with respect to the D lines, that is the rotation was the same at points in the spectrum at equal distances (measured in wave-lengths) to the right and left of the D lines. This was not the case with less dense vapours, the rotation being greater in the vicinity of D_2 than in the vicinity of D_1 . The rotation constant of D_2 was found to be about double that of D_1 ; but since the direction of rotation is the same on opposite sides of the absorption-bands, the effects of the two bands are additive, and lack of symmetry will be less noticeable with very dense vapours, where the measurements are made in a region not very close to the lines.

Verification of the Rotatory Dispersion Formula.

Drude, in his *Lehrbuch der Optik*, has given two formulæ for the magnetic rotatory dispersion, the first of which, developed from the hypothesis of molecular currents, calls for an anomalous effect on crossing the band, and obviously does not apply to sodium vapour. The second, developed from the Hall-Effect Hypothesis, predicts rotations of similar sign and equal magnitude for wave-lengths symmetrically situated in the spectrum, with respect to the centre of the absorption-band. The formula deduced for the rotation is

$$\delta = \frac{1}{n} \left(\frac{a}{\lambda^2} + \frac{b\lambda^2}{(\lambda^2 - \lambda_m^2)^2} \right).$$

In the case of sodium vapour n differs so little from unity that it can be left out of account. Investigations of the refractivity of the vapour by the writer, have shown that with very dense vapour (comparatively speaking) the value of n may be as great as 1.3 in the immediate vicinity of D_2 , but calculations showed that in all of the cases dealt with in the study of the magnetic rotation, n was practically equal to unity. When working very near the D lines the vapour was extremely rare, while observations made with denser vapours covered regions not very near the absorption-lines. Since, moreover, the rotation is zero for very short waves, the first term in the formula drops out, i. e., $a=0$. Precisely the same thing was done in the case of the ordinary dispersion formula, since the refractive index was found to be unity for very short waves*. The similarity in the sign of the rotation on opposite sides of the bands results from the fact that the

* See previous paper on the "Dispersion of Sodium Vapour," this Journal, vol. viii. p. 293 (1904).

quantity $(\lambda^2 - \lambda_m^2)$ (which changes sign on crossing the absorption-band) is squared. An attempt was first made to verify the simplified formula by writing $\lambda_m = 5893$, *i. e.*, assuming the D lines to be a single absorption-band. The value of the constant could then be determined from a single observation, and the rotations for different values of λ calculated. The calculated values agreed very closely with the observed on one side of the double absorption-band, but the agreement on the other side was very poor. This was at once seen to be due to the fact that the rotatory power of the two lines was very different, and the following formula was consequently adopted:—

$$\delta = \frac{a\lambda^2}{(\lambda^2 - \lambda_{D_2}^2)^2} + \frac{b\lambda^2}{(\lambda^2 - \lambda_{D_1}^2)^2}.$$

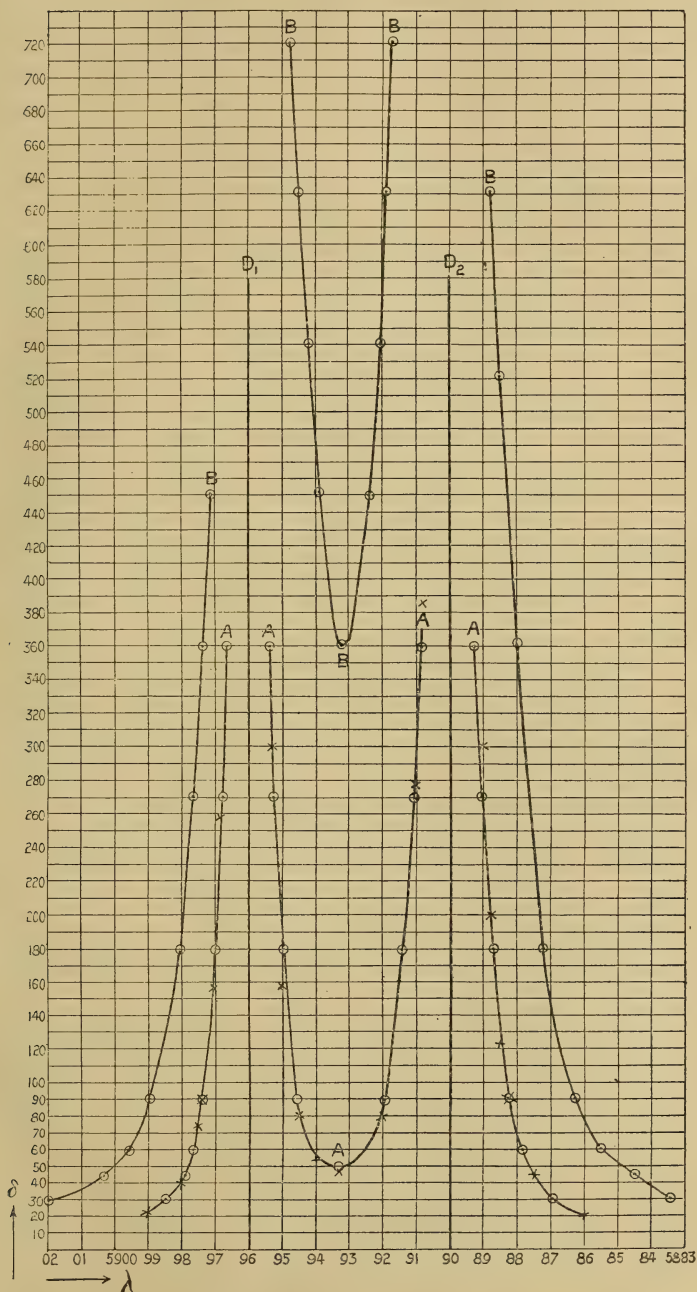
The constants a and b were calculated from two observations of λ , one greater than D_1 , the other less than D_2 . In almost every case the ratio $\frac{a}{b}$ was found to lie between the limits 1·8 and 2·03. Writing $D_1 = 5896$ and $D_2 = 5890$, the values of the constants a and b for vapours of different densities are recorded in the following table. The densities are defined by the magnitude of the rotation for a wave-length midway between D_1 and D_2 .

Density	a .	b .	a/b .	Centre Rotation Calc.
Centre $\delta = 50^\circ$	1123	587	1·9	51°
„ „ 90°	2217	1350	1·65	98°
„ „ 180°	4309	1860	2·3	161°
„ „ 360°	5111	2571	1·98	205°
„ „ 450°	9279	4545	2·03	369°
„ „ 540°	10326	4607	2·24	392°
„ „ 720°	12750	7100	1·8	538°
„ „ 960°	23873	10333	2·6	893°

The “centre” as defined above does not in fact fall at $\lambda = 5893$, but at $5893\cdot5$, *i. e.*, when the bright bands unite, the point of unison lies nearer D_1 than D_2 . This is owing to the fact that the expansion of the bands from D_2 is more rapid than from D_1 .

It was found that if the constants a and b were calculated from different pairs of readings, there was considerable difference between the values, a circumstance which indicates either that the formula is at fault, or that the observations were not sufficiently accurate. We are dealing with very steep curves, as will be seen by reference to fig. 3, and

Fig 3.



small errors in wave-length readings will make large errors in values deduced from them. The severest test of all is to calculate the rotation at the centre from values of the constants determined from two medium values of δ (say 90 or 180), outside of the D lines. This has been done in each case, and in almost every case the calculated value has turned out too small. I have given all of the tables of data thus far obtained, and some of them are doubtless less accurate than others. By means of an improved heating coil (which is in process of construction), of porcelain and strip platinum, I hope to be able to hold the conditions more nearly steady and obtain still more accurate results. It will also be necessary to secure greater rigidity of the grating. For large rotations at the centre, say above 180, we cannot be quite sure of the exact value, as the best that can be done is to regulate the heat so that the condition at the centre is either of maximum brightness or darkness. Errors of say 15 or 20 degrees can be made easily. It is quite possible too that the formula is incomplete. It seems more than likely that the molecular currents play some part, and that the formula built up on the hypothesis of the Hall effect is incomplete. As we shall see presently, however, it represents the rotation outside of the D lines with a surprising degree of accuracy, while between the lines it gives in most cases a curve which is elevated somewhat above the experimental curve. It is possible that the refractive index plays some part, though I regard this as very doubtful. Midway between the D lines the refractivity is equal to unity, rising and sinking rapidly to the right and left of this point. It is not worth while to experiment further with the formula until still more accurate data are at hand, which I am confident of obtaining in the autumn.

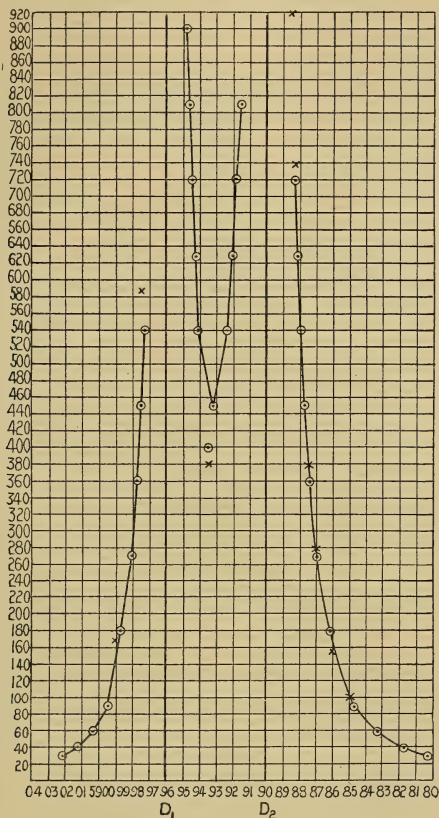
Another matter still to be determined is whether the variation of the ratio $\frac{a}{b}$ results from errors, or whether it increases with the density of the vapour, as appears to be the case. These points will be taken up again in the second paper.

For densities giving rotations of 50°, 90°, and 180°, at the centre, the calculated value agrees with the observed to within the probable experimental error, for, as I have said, it is impossible to fix the rotation at the central band to within say 15. In the case of denser vapours the discrepancies are much too large to be attributed to errors of this sort.

Outside of the D lines the calculated values agree remarkably well with the observed, even in the case of the densest vapour used, as will be seen by referring to the

curves shown in figs. 3 & 4. The observed values are represented by circles, the computed by crosses. The observed values are, of course, for values of δ which are multiples of 90. In the computations, values of λ were selected, which

Fig. 4.



could be accurately represented by five figures, consequently we judge of the accuracy with which the formula represents the dispersion by seeing whether the calculated values fall on the curve determined by experiment. The computations are easily made. One has but to determine the value of

$$\frac{\lambda^2}{(\lambda^2 - \lambda_{D_2}^2)^2} \quad \text{and} \quad \frac{\lambda^2}{(\lambda^2 - \lambda_{D_1}^2)^2}$$

for wave-lengths in the region under investigation. These terms being independent of the vapour-density, can be used

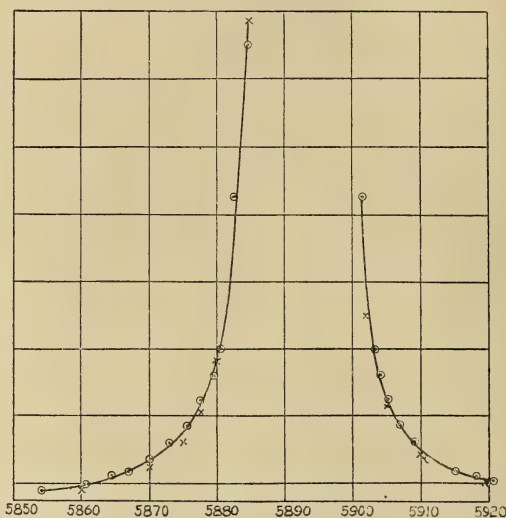
in all cases, the rotations being found by multiplying them by the constants a and b as determined for a particular density, and adding the two products.

On fig. 3 will be found two curves, A and B, for centre rotations of 50° and 360° . The values of δ calculated for the curve A, will be seen to fall in most cases exactly upon the curve of observed values.

Fig. 4 shows that the agreement is also good for a density giving a rotation of 450° at the centre.

Fig. 5 represents the rotatory dispersion of the densest vapour investigated by visual methods. The bright band

Fig. 5.



between the D lines had disappeared completely owing to absorption. This corresponds to a rotation at the centre of about 1500, since 1440 is the largest rotation actually observed. The observed values in this case were obtained with the plane grating and long focus telescope-lenses of five inches aperture. The positions of the broad, dark bands were measured with a micrometer eyepiece for different settings of the nicol, and readings were also taken on the narrow bright and dark bands lying between the broad ones and the D lines, whenever they were distinct enough. In this case, the calculated values lie on a curve slightly lower than the observed curve. This may be due to the fact that the value of λ for which the rotation is equal to that of the nicol may not lie exactly at the centre of the band. On the whole, the formula may be said to represent the rotatory dispersion,

exclusive of the region between the D lines, with a fair degree of accuracy. If, however, the constants a and b are calculated from different pairs of readings on the same curve, different values are found. On this account, it seems desirable to redetermine the curves with still greater precision, which will be done in the autumn.

Vapour of a still greater density was investigated by a photographic method.

The spectrum of the light transmitted through the two nicols and the sodium tube was photographed with the three-prism spectrograph. Seven exposures were made on a trichromatic plate, with the second nicol in seven different positions. A sodium flame, burning continuously in front of the slit of the spectrograph, recorded the D lines on the plate. The tube was heated electrically, and the density of the vapour remained constant throughout the total time of exposure. Each exposure amounted to less than half a minute. One of these multiple photographs is reproduced on Plate V. fig. 2. The picture may be said to show the dispersion curve. The positions of the dark bands, which appear on each side of the D lines, were measured on the dividing engine, and the wave-lengths determined from the calibration curve of the spectrograph. For these dense vapours it was found that the rotatory dispersion curves were symmetrical with respect to a point midway between D_1 and D_2 . The dispersion should therefore be well represented by a single-term formula

$$\delta = \frac{a\lambda^2}{(\lambda^2 - \lambda_D^2)^2},$$

in which $\lambda_D = 5893$. This was found to be the case, as the following table shows.

Value of constant $a = 135600$.

λ .	δ Observed.	δ Calculated.
5980	5	4.47
5950	10	10.4
5933	20	20.2
5923	40	38.0
5917	60	58.9
5912.5	90	89.2
5874	90	93.1
5869	43	43.0
5864	40	40.6
5852	20	20.2
5833	10	9.2
5814	5	5.2

The value of the constant was determined from the value of δ at wave-length 5869, as given by the curve.

The Bright Line-Spectrum produced by Magnetic Rotation.

Attention has already been drawn to the remarkable bright line-spectrum which presents itself when the nicol prisms are crossed. This spectrum, which at first could only be seen with the greatest difficulty, was finally obtained of such brilliancy that it could be photographed with the 14-foot concave grating. A good vacuum was found to be the most essential condition. In the earlier work with tubes sealed off from the pump, the pressure due to the liberated hydrogen was probably responsible for the faintness of the lines. The density of the sodium vapour must be just right. If it is too rare, the rotation is insufficient to bring out the lines, if too great the absorption weakens the spectrum. When the conditions are exactly right, the lines are almost as bright and numerous as the metallic lines in the arc. Photographs of the complete spectrum were made on trichromatic plates, made by the Cramer Dry Plate Co. of St. Louis. These plates were found to be sensitive up to the red lithium line ($\lambda = 6705$). An exposure of fifteen minutes was sufficient with a small concave grating of about a metre radius. Two of these photographs are reproduced natural size on Plate V. fig. 4.

I have never observed a doubling of any of these lines such as occurs in the case of D_1 and D_2 . So great is the quantity of the light transmitted by the second nicol, that a brilliant orange-coloured image of the crater of the arc appears on the slit of the spectroscope the moment the current is turned into the magnet, notwithstanding the fact that the nicols are crossed.

Inasmuch as it seemed desirable to investigate these lines under higher dispersion and determine their wave-lengths as accurately as possible, the apparatus was transferred to the concave-grating room.

The Ruhmkorff magnet was abandoned at this point, as it was found that an exposure of four or five hours would be necessary, and the magnet became dangerously hot in half an hour, when fed with the necessary current. In its place a very large magnet, built by Professor Rowland for the study of the Zeeman effect, was used. This magnet could be fed with a 110-volt current without resistance, and operated continuously for any necessary length of time.

A steel tube nearly 3 cms. in diameter could be used with this magnet, which increased in no small degree the amount of light available. After a number of failures, a very satisfactory negative was secured with the 14-foot grating showing the rotation spectrum and the absorption spectrum side

by side on the same plate. The plate showed over 60 lines in the blue-green region, some of them so faint that they could scarcely be seen under the microscope of the dividing engine, while others were strong enough to yield prints capable of reproduction.

Another plate was exposed and a comparison iron spectrum recorded by the side of the rotation spectrum. The wave-lengths of the rotatory lines were measured on both plates, using the iron lines and the sodium absorption lines as standards. Some of the latter were redetermined, as it was found that they were much sharper when the sodium was heated in a high vacuum, than when heated in hydrogen, as in the previous work by Dr. Moore and the writer. A print obtained from the plate on which the rotatory spectrum and absorption spectrum were recorded, is shown on Plate V. fig. 5. The absorption spectrum is above. Many lines are, of course, visible on the negative which do not show on the print. The rotatory lines in many cases coincide with the heads of the groups of absorption-lines, though the centre of the line appears to be slightly displaced beyond the head of the group of absorption-lines. The displacement is, however, very slight, not more than half the width of the line. A list of the wave-lengths of all the lines visible on the negative follows. The approximate intensities are represented by numerals, 10 indicating the maximum intensity and 1 the minimum. Lines marked with an interrogation point were so faint as to be doubtful.

Green Rotation Spectrum.

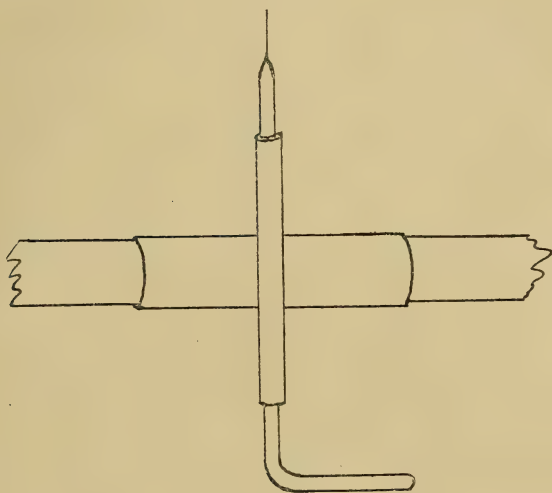
1 5225.34	1 5033.54	2 4839.56
1 5218.49	1 5025.66	9 4837.49
1 5212.02	1 5003.12 broad	2 4819.43
1 5186.70	10 5001.57	1 4814.60
1 5179.71	5 4979.34	1 4812.68
1 5172.98	1 4970.85	3 4810.16
5171.98	1 4967.10	3 4802.62
1 5169.04	1 4964.39	5 4792.67
5165.85	9 4962.85	3 4782.89
1 5140.71	1 4958.62	1 4777.00
2 5133.73	1 4933.93	1 4766.94
2 5126.54	3 4932.64	6 4756.69
1 5119.34	2 4924.32	1 4752.04
2 5094.78	4 4912.10	2 4738.51
7 5087.31	1 4904.67	4 4727.52
8 5079.78	1 4903.38	1 4716.90
1 5071.58	1 4896.65	1 4715.63
1 5052.83	1 4894.58	1 4703.78
1 5049.56	1 4892.77	2 4692.54
3 5048.49	2 4883.81	2 4670.30
7 5040.65	2 4865.59	

It is especially noteworthy that many, in fact most, of the strongest absorption-lines are not represented at all in the rotation spectrum. This fact is of fundamental importance, for it indicates that the absorbing mechanism is different in the two cases. Just wherein the difference lies cannot as yet be definitely stated. On either hypothesis as to the cause of the magnetic rotation, the effect will diminish as the mass of the electron increases. The lines of the rotatory spectrum may correspond to the negative electrons of small mass; the other lines in the absorption spectrum which show no rotatory power may be due to positive electrons, or at all events to some form of vibrator having a much larger mass than the negative electrons. The fact that the bright lines in the fluorescence spectrum coincide with the lines of the rotatory spectrum appears in accord with this assumption; for the agitation of the electrons by the light vibrations will be greatest for those having the smallest mass. It will doubtless be possible to speak more definitely in regard to this point, after the fluorescence spectrum has been more carefully studied. The matter will be discussed further in the paper on the fluorescence of the vapour.

The rotation spectrum in the red and orange region is more brilliant than the green-blue one, but it was found impracticable to photograph it with the 14-foot concave grating. Excellent photographs of it were secured with a small concave grating of about a metre radius. One of these, together with the absorption spectrum, is reproduced in fig. 3, Plate V. As the red lines could be *seen* without difficulty with the 14-foot grating, a method was devised for mapping them which may prove useful in other lines of work. A brass rod, on which a short brass tube had been fitted, was fastened into the grating camera just below the groove which held the plate. To the movable tube was soldered a second tube of 2 mm. bore, into which was fitted a short piece of brass rod. This rod carried a needle point. The whole arrangement is shown in fig. 6. A plate of thin glass was carefully smoked over a gas-flame, and then wiped clean with the exception of a narrow strip 4 mms. wide along the centre. This plate was inserted in the camera in place of the photographic plate, and the spectrum brought into such a position that the lines crossed the smoked strip. The positions of the lines could be accurately recorded by bringing the needle-point in contact with the glass exactly on the centre of a line

and then pushing it up across the soot-film. Intensities could be recorded approximately by varying the length of the lines. The sodium lines were also recorded on the plate in the same manner to serve as reference marks. An iron arc spectrum was then photographed together with the sodium lines, and by means of the two plates the wave-lengths of the lines of the red rotation spectrum could be determined. The

Fig. 6.



smoked plate was fixed by flowing it with a very dilute solution of collodion in ether. A print from the plate mounted "in register," with a photograph of the absorption spectrum, is shown in fig. 6, Plate V. Owing to the inequality of the absorption in the different parts of the spectrum above the D lines, it was necessary to take photographs, at three different vapour densities, to represent the entire set of lines. The prints have been carefully registered in mounting. The small percentage of lines which exercise rotatory power is very marked in this case. The position of the D lines is indicated on the left-hand end of the plate, the red being to the right. The wave-lengths of the lines in the red rotation spectrum are given in the following table:—

Red Rotation Spectrum.

8	6005.26	3	6218.94	2	6427.45
8	6019.22	5	6230.59	7	6434.23
4	6023.45	7	6235.96	2	6435.05
5	6027.47	3	6240.19	3	6438.30
7	6031.79	5	6252.18	3	6441.62
4	6060.25	7	6255.73	3	6445.24
6	6063.91	2	6260.32	10	6449.86
7	6067.47	10	6262.55	1	6470.58
2	6075.42	6	6272.78	10	6481.89
10	6107.73	3	6283.40	10	6490.36
9	6108.25	3	6283.84	7	6501.45
4	6113.59	10	6314.14	3	6515.69
2	6113.81	6	6317.09	4	6545.91
10	6125.67	10	6323.81	3	6555.71
10	6139.17	2	6358.65	5	6556.66
8	6162.74	10	6374.11	7	6579.55
2	6166.10	4	6379.24	8	6609.80
6	6172.32	10	6386.50	7	6623.96
4	6179.92	6	6399.79	3	6676.88
8	6183.93	3	6405.84	4	6692.60
9	6196.93	8	6419.69	4	6745.61
8	6216.56	3	6426.85	5	6761.19

The fact that the rotatory effects of the vapour only manifest themselves in a vacuum is of fundamental importance. The exact nature of the changes which occur when an inert gas is admitted have not as yet been determined. The fact that no trace of the red or green rotation spectrum could be seen when the vapour was formed in an atmosphere of hydrogen at atmospheric pressure, and that only a very slight restoration of light was visible at the D lines, led me to infer that the rotatory power had been destroyed. On experimenting with helium light, however, it was found that the rotation measured in degrees was the same after the admission of the hydrogen as before, the intensity of the transmitted light being however very much less. On this account it seems probable that the effect of the chemically inert gas is to modify the absorbing power of the gas. It has been found that the presence of hydrogen or nitrogen interferes in a very marked degree with the fluorescence of the vapour.

The same thing is true in the case of iodine vapour, which has been found to possess a very great rotatory power for green light, when the vapour is formed in an exhausted tube. A small bulb, containing a few crystals of iodine, is exhausted and sealed off from the pump. On placing it between the poles of the magnet between crossed nicols, a most beautiful emerald-green light is restored the moment the current is turned into the coils. The spectrum of this light has been photographed and observed visually. It resembles the absorption spectrum so closely as to lead to the belief that the narrow black lines in it are produced by absorption. Little

or no rotatory power is exhibited for red or yellow light, though this portion of the spectrum is filled with fine absorption-lines, and the blue region is also wanting in the rotation spectrum, though the absorption is strongest at this point. These effects are at present under investigation, and will be reported in a subsequent paper.

The investigation reported in the present paper is one of a series made possible through substantial aid received from the Rumford Fund, two grants having been made by the American Academy for a study of the optical properties of sodium vapour.

Much assistance has also been rendered by Mr. A. H. Pfund, for whose services I am indebted to the Carnegie Institute.

L. *The Scintillations produced by Radium.*
By Prof. R. W. WOOD*.

[Plate VI.]

THE scintillations of phosphorescent zinc sulphide, when subjected to the bombardment of the radium corpuscles, does not appear to have been satisfactorily explained up to the present time. Crookes regarded each flash as due to the impact of a positive electron or alpha particle, while Becquerel regards the production of light as a secondary effect resulting from cleavages of the crystals brought about by the action of the rays. Neither hypothesis, as it stands, seems quite convincing. I do not know whether any estimate has ever been made of the actual number of alpha particles emitted in unit time from a given amount of radium, but it certainly seems as if the number must far surpass the number of flashes of light as seen in the spinthariscopes. As to the second hypothesis, the only argument advanced in support of it appears to be the circumstance that zinc sulphide crystals when rubbed or crushed show similar flashes of light. This argument is not very convincing, for it amounts to saying that similar effects must result from similar causes. On the other hand, the cleavage hypothesis removes the trouble regarding the number of flashes as compared with the probable number of electrons, as well as the difficulty in the conception of an appreciable flash being produced by a single electron. The experiments described in this paper were completed just a year ago, and were undertaken in the hope of settling the question of the cause of the flash. As they did not lead to sufficiently definite conclusions to satisfy me,

* Communicated by the Author.

I refrained from publishing the results, in the hope and belief that some one else would succeed in solving the question in a more conclusive manner. As no further work appears to have been done, and no more satisfactory theory has been advanced, I have decided to publish them for what they are worth, together with the conclusion which I have drawn from them.

It was hoped that the cleavage hypothesis could be either strengthened or weakened by determining the duration of the spinthariscopes flashes, and the flashes produced by cleavage. If the order of magnitude was found to be different in the two cases, it would be a very strong argument against the hypothesis of Becquerel. The duration of the flash due to the radium bombardment was determined without difficulty. The rim of a wooden disk, mounted on the shaft of a small electric motor, was coated with zinc sulphide, and a speck of radium supported above it on a needle-point. The bombarded surface was viewed in a dark room with a small lens. The flashes remained sharp even when the motor was running at a pretty good rate, but on further increasing the speed they became less distinct, being drawn out into short streaks. It was pretty definitely determined that the duration of the flash was somewhere between $\frac{1}{15000}$ and $\frac{1}{20000}$ of a second.

The only method I was able to devise for determining the duration of the triboluminescence was the simple expedient of touching the rim of the revolving disk with the point of a slender rod of glass. The glow due to the crushing of the crystals extended halfway around the disk, even when it was running at low speed. In addition to the long streak of light, there is always a bright star at the point where the rod is in contact with the sulphide surface. This is probably due to the fact that some of the luminous crystals are held back by the rod, stick to it in other words, and does not necessarily indicate that the first flash is of very brief duration. If the latter were the case, we should expect the glowing star to be slightly drawn out at very high speeds, which is not the case. This experiment appears to indicate that the flashes due to the radium bombardment have a much shorter duration than the flashes which result from fractures of the crystals, and there seems to be no way of escaping from the conclusion that the two phenomena are not very closely related.

A photographic study of the phenomenon was then undertaken to determine the integral effect of a large number of flashes, for it seemed possible that the flashes might recur at certain points more often than at others.

A small heap of the phosphorescent sulphide was pressed flat with a clean piece of glass. A perfectly flat smooth surface was thus formed, free from any cement or other binding substance. A speck of radium was mounted above it, and the scintillating surface photographed with a Zeiss microscope, using a low-power objective, the magnification not exceeding 20 diameters. An exposure of three days was given, which was sufficient to yield an excellent negative. A print from the plate is reproduced in Pl. VI. fig. 1. The photograph reminds one strongly of a picture of the moon made with telescope of moderate size. There are patches of considerable size which only phosphoresce feebly, and small points which shine brightly. The speck of radium was located at the extreme edge of the field. Although the sulphide was in the form of a very fine powder, the dark areas seemed to have a considerable size, many times larger than the largest crystals. The powder was rubbed for fifteen minutes in an agate mortar and the experiment repeated. In this case the dark areas were smaller and the bright specks more conspicuous (fig. 2). Evidently certain crystals phosphoresced much more brightly than others. To get a better idea of what was going on, a glass slide was lightly dusted with the powder and placed under the microscope. Two photographs were made of the same field of view, one by illuminating the particles with light (dark-ground illumination), and the other by exposing them for three days to the radium bombardment. The two pictures are reproduced in figs. 3 and 4. The arrows point to the same spots on the two fields. It is at once apparent that only a small percentage of the crystals become luminous under the action of the radium rays.

This may be due to the following circumstance. It is well known that large crystals do not show the scintillations to advantage. It is also a fact that the phosphorescent power of most substances is due to minute traces of impurities. The inert crystals are probably those in which the impurity causing phosphorescence is not present, or if present, is buried under the surface. This explanation of course assumes that the crystals are not homogeneous, and we may suppose that the flash of light occurs only when an electron strikes a molecule of the impurity. Some crystals may not have the necessary molecules in a position which can be reached by the electrons. It would be worth while to pick out the active crystals and photograph them in different positions, *i. e.* turning them over between the exposures. They are so small that the experiment would not be an easy one, but doubtless those who make pictures out of diatoms would find no difficulty in accom-

plishing the feat. The increase in the scintillating power produced by crushing the crystals may be due to the exposure of more of the molecular impurities. If any difficulty is felt regarding the action of a single electron, we can assume as well, that the continued action of the rays results in an unstable chemical change effected upon the impurity, the flash being due to the toppling over of the molecular complex into its original condition.

The fact that the flash lasts as long as $\frac{1}{20000}$ of a second seems to point to some such action, for the wave train emitted even in this brief length of time is measured in miles, and a simple vibration started by the impact of an electron could scarcely be supposed to persist for so long a time.

LI. *The Specific Heat of Iron at High Temperatures.* By J. A. HARKER, D.Sc., Assistant at the National Physical Laboratory, Teddington*. (From the National Physical Laboratory.)

ALTHOUGH a knowledge of the specific heat of iron at high temperatures would seem to be of vital importance to the metallurgical industry, yet it would appear that no determination of this physical constant has been made since our high-temperature thermometric scale has been rendered definite and precise by the investigations of the last fifteen years.

Probably on account of its difficulty, the work done on the subject of specific heats at high temperatures is comparatively meagre. Apart from observations dating from more than forty years ago, a careful search in the literature of the subject has revealed the work of only one observer, who carried his observations up to really high temperatures—Pionchon, a pupil of Berthelot, who worked in Paris in 1886. His experiments were made by the usual mixing calorimeter method, but were carried out with exceptional skill and accuracy. His specimens were heated in a specially constructed gas-furnace, those such as iron, which were of a readily oxidizable character, being protected by enclosure in a thin platinum envelope. His temperatures were measured by the Violle method of calorimetric estimation, using a small ingot of platinum heated alongside the specimen. Employing the values previously determined by Violle for the specific heat of platinum, Pionchon found for the melting-

* Communicated by the Physical Society: read May 26, 1905.

point of silver by this method 907°C . We know from more recent determinations that the correct value for this melting-point lies between 955°C . and 962°C . From this it is obvious that before the results of Pionchon's experiments can be compared with those of modern work, his temperature-scale must be corrected, at all events at the higher ranges. For this purpose I have assumed that the scale is accurate up to 400°C ., and from that point upwards have added a gradually increasing correction to reduce the value of the silver point to its proper figure. The results obtained are discussed at the end of the paper.

The experiments now to be detailed were undertaken to test the accuracy of the results obtained with the iron ball pyrometer still employed in certain industries for the measurement of temperature. No protecting envelope was employed for the earlier experiments at lower ranges. The specimens, of which several of different sizes were used, were cut to the shape of a cold chisel, and were made of a sample of exceptionally pure metal containing only .01 per cent. of carbon*, supplied to the laboratory by Mr. Hadfield of Sheffield. The largest weighed 197 and the smallest 7 grams. They were heated to the required temperature in a small vertical electric furnace conveniently mounted on a swinging bracket, which permitted the whole to be rapidly brought to an exact position over the calorimeter when required. The furnace was closed at the top and bottom by closely-fitting hinged lids. In the earlier experiments the temperature was measured by a platinum thermometer, enclosed in a thin porcelain tube, the bulb of the thermometer being in close contact with the specimen. To ensure equality of temperature between the specimen and the thermometer, a thick sheath of iron tubing was placed round both, and in all cases ample time was allowed for the establishment of a sufficiently steady temperature in the furnace to render errors in the measurement of the temperature of the specimen as small as possible.

The calorimeters employed were of the type formerly used by the author for investigations on the latent heat of steam, and were of the form of flattened cylinders made of thin brass, nickel-plated heavily on the exterior. The stirring was continuous and was effected by a system of screw-blades

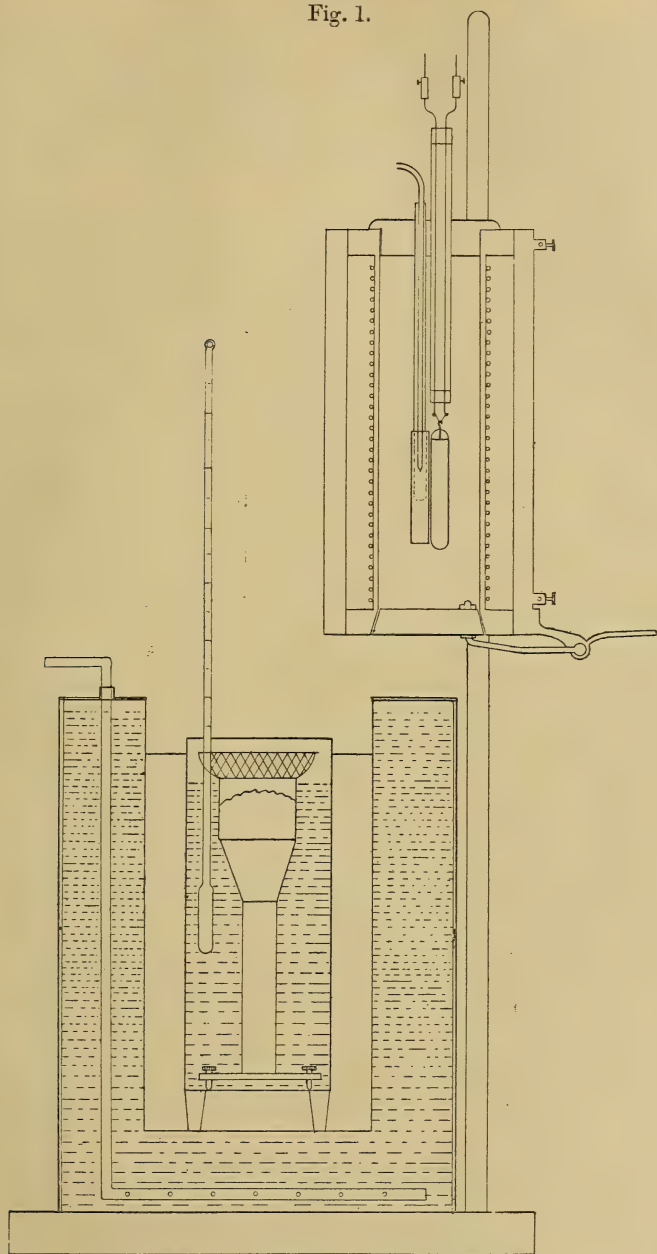
* The complete analysis of the specimen is as follows:—

Carbon	= 0.01
Silicon	= 0.02
Sulphur	= 0.03
Phosphorus	= 0.04
Manganese	= trace

on a vertical shaft driven by a small electric motor. This arrangement has the great advantage of leaving a relatively large proportion of the space in the calorimeter free for the introduction of the hot body, without undue risk to the thermometer. The two calorimeters used in the experiments were of capacities chosen so as to make the rise of temperature obtained with a given specimen the most favourable for accurate measurement, and were of about 4500 and 1700 cubic centimetres capacity respectively. Each was provided with a suitable double-walled enclosure of about ten times the capacity of the calorimeter, having internal walls of brightly polished metal. By this arrangement the temperature of the jacket water, and hence the rate of rise or fall of the calorimeter, could be regulated at will. A diagrammatic representation of the whole arrangement is shown in fig. 1.

Some considerable difficulty was experienced in designing a suitable form of automatic release, which would free the specimen at the right moment and allow it to drop vertically into the calorimeter. It was found that various arrangements of spring tongs which acted reasonably well at moderate temperatures became unusable after exposure to the higher temperatures reached in this work. The following method was therefore adopted. A small loop of platinum wire, bent into an open hook, was attached to the top of the specimen. To two thick nickel leads was twisted a short piece of thin platinum wire, the free portion between them being about 10 mm. long. The leads insulated from one another by porcelain tubes are coupled to a key and battery, by which the thin wire can be instantly fused when required. The electric furnace with the specimen in position, suspended from the fine platinum by means of the hook, was arranged so that only a very few seconds were necessary to open the lower door, swing the whole into position over the calorimeter, drop the specimen, bring back the furnace and reclose. Special blank experiments showed that the heat gained by the calorimeter from the glowing walls of the furnace during the transfer might attain, under unfavourable circumstances, perhaps 2 per cent. of the amount given out by the specimen. Even if some form of the ingenious tramway calorimeter of Prof. Louguinine, with modifications to render it suitable for high-temperature work, had been adopted, it is doubtful whether this source of uncertainty would have been capable of much further reduction. It may be pointed out that in these experiments it is probable that a considerable part of the uncertainty, due to the gain of heat from this source, may be compensated for by the inevitable small loss in transference of the specimen from the interior of the heated

Fig. 1.



Diagrammatic sketch of apparatus for the higher-range experiments. The stirrer of the calorimeter and the ice-box in which the "cold junctions" of the thermocouple were placed are not shown.

furnace to the calorimeter. Since in these experiments the specimen was permitted to fall freely under gravity, and the vertical distance from the normal position of the specimen in the furnace to the surface of the calorimeter water was only about 20 cm., it is obvious that the uncertainty from this source is not great, even at the highest temperatures.

In the later series of experiments, where it was found possible to push the temperatures up above the safe working limit for accurate platinum thermometry, thermocouples were substituted for the thermometers. In the later experiments with the couples, to ensure accuracy the end of the thin steel tube enclosing the couple was inserted into a block of iron of similar shape and size to the sample.

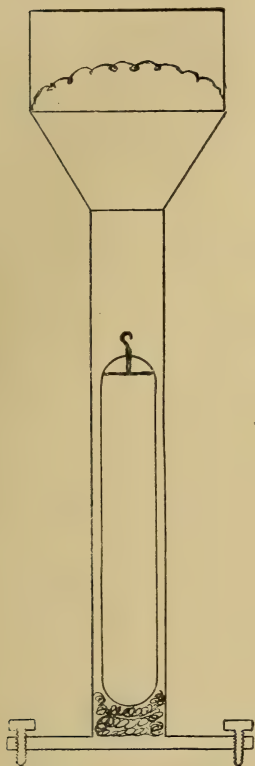
Above 1000° C., however, prolonged exposure of the couple to contact with iron direct is not permissible in the oxidizing atmosphere of a furnace arranged vertically as in this case. The thin steel tube is therefore to be replaced by one of porcelain.

Above about 800° C. it was found that the oxidation of the specimen of iron in the furnace, and its disintegration when dropped suddenly into the calorimeter water, was so rapid as to introduce appreciable uncertainty into the experiments, and, indeed, at temperatures not much higher it was possible by prolonged exposure to transform the whole of a small piece of the same iron into oxide. A number of experiments were tried employing various means to prevent, or at least retard, this action, among others gilding the specimen by several different processes. Although by some of these it appeared possible to considerably moderate the oxidation, yet it was found that the sudden cooling always detached the surface layer with its protecting covering, when the whole was plunged directly into water. For work at the higher ranges it was obvious therefore that a sheath of some kind such as that used by Pionchon was necessary. A thin tube of fused quartz will stand plunging suddenly into water when at a fair red heat, but experiments showed that when enclosing a cylinder of iron, even when this is separated from it by a thin layer of inert material, the tube almost invariably breaks up, when dropped directly into cold water.

The following modification of the process was then tried:— Instead of allowing the iron to fall directly into the water, it was caught in a vertical funnel-shaped tube of the form shown in fig. 2. A pad of asbestos-wool on the bottom of the tube was arranged to break the fall. The whole of the

rest of the tube was filled with dried "light magnesia." This substance possesses the curious property of offering practically no resistance to the passage through it of any reasonably heavy body, even when the magnesia is in the

Fig. 2.



Section of magnesia tube.

form of a layer of considerable thickness. The hole formed in it by the falling of the specimen fills itself up almost instantaneously, leaving a layer of about 8 to 10 cm. in thickness to prevent radiation of heat from the hot specimen upwards.

A considerable number of experiments were tried using this arrangement and having the iron specimen protected by a quartz sheath. Experiments at low temperatures gave values agreeing within the limits of error with those got by the earlier method. It was found, however, that above about 900°C. there was a fairly rapid action of the quartz on the surface-film of the iron even when great precautions were taken in having no oxide present on the surface of the sample on its introduction. Besides, an additional objection to the use of quartz in the experiment in this form was that the time required for complete equilibrium to be established in the calorimeter after the specimen had fallen was so great, owing to the low heat conductivity of the quartz in the thickness compatible with strength, that appreciable uncertainty was introduced

into the calculation of the true amount of heat given out in an experiment.

It was found, however, that by sealing up the iron in a thin tube of glazed porcelain, the oxidation difficulty could be avoided, and also that this substance did not possess the disadvantages named in the case of quartz. After prolonged use of one specimen at temperatures up to nearly 1200°C. , the iron on examination was found to be only slightly tarnished, probably from some action of the vapour of the glaze which at these temperatures is quite soft.

It was found best for the experiment to use a somewhat long and narrow sample, and that the iron should not be in the form of a cylinder but of small pieces fitting not too tightly into the tube. This necessitated the weight of the porcelain being a rather large fraction of the whole—about two-fifths of the total weight. It was obvious therefore that an accurate determination of the specific heat of the kind of porcelain used was necessary. Accordingly a set of experiments was made with a specimen consisting of a porcelain tube filled with fragments of the same kind of porcelain weighing 11 grams in all. The results of the experiments are given in the following table :—

Mean Specific Heat of Porcelain between 15° C. and
the temperature given.

Temp. cent.	Spec. heat.
912	·2582
958	·2563
1075	·2539

These experiments indicate a small fall in the specific heat over the range, through which the porcelain was used as protecting sheath in the iron experiments.

The results of the whole three sets of experiments on the iron, made with an interval of more than a year and employing several different specimens, are tabulated together. The most striking feature is the marked drop in the value of S_0^t above 900°. As the specific heat of both oxides of iron is much greater than that of the metal, it is probable that in those experiments where oxidation took place in the furnace, the value found would be too high. The specimen was cleaned from all but a thin firmly adhering film after each experiment, and was weighed both immediately before and after. The time of attainment of thermal equilibrium was of course much shorter in the earlier observations, but even in the series with the magnesia tube it is unlikely that the calculation of the quantity of heat given out should be in error more than about half a per cent. in most of the experiments.

The final result of the whole set of experiments is shown in the table appended, arranged in order of ascending temperature. Preliminary observations with the protected

specimen at lower ranges showed that the method gave a result in agreement with that obtained by the earlier method, and when plotted together it is impossible to distinguish the observations made with the various sized specimens in the different series. Further, no systematic difference was shown between the earlier sets, using a platinum thermometer for temperature measurement, and those done later with thermocouples.

All the values of Q_0^T obtained from the individual observations were plotted on a large scale, and a mean curve drawn from which the results in the later tables were calculated.

	Temp.	Q_0^T .		Temp.	Q_0^T .
1	216	25.5	19	672	95.8
2	216	25.3	20	721	111.5
3	385	49.1	21	741	113.8
4	386	49.7	22	782	123.6
5	392	49.9	23	787	126.6
6	397	50.7	24	818	133.1
7	466	59.9	25	819	133.7
8	489	65.2	26	826	132.4
9	494	66.1	27	842	136.4
10	503	67.1	28	856	139.6
11	520	69.0	29	859	142.6
12	585	80.3	30	866	140.4
13	588	82.3	31	896	146.6
14	597	84.7	32	923	150.7
15	601	85.3	33	1006	156.0
16	613	87.1	34	1077	161.9
17	636	88.9	35	1111	174.4
18	637	91.0	36	1144	190.0

These show the value for Q_0^T obtained by the author, along with the original and recalculated values given by Pionchon's experiments*. It will be seen that the agreement between the two latter is almost perfect up to 900°C . In his paper Pionchon mentions the fact that above this temperature the experiments become very much more difficult, and that the

* Pionchon's experimental values are quoted in Le Chatelier and Boudouard's 'Mesure des températures élevées.' Both in the original French and in the new English translation by Burgess there are, however, two serious errors.

The value for 200° should be 23.5 instead of 22.5
and that for 400° „ 51.5 „ 41.5

to the nearest 0.5 unit.

platinum envelope used suffers deterioration at a fairly rapid rate.

The diminution at 900° and subsequent rise in the specific heat of iron, shown by the author's experiments at the higher part of the range, require confirmation, and some experiments now in progress indicate that it may be possible to construct envelopes of inert materials more refractory than porcelain, which would permit the extension of the range upward of the experiments to above the melting-point of the metal.

Total Heat between 0° and T° .— Q_0^T .

T.	Pionchon's value from formula.	Pionchon's value re- calculated.	The Author's value.
200	23.5	23.5	23.5
300	36.8	36.8	37.0
400	51.6	51.6	51.3
500	68.2	66.0	66.9
600	87.0	83.2	83.8
700	108.4	102.2	104.1
800	135.4	125.0	127.8
900	157.2	146.7	148.0
1000	179.0	166.0	155.7
1100	...	...	168.8

Mean Specific Heat between 0° and T° .— S_0^T .

T.	S_0^T .	T.	S_0^T .
200	.1175	700	.1487
250	.1204	750	.1537
300	.1233	800	.1597
350	.1257	850	.1647
400	.1282	900	.1644
450	.1311	950	.1612
500	.1338	1000	.1557
550	.1361	1050	.1512
600	.1396	1100	.1534
650	.1440		

LII. *On the Transfinite Numbers.*
 By A. E. HARWARD, *Indian Civil Service* *.

I HAVE read with great interest Mr. Jourdain's articles on the transfinite cardinal numbers in recent numbers of the *Phil. Mag.*, which constitute a most important addition to our knowledge of the subject. But as there appears to be considerable logical confusion about the subject at present, and as Mr. Jourdain's proofs of Schröder and Bernstein's theorem and the theorems that $\aleph = \aleph_0 \aleph$ and that $\aleph^2 = \aleph$ appear to me to be open to serious criticism, I have attempted in this article to give a brief restatement of the whole subject with rigorous proofs of the above theorems.

1. The term "class" cannot be defined, but it implies that individuals belonging to a class can be distinguished in some way from individuals which do not belong to that class. It does not imply that the individuals belonging to a class can be thought of collectively as a whole. The term "aggregate" does imply that the individuals spoken of can be thought of collectively as a whole. † Any class which is such that the individuals belonging to it cannot be thought of collec-

* Communicated by the Author.

I must apologise for absence of references to earlier works on the subject, but I have not had any opportunity of referring to any works on the subject except Russell's 'Principles of Mathematics' (Cambridge, 1903), Mr. Jourdain's two papers (*Phil. Mag.* vol. vii. p. 61 & p. 294, 1904), and a paper by Mr. Hardy (*Quarterly Journal of Mathematics*, 1903, pp. 87-94). The extent of my indebtedness to 'The Principles of Mathematics' will be obvious to all who have read that book. As I have ventured to criticise some of Mr. Jourdain's reasoning adversely, I feel bound to say that I owe entirely to him the notion that every aggregate can be arranged in a well-ordered series. I had previously arrived independently at the conclusion that some distinction must be drawn between aggregates and unlimited classes, but I could not see how the distinction could be practically brought into evidence in mathematics. Mr. Jourdain's paper solved the difficulty for me. I am also indebted to the same paper for the proposition that there cannot be a progression of ordinals in inverse order of magnitude.

[This article was originally written in December 1904. The manuscript was submitted to Mr. Jourdain, and I am indebted to him for some valuable comments which have enabled me to correct some errors due to imperfect acquaintance with the literature of the subject.—A. E. H. May 17, 1905.]

† This definition should be regarded as merely provisional, *vide* Note B. It should be observed that I use the term aggregate to denote what Mr. Jourdain calls "consistent aggregate" or "manifold." The term aggregate by itself implies that the elements are to be taken together as a whole. Unlimited classes (inconsistent multitudes) ought not to be called aggregates. The term manifold has, I think, a more restricted meaning, and should not be applied to aggregates generally.

tively as a whole is an "unlimited class," other classes are "limited classes." That there are unlimited classes is easily shown, for the class of all classes is a class, and as such is a member of itself. If all classes could be thought of collectively as a whole the whole would be identical with one among many individuals comprised in itself, which is a contradiction in terms.

Strictly speaking a distinction can be drawn between a limited class *quâ* class, and the aggregate of individuals belonging to it, but as the distinction is not important for my present purpose I shall, to avoid circumlocution, speak of limited classes as "being" aggregates, and *vice versâ*.

It is useful to have a synonym for "class" which can be used when it is necessary to make it evident that the word has no collective import, and for this purpose I use the word "type." By "type" I mean merely "class" (*quâ* "class," and not *quâ* "aggregate") and not some mystical entity which the members of the class are supposed to resemble.

The following propositions follow immediately from the meaning of the terms:—

- (a) Every class which has an unlimited sub-class is itself unlimited; and conversely every sub-class of an aggregate is an aggregate.
- (b) The logical sum* of an aggregate of aggregates is itself an aggregate, for in arranging all the members of a class in an aggregate of aggregates we do thereby exhibit them collectively as a whole†.
- (c) Any class of which the individuals can be correlated one to one with the elements of an aggregate is itself an aggregate.
- (d) Any class of which the individuals can be correlated one to one with the members of an unlimited class is itself unlimited.

2. Two aggregates or classes are said to be "similar" when their elements can be correlated one to one so that one term of either corresponds to one and only one term of the other. The relation of similarity is symmetrical‡ and transitive to all the aggregates similar to a given aggregate from a class such that any two members of the class are similar to one another. By this relation of similarity all aggregates are classified in exclusive§ classes of similar aggregates.

* *Vide* Russell's 'Principles of Mathematics,' p. 117.

† This argument does not amount to formal proof. It is rather an appeal to intuition, *vide* Note B.

‡ *Vide* 'Principles of Mathematics,' p. 218.

§ Classes are said to be exclusive when no two have a common member

These classes or types of aggregates are the "cardinal numbers." An aggregate is said to be infinite when it is similar to a proper part* of itself. The cardinal number of an infinite aggregate is said to be transfinite. If an aggregate M is similar to part of an aggregate N , and if N is not similar to the whole or to any part of M , then the cardinal number of M is said to be "less than" the cardinal number of N .

Before we can assert that of two different cardinal numbers one must be less than the other, we require to prove that of two dissimilar aggregates one must be similar to a part of the other, and that it is not possible for each to be similar to a part of the other.

3. The addition and multiplication of cardinals are best defined in the manner explained in Russell's 'Principles of Mathematics,' ch. xii. The sum of any aggregate of cardinal numbers is the cardinal number of the logical sum of an aggregate of aggregates, of which no two have any common element, and which have those cardinal numbers.

The multiplicative class of an aggregate of aggregates, no two of which have any common element, is the class each of whose terms is an aggregate formed by taking one and only one element from each of those aggregates.

I provisionally assume† as an axiom that the multiplicative class of an aggregate of aggregates is itself an aggregate. The product of an aggregate of cardinal numbers is the cardinal number of the multiplicative class of an aggregate of aggregates which have those cardinal numbers.

The expression $\mathfrak{a}^b$ is defined as the product of an aggregate of cardinal number b of cardinal numbers each equal to $\mathfrak{a}$.

From the above axiom it follows that the class of sub-classes of any aggregate is itself an aggregate, for in forming a sub-class we have as regards each element two alternatives (to include it, or to exclude it), therefore if $\mathfrak{a}$ be the cardinal of the aggregate, then (including *none* and *all* as sub-classes) the cardinal of the aggregate of subclasses is $2\mathfrak{a}$, and it can be shown that its cardinal number is greater than that of the original aggregate. For if we suppose the sub-classes to be correlated one to one with the terms of the aggregate, it can be proved that there is at least one sub-class omitted from the correlation, namely, that which is formed by rejecting or including each element of the aggregate according as it occurs or does not occur in the sub-class correlated with it.

* *I. e.* a part which is not the whole.

† The necessity for assuming some axiom arises from the informal character of our definition of the term aggregate, *vide* Note B.

From this it follows that there cannot be any greatest cardinal number.

4. At this stage it is necessary to introduce ordinal notions*. A simply-ordered series is any aggregate of elements such that there is a certain transitive asymmetrical relation R such that if a and b be any two elements of the aggregate, then either a has the relation R to b , or b has the relation R to a . In one of these cases (it does not matter which we choose) we say that a is before b , in the other we say that a is after b .

It should be observed that the term "series" is by this definition restricted to aggregates.

It may happen that the terms of an unlimited class are related *inter se* in the manner described above, in that case it is called a "simply-ordered serial class," but must not be called a series.

In what follows I use the word series to mean simply-ordered series.

Two series are said to be ordinally similar when their terms can be correlated one to one, so that earlier terms correspond to earlier and later to later†.

Hereafter, whenever I speak of two *series* as similar it is to be understood that I mean ordinally similar.

The relation of ordinal similarity is symmetrical and transitive, so all the series ordinally similar to a given series form a class such that any two members of the class are ordinally similar to each other.

In this way all series are classified in exclusive classes of similar series. These classes or types of series may be denoted by symbols, and these symbols can be combined by addition and multiplication. The symbol $\alpha + \beta$ means the type of the series formed by a series of type α followed by a series of type β .

The symbol $\alpha\beta$ means the type of the series formed by an aggregate of series of type α arranged in a series of type β ‡.

By a part of a series is meant any aggregate of terms belonging to a series. By a segment of a series is meant

* The idea that any progress can be made in the study of cardinals without introducing ordinal notions appears to me to be chimerical. It is true that the notion "aggregate" is logically prior to the notion "series," but it is only by arranging the elements of aggregates in comparable series of some kind that we can determine whether they are similar or dissimilar.

† I. e., if x correspond to x' and y to y' , then if x be before y , x' must be before y' , and *vice versa*.

‡ $\alpha + \beta$ must be distinguished from $\beta + \alpha$, and $\alpha\beta$ from $\beta\alpha$.

any part which is such that if it includes any two terms it also includes all the terms between those two. By an initial segment is meant any part which is such that if it includes any term it also includes all the terms before that term.

A *well-ordered series* is one which is such that every part of it has a first term*.

An unlimited serial class which is such that any aggregate of terms selected from it has a first term is a "well-ordered serial class."

It follows immediately from the definition of a well-ordered series, that every term which has any terms after it has a term which is next after it, and if any part of the series is such that these are terms of the series after all the terms of the part, then there is one term of the series which is the first after all the terms of the part.

The correlation of the terms of two similar well-ordered series is unique, *i. e.*, there is one and only one term of the one series which can correspond to any given term of the other. For the first term of the one must correspond to the first term of the other, and the first term after any initial segment of the one must correspond to the first term after the corresponding initial segment of the other.

If two well-ordered series are not similar then one of them must be similar to some particular initial segment of the other.

For the first term of the one can be correlated with the first term of the other and the next with the next, and the first term after any initial segment of the one with the first term after the corresponding initial segment of the other, and so on until one or other series is exhausted.

Conversely, if a well-ordered series is similar to an initial segment of another well-ordered series, then the two series are dissimilar.

A *progression* is a well-ordered series which has no last term and which is such that each term except the first has a term next before it †.

All well-ordered series can be classified in exclusive classes such that any two of the same class are similar to each other.

* It is not necessary to state that the series (or class) has a first term, as this follows from the definition. In the case of the series this is obvious; in the case of the unlimited class we can proceed as follows. Take any term if this be not the first, then take one before it, if this be not the first then one before it and so on; in this way we must in a finite number of steps arrive at the first term of the class, otherwise we should have an aggregate of terms having no first term.

† It is here tacitly assumed that an enumerable class is a limited class.

These classes or types of well-ordered series are the *ordinal numbers* *.

Any two progressions are ordinally similar. The type of well-ordered series to which a progression belongs is an ordinal number denoted by the symbol ω .

If α and β be two ordinal numbers and if a series of type α be similar to an initial segment of a series of type β , then α is said to be less than β .

It necessarily follows that if α and β be any two different ordinals, then either $\alpha < \beta$ or $\beta < \alpha$.

The class of ordinals is therefore simply ordered.

It is obvious that the initial segments of a well-ordered series themselves form a well-ordered series, for each segment can be correlated with the term next after it. Therefore the initial segments form a series similar to the series obtained by removing the first term of the original series. If the original series be transfinite, the series so obtained is of the same type as the original series.

The ordinals less than any given ordinal β can be correlated in order of magnitude with the initial segments of a series of type β , and therefore they form a well-ordered series.

If β be transfinite the ordinals less than β form a series of type β . If β be finite the series has one term less than β .

The class of ordinals is therefore a well-ordered serial class.

There cannot be a greatest ordinal, for every well-ordered series can be increased by adding on terms at the end. It follows that the ordinals are not a series †, for if they were a series, the type of that series would be the greatest ordinal. The class of ordinals is therefore an unlimited serial class. In other words, the ordinals cannot without contradiction be thought of collectively as a whole, because if we attempt to think of them in that manner we are at once led to the conclusion that there are other ordinals not comprised in the collection of which we are trying to think.

It immediately follows from this that the elements of any aggregate can be arranged in a well-ordered series, for we can take elements from the aggregate one by one and correlate them with the ordinals in order of magnitude, and this process must at some stage come to an end by exhaustion of the aggregate, for if it did not, the whole or some part of the aggregate would be similar to the whole class of ordinals and this is not possible because that class is unlimited.

* Hereafter letters of the Greek alphabet will be used to denote ordinal numbers exclusively.

† I. e. not an aggregate.

(a) Since every aggregate of ordinals in order of magnitude has a first term, it follows that there cannot be a progression of ordinals in inverse order of magnitude. That is to say, if we take any ordinal β_1 and then any ordinal $\beta_2 (< \beta_1)$ and any ordinal $\beta_3 (< \beta_2)$ and so on, we must after some finite number of steps arrive at the first ordinal.

(b) Any part of a well-ordered series is ordinally similar either to the whole series or to some initial segment of it. For we can correlate the first term of the part with the first term of the series and so on, and in this way every term of the part is correlated either with itself or with some term which precedes it in the original series. Therefore if α and β be ordinals and if a series of type α be similar to a part of a series of type β , it follows that $\alpha \leq \beta$.

From this it follows that $\alpha\beta \geq \beta$ (it is necessarily $> \alpha$), also $\alpha + \beta \geq \beta$ (it is necessarily $> \alpha$).

5. We are now in a position to tackle the cardinal numbers. Since every aggregate can be arranged in a well-ordered series, it follows that of two dissimilar aggregates one must be similar to a part of the other, for when they are arranged in well-ordered series, one must be similar to an initial segment of the other.

It is not possible for two dissimilar aggregates to be each similar to a part of the other*.

For let A be an aggregate of cardinal number **a** and B an aggregate of cardinal number **b** and let A be similar to a part of B and B be similar to a part of A.

Let A be arranged in a well-ordered series, and let α denote the ordinal type of this. This contains a part B_1 similar to B, let β_1 denote the ordinal type of this, B_1 contains a part A_1 similar to A, let α_1 denote the ordinal type of this; A_1 contains a part B_2 similar to B, let β_2 denote the ordinal type of B_2 and so on.

We thus get a progression † of parts of the aggregate A similar to B and A alternately. It follows from § 4 b that

* This is Schröder & Bernstein's theorem. I have not seen their proofs, but the proof which Mr. Jourdain gives (Phil. Mag. Jan. 1904, pp. 71-73) appears to me defective. See Note A at the end of this article.

† It should be observed that this argument is not open to the objection which I have urged against the argument used in the proof given by Mr. Jourdain. We do not require any step by step process of selection to get the series of parts of A which are similar to B and to A alternately. The whole series is completely determinate when once we have defined the correlation of all the elements of A with some of the elements of B and of all the elements of B with some of the elements of A.

$\alpha \overline{>} \beta_1 \overline{>} \alpha_1 \overline{>} \beta_2 \dots$ &c. Since there cannot be a progression of ordinals in inverse order of magnitude, it follows that we must after some finite number of steps come to a stage where $\alpha_n = \beta_{n+1}$ or $\beta_n = \alpha_n$, in either case we get a part of the aggregate A which is similar both to A and B. Therefore A is similar to B, and $\mathbf{a} = \mathbf{b}$.

We are now in a position to assert that if $\mathbf{a}$ and $\mathbf{b}$ be any two different cardinals then either $\mathbf{a} > \mathbf{b}$ or $\mathbf{b} > \mathbf{a}$; *i. e.* the cardinals are a simply-ordered class*.

6. It is easy to prove that the finite cardinals form a progression, and that $\aleph_0$ which is defined as the cardinal number of the aggregate of the terms of a progression, is the smallest transfinite cardinal, for any initial segment of a progression is finite.

Since any transfinite well-ordered series with a last term can be converted into a series with no last term by merely transferring a finite number of the terms from the end to the beginning; it follows that any infinite aggregate can be arranged in a well-ordered series with no last term, and since the type of such a series is unaltered by substituting for each term a finite series of n terms, it follows that $n\mathbf{a} = \mathbf{a}$, if $\mathbf{a}$ be transfinite.

If $\mathbf{b} < \mathbf{a}$, $\mathbf{a} + \mathbf{b} < \mathbf{a} + \mathbf{a} = \mathbf{a}$;

but $\mathbf{a} + \mathbf{b} > \mathbf{a}$; $\therefore \mathbf{a} + \mathbf{b} = \mathbf{a} (\mathbf{b} < \mathbf{a})$.

By diagonal enumeration of a progression of progressions it is proved that $\aleph_0^2 = \aleph_0$.

It is easy to prove now that $\aleph_0 \mathbf{a} = \mathbf{a}$ if $\mathbf{a}$ be any transfinite cardinal.

For an aggregate of cardinal number $\mathbf{a}$ can be arranged in a well-ordered series with no last term, *i. e.* a series of type $\omega\beta$ where β is some ordinal.

By substituting for each term of this series a progression of terms we convert the series into one of type $\omega^2\beta$ and the cardinal number of terms in the new series is $\aleph_0 \mathbf{a}$. But in this series each component of type ω^2 can be rearranged in a series of type ω ; the series is then of type $\omega\beta$.

$$\therefore \aleph_0 \mathbf{a} = \mathbf{a}.$$

7. We are now in a position to classify the ordinals. I use

* By following Cantor's method of procedure it can be proved, without using Schröder & Bernstein's theorem, that the cardinals form a well-ordered series. S. & B.'s theorem can then be inferred as a corollary. But the course of the argument is much simplified if S. & B.'s theorem be first proved.

the symbol $C(\beta)$ to denote the cardinal number of the aggregate of terms of a series of type β (β =any ordinal). It is obvious that if $\beta < \gamma$, $C(\beta) \leq C(\gamma)$, because the aggregate of terms in a series of type β is similar to a part of the aggregate of terms in a series of type γ , and therefore (by the definition of "less than" for cardinals) the cardinal number of the latter aggregate is not less than the cardinal number of the former. Therefore it is equal or greater.

The first class of ordinals consists of the finite ordinals.

Two transfinite ordinals β and γ are said to be of the same class if $C(\beta) = C(\gamma)$, and the cardinal number $C(\beta)$ ($= C(\gamma)$) is said to be the cardinal associated with that class.

If β and γ are of the same class then all the intermediate ordinals belong to that class.

For if $\beta < \delta < \gamma$

then $C(\beta) \leq C(\delta) \leq C(\gamma)$;

therefore if $C(\beta) = C(\gamma)$,

then $C(\delta) = C(\beta) = C(\gamma)$.

The classes of the ordinals are therefore segments of the serial class of ordinals, and therefore form a simply-ordered serial class, and this class of segments must be well-ordered, for if there were any aggregate of segments with no first segment it would be possible by taking one ordinal from each to obtain a progression of ordinals in descending order of magnitude.

The cardinal associated with any class is greater than any cardinal associated with any preceding class, for it is by definition not equal to any of those cardinals, and it cannot be less because if $\beta > \gamma$, $C(\beta) > C(\gamma)$.

Therefore the cardinals associated with the classes of transfinite ordinals form a well-ordered serial class similar to the class of classes of ordinals.

There cannot be any last class of ordinals, because if there were the associated cardinal would be the greatest cardinal*.

Therefore the ordinals belonging to any class form an aggregate, for they are part of the aggregate of ordinals preceding the first ordinal of the next class.

The classes of ordinals are not a series (*i. e.* not an aggregate), for if they were so, there would be an aggregate of

* It should be observed that it is at this stage of the argument that I for the first time make use of the axiom assumed in § 3, for the proof that there is no greatest cardinal depends on that axiom.

aggregates of ordinals, comprising all ordinals, and the ordinals would be an aggregate.

The class of classes of ordinals is therefore an unlimited well-ordered serial class similar to the class of ordinals.

The same is true of the class of associated cardinals.

Every transfinite cardinal is included in this class of cardinals because every aggregate can be arranged in a well-ordered series.

The first ordinals of the classes of transfinite ordinals are denoted by the symbols

$$\omega, \omega_1, \omega_2, \dots \omega_\omega, \omega_{\omega+1}, \dots \omega_\beta, \dots$$

(β any transfinite ordinal).

The class of which ω is the first is the 2nd class, the class of which ω_1 is the first is the 3rd class, the class of which ω_ν is the first (ν finite) is the $(\nu+2)$ th class, the class of which ω_ω is the first is the ω th class, and the class of which ω_β is the first (β transfinite) is the β th class.

The cardinal associated with the 2nd class (of which ω is the first ordinal) is $\aleph_0$ and the cardinal associated with the class of which ω_β is the first (β finite or transfinite) is designated by the symbol $\aleph_\beta$.

It is implied by what I have proved above that there can be no cardinal intermediate in value between $\aleph_\beta$ and $\aleph_{\beta+1}$. But as the point is of great importance it is as well to restate the proof.

The elements of an aggregate of cardinal number $\aleph_\beta$ can, by definition, be arranged in a series of type ω_β , and the elements of an aggregate of cardinal number $\aleph_{\beta+1}$ can be arranged in a series of type $\omega_{\beta+1}$.

Let the elements of any other aggregate be arranged in a well-ordered series, call the type of that series γ .

Then, if $\gamma < \omega_\beta$,

$C(\gamma) \leq \aleph_\beta$ (it is in fact $<$, but it is not necessary to assert this),

if $\gamma \geq \omega_{\beta+1}$,

$C(\gamma) \geq \aleph_{\beta+1}$,

if $\omega_\beta \leq \gamma < \omega_{\beta+1}$,

then γ belongs to the same class as ω_β and $C(\gamma) = \aleph_\beta$.

It is easy to show that the cardinal number of the aggregate of ordinals belonging to any class is equal to the cardinal number associated with the next following class. For let x be the cardinal number of the aggregate of ordinals belonging

to the class of which ω_β is the first term. Then

$$\aleph_{\beta+1} = \aleph_\beta + x,$$

$$\therefore x \leq \aleph_{\beta+1},$$

$$\text{if } x < \aleph_{\beta+1} \text{ then } x \leq \aleph_\beta,$$

$$\text{in that case } \aleph_{\beta+1} = \aleph_\beta + x = \aleph_\beta,$$

which is impossible.

$$\therefore x = \aleph_{\beta+1}.$$

8. At this stage it is desirable to state clearly the conditions necessary for proving by induction that a proposition is true for every ordinal (or every transfinite ordinal). If a proposition (or set of propositions) κ is true for every ordinal belonging to some initial segment of the ordinals (or of the transfinite ordinals), and if it be proved that if κ is true for every ordinal (or every transfinite ordinal) less than β , then it is also true for β , then it follows that κ is true for every ordinal (or for every transfinite ordinal). It should be observed that this statement includes both the case where β has a term next before it, and the case where β has no term next before it.

It is not sufficient to prove that if κ is true for β and the ordinals less than β , then it is true for $\beta + 1$.

9. I now proceed to define ω^β where β is any ordinal:—

$$\omega^2 \text{ means } \omega\omega.$$

$$\omega^3 \text{ means } \omega^2\omega.$$

$$\omega^{\nu+1} \text{ means } \omega^\nu\omega \text{ } (\nu \text{ any finite ordinal}).$$

Therefore the meaning of ω^ν is determinate where ν is any finite ordinal. ω^β (β transfinite) is defined as follows:—

In the first place if β denotes a type of series with no last term (*i. e.* if β has no immediate predecessor), then ω^β denotes the first ordinal after all the ordinals ω^γ ($\gamma < \beta$).

If $\beta = \gamma + 1$ then ω^β means $\omega^\gamma\omega$.

It is clear from this definition that ω^β denotes an ordinal uniquely determined, provided that ω^γ denotes an ordinal uniquely determined, for every value of γ less than β . But ω^β does denote an ordinal uniquely determined when β is finite.

$\therefore \omega^\beta$ denotes an ordinal uniquely determined for every value of β .

I proceed to prove that $\omega^{\alpha+\beta} = \omega^\alpha\omega^\beta$.

This follows at once from the definition when β is finite.

It also follows from the definition that if this proposition is true when $\beta = \gamma$, it is also true when $\beta = \gamma + \nu$ (ν finite).

I proceed to prove that if the proposition is true for every ordinal less than β , it is also true for β in the case where β denotes a type of series with no last term.

In this case $\omega^{\alpha+\beta}$ denotes the first ordinal after all the ordinals ω^γ ($\gamma < \alpha + \beta$).

The series of ordinals preceding $\omega^{\alpha+\beta}$ (which is a series of the type denoted by $\omega^{\alpha+\beta}$) can be divided as follows into an aggregate of segments each of type ω^α , arranged in a series of type ω^β .

- | | | | |
|--------------|--|------------------|---|
| (1) | 1, 2, 3 | γ_1 | $(\gamma_1 < \omega^\alpha)$. |
| (2) | ω^α | γ_2 | $(\gamma_2 < \omega^\alpha 2)$. |
| (3) | $\omega^\alpha 2$ | γ_3 | $(\gamma_3 < \omega^\alpha 3)$. |
| . | . | . | . |
| . | . | . | . |
| (δ) | $\omega^\alpha \delta (\delta < \omega^\beta)$ | γ | $\{\gamma < \omega^\alpha (\delta + 1)\}$ |
| . | . | . | . |

In this series δ will among other values take all the values ω^γ ($\gamma < \beta$), and since $\omega^\alpha \omega^\gamma = \omega^{\alpha+\gamma}$ if γ is less than β this series includes all the ordinals ω^γ ($\gamma < \alpha + \beta$), and since the type of series denoted by β has no last term, it is clear that each term of this series is less than some ordinal ω^γ ($\gamma < \alpha + \beta$). Therefore this series is the series of ordinals less than $\omega^{\alpha+\beta}$.

$$\therefore \omega^{\alpha+\beta} = \omega^\alpha \omega^\beta.$$

Therefore this proposition is true for all values of β .

10. I next prove that if β be any ordinal of the 2nd class, then ω^β is also of the 2nd class.

If ω^γ belongs to the 2nd class it is obvious that $\omega^{\gamma+1}$ also belongs to the 2nd class, for

$$\begin{aligned} C(\omega^{\gamma+1}) &= C(\omega^\gamma \omega) = C(\omega^\gamma) \aleph_0 \\ &= \aleph_0^2 = \aleph_0. \end{aligned}$$

If β (of the 2nd class) denote a type of series with no last term, and if the proposition that $C(\omega^\gamma) = C(\gamma)$ is true for all transfinite values of γ less than β ; then it can be shown that this proposition must be true for β also.

For the series of ordinals less than ω^β is partitioned by the ordinals

$$1, \omega, \omega^2, \omega^3, \dots \omega^\omega, \omega^{\omega+1}, \dots \omega^\gamma, \dots (\gamma < \beta)$$

into segments of which these ordinals are the first terms.

Each segment is a series of type ω^γ ($\gamma < \beta$), and therefore

it follows from our premisses that the cardinal number of the aggregate of terms in each segment is $\aleph_0$, for if γ be finite $C(\omega^\gamma) = \aleph_0$, and if γ be transfinite $C(\omega^\gamma) = C(\gamma) = \aleph_0$.

The series of segments is a series of type β , and $C(\beta) = \aleph_0$.

Therefore the cardinal number of the whole aggregate of terms is $\aleph_0^2 = \aleph_0$.

Therefore ω^β also belongs to the 2nd class.

Now it is easy to see that ω^ω belongs to the 2nd class*, for the series of ordinals less than ω^ω is

$$\begin{aligned} &1, 2, 3, \dots \\ &\omega, \omega + 1, \dots \omega 2, \omega 2 + 1, \dots \omega 3, \dots \omega \nu, \dots \\ &\omega^2, \omega^2 + 1, \dots \omega^2 2, \dots \omega^2 \nu, \\ &\omega^3, \omega^3 + 1, \dots \\ &\dots \dots \dots \\ &\omega^\nu, \dots \dots \dots \\ &\dots \dots \dots \end{aligned}$$

i. e. it is a progression of segments each of type ω^ν (for some finite value of ν).

The cardinal number of the aggregate of terms in each segment is $\aleph_0$.

$$\therefore C(\omega^\omega) = \aleph_0^2 = \aleph_0,$$

$$\therefore C(\omega^{\omega+\nu}) = \aleph_0.$$

Therefore, from what I have proved above, it follows that if β be any ordinal of the 2nd class, then $C(\omega^\beta) = C(\beta)$; or, in other words, ω^β also belongs to the 2nd class.

11. I now proceed to prove that $\aleph_\beta^2 = \aleph_\beta$ for all values of β (finite and transfinite).

Consider the two propositions

$$\left. \begin{aligned} &C(\omega^\beta) = C(\beta) \\ &\{C(\beta)\}^2 = C(\beta) \end{aligned} \right\} \dots \dots \dots (A)$$

It has been proved that these two propositions are true if β be any ordinal of the second class.

I shall prove that if they are true for all transfinite ordinals less than β then they are also true for β .

Assuming the propositions A to be true for all transfinite ordinals $< \beta$.

* This can also be verified by arranging the finite numbers in a series of type ω^ω , viz., first the primes in order of magnitude, then the numbers with two factors in a series of type ω^2 , then the numbers with three factors in a series of type ω^3 , and so on.

If $\beta = \gamma + 1$,
 then $C(\beta) = C(\gamma)$ and $\omega^\beta = \omega^\gamma \omega$,
 and $C(\omega^\beta) = C(\omega^\gamma) \aleph_0$,
 $= C(\gamma) \aleph_0$ as $\gamma < \beta$,
 $= C(\gamma) = C(\beta)$.

If β denote a type of series with no last term, then the series of ordinals less than ω^β (which is a series of type ω^β) is partitioned by the numbers

$$\dots, 1, \omega, \omega^2, \dots, \omega^\omega, \dots, \omega^\gamma, \dots (\gamma < \beta)$$

into segments of which these numbers are the first terms. Each segment is of type ω^δ ($\delta < \beta$).

Therefore the cardinal number of the aggregate of terms in each segment is equal to or less than $C(\beta)^*$.

The series of segments is of type β ; therefore the cardinal number of segments $= C(\beta)$.

$$\therefore C(\omega^\beta) \overline{=} \{C(\beta)\}^2.$$

If $\{C(\beta)\}^2 = C(\beta)$, then it follows that $C(\omega^\beta) = C(\beta)$.

Now if β be not the first ordinal of a class, there is some ordinal $\gamma < \beta$ such that $C(\beta) = C(\gamma)$; as the propositions A are true for γ , it follows that $\{C(\beta)\}^2 = C(\beta)$, and therefore in this case the propositions A are true for β also.

But if β be the first ordinal of a class, then we have to prove that $\{C(\beta)\}^2 = C(\beta)$. This it is easy to do with the aid of the assumption that the propositions A are true for all ordinals less than β .

Let $\beta = \omega_\kappa$ the first ordinal of a class.

We can classify all the ordinals less than ω_κ as follows:—

- | | | | | |
|--------------|----------------------|--------------------|----|--|
| (1) | ordinals of the type | ν | or | $\omega\delta + \nu$, |
| (2) | „ „ | $\omega\nu$ | „ | $\omega^2\delta + \omega\nu$, |
| (3) | „ „ | $\omega^2\nu$ | „ | $\omega^3\delta + \omega^2\nu$, |
| . | . | . | . | . |
| (ω) | „ „ | $\omega^\omega\nu$ | „ | $\omega^{\omega+1}\delta + \omega^\omega\nu$, |
| . | . | . | . | . |
| (γ) | „ „ | $\omega^\gamma\nu$ | „ | $\omega^{\gamma+1}\delta + \omega^\gamma\nu$. |
| . | . | . | . | . |

In the above scheme γ and δ stand for any ordinals less than $\omega_\kappa = \beta$, and ν stands for any finite ordinal (1, 2, ...).

It has to be proved that all these ordinals are less than ω_κ .

* Because $C(\omega^\delta) = C(\delta)$ (δ transfinite) and $= \aleph_0$ (δ finite).

for all the transfinite ordinals less than β , then they are also true for β . But they have been proved to be true for every ordinal of the 2nd class, therefore they are true for all transfinite ordinals.

In other words, ω^β always belongs to the same class as β when β is transfinite, and $\aleph_\gamma^2 = \aleph_\gamma$ for all finite and transfinite values of γ .

It follows from this, of course, that $\aleph_\gamma'' = \aleph_\gamma$ (γ finite).

12. The whole of § 9 still holds good *mutatis mutandis*, if instead of ω we take any ordinal number.

In this way α^β is uniquely defined whatever ordinal α and β may be, and it is proved that $\alpha^{\beta+\gamma} = \alpha^\beta \alpha^\gamma$.

It can also be proved that *

$$\alpha^{\beta\gamma} = (\alpha^\beta)^\gamma.$$

For if this be true for all ordinals less than γ , then, if $\gamma = \delta + 1$,

$$\begin{aligned}\alpha^{\beta(\delta+1)} &= \alpha^{\beta\delta+\beta} = (\alpha^\beta)^\delta \alpha^\beta \\ &= (\alpha^\beta)^{\delta+1},\end{aligned}$$

and if γ have no next predecessor then $\alpha^{\beta\gamma}$ is the next ordinal after all the ordinals α^κ ($\kappa < \beta\gamma$), and (since the law is assumed to be true for all ordinals less than γ) it is easily seen that this statement is equivalent to the statement that $\alpha^{\beta\gamma}$ is the next ordinal after all the ordinals $(\alpha^\beta)^\delta$ ($\delta < \gamma$).

Therefore

$$\alpha^{\beta\gamma} = (\alpha^\beta)^\gamma,$$

and therefore this law is universally true.

It is easy to show that ω_κ^β ($\beta \geq \omega_\kappa$) belongs to the same class as β .

For if this law be true for all ordinals γ ($\omega_\kappa \leq \gamma < \beta$), then, if $\beta = \delta + 1$,

$$\begin{aligned}C(\omega_\kappa^\beta) &= C(\omega_\kappa^\delta \omega_\kappa) = C(\delta) \aleph_\kappa \\ &= C(\delta) = C(\beta); \end{aligned}$$

and if β have no next predecessor, then the type of series denoted by ω_κ^β can be partitioned into a series of type β of segments each of cardinal number $\overline{C}(\beta)$.

$$\therefore C(\omega_\kappa) \overline{<} \{C(\beta)\}^2 = C(\beta),$$

$$\therefore C(\omega_\kappa^\beta) = C(\beta) \quad (\beta \overline{>} \omega_\kappa);$$

and therefore this law is universally true.

* The order of the symbols must be strictly adhered to.

In the same way it can be proved that

$$C(\alpha^\beta) = C(\beta) \beta \overline{\succ} \alpha,$$

obviously if

$$\beta < \alpha \quad C(\alpha^\beta) = C(\alpha).$$

13. We can subdivide the classes of ordinals into sub-classes of which the first terms are ordinals $\kappa \epsilon_\beta$, where $\kappa \epsilon_1$ denotes the first ordinal such that

$$\omega_\kappa^\beta < \kappa \epsilon_1 \text{ if } \beta < \kappa \epsilon_1,$$

and $\kappa \epsilon_\beta$ denotes the first ordinal, such that

$$\kappa \epsilon_\gamma^\delta < \kappa \epsilon_\beta,$$

if $\gamma < \beta$ and $\delta < \kappa \epsilon_\beta$.

The cardinal number of such sub-classes in any class is obviously equal to the cardinal number of ordinals in that class.

The following propositions follow immediately from the proposition that

$$\aleph_\kappa^2 = \aleph_\kappa.$$

If in any class we select any aggregate of ordinals, of cardinal number not greater than the cardinal associated with the class, then there are ordinals of the class after all the ordinals of that aggregate. If the ordinals of a class be divided into segments in any way so that there is no last segment, then the cardinal number of the segments is equal to the cardinal number of the aggregate of the ordinals of the class, *i. e.* the cardinal associated with the next class.

I think it probable that

$$\aleph_\beta^\aleph_\gamma = \aleph_\beta (\gamma < \beta)$$

$$\text{and} \quad = \aleph_{\gamma+1} (\gamma \overline{\succ} \beta),$$

but I cannot prove it at present.

United Service Club,
Calcutta.

NOTE A.

*The proof which Mr. Jourdain gives of Schröder and Bernstein's theorem (Phil. Mag. Jan. 1904, pp. 71-73) appears to me to be defective. The proof involves the theorem that if $\mathfrak{a} = \mathfrak{a} + \mathfrak{b}$ then $\mathfrak{a} = \mathfrak{a} + \aleph_0 \mathfrak{b}$. The argument

* A note on these points was submitted with the above article to Mr. Jourdain, but the note was defective and incorrectly expressed, and the reply with which Mr. Jourdain has favoured me, does not appear to me to meet the real objection to his arguments which I failed to make clear in my former note.

employed is that from an aggregate M of cardinal number $\mathfrak{a}$ we can take away in succession aggregates

$$P_1, P_2, P_3, \dots P_\nu, \dots,$$

each of cardinal number $\mathfrak{b}$ so as to leave at each stage a remainder of cardinal number $\mathfrak{a}$. Since when we have performed ν operations we can always perform a $(\nu+1)$ th, it necessarily follows that the series of *operations* which we can perform is a series of type not less than ω . So far the argument is sound, but Mr. Jourdain goes on to infer that some or all of the terms of M can be arranged in a progression of *aggregates* P . It is this step which appears to me to be unsound. We have only proved that we can go on performing a certain endless process*, and we are not entitled to assume that this endless process is equivalent to the completed operation whereby some or all of the terms of M are arranged in a progression of aggregates each of cardinal number $\mathfrak{b}$.

If the arrangement used by Mr. Jourdain be sound there is no reason why we should stop at the conclusion

$$\mathfrak{a} = \mathfrak{a} + \aleph_0 \mathfrak{a};$$

making use of that result we can go on taking from M aggregates

$$P_\omega, P_{\omega+1}, \dots P_{\omega 2}, \dots P_\beta, \dots,$$

each of cardinal number $\mathfrak{b}$, where β may be any ordinal of the second class, at each stage we can leave a remainder of cardinal number $\mathfrak{a}$, so we can always continue the process. The series of operations which we can perform is therefore a series of type not less than ω_1 . So far the argument is sound and the conclusion true†. If we infer from this that we can arrange some or all of the elements of M in a series of type ω_1 of aggregates each of cardinal number $\mathfrak{b}$, we can deduce the conclusion that $\mathfrak{a} = \mathfrak{a} + \aleph_1 \mathfrak{b}$, and from this point we can go on and prove in the same way that $\mathfrak{a} = \mathfrak{a} + \aleph_2 \mathfrak{b}$, and so on indefinitely. These results are obviously untrue if $\mathfrak{a} = \aleph_0$.

* A finite series of operations which can be successively performed is itself an operation, but an infinite series of operations is not an operation but an endless process, and before we treat such a process as equivalent to a completed operation we must justify our procedure.

† The series of *operations* which we can perform is of type ω_1 , even in the case where $\mathfrak{a} = \aleph_0$. In this case the series of operations is of course merely an endless process, and is not equivalent to any completed operation.

The argument must therefore be condemned as unsound. The missing link in the proof can be supplied as follows :— Let M be an aggregate of cardinal number $\mathfrak{a}$ and let P be an aggregate of cardinal number $\mathfrak{b}$ having no term in common with M , and let N be the logical sum of M and P . Then the cardinal number of N is $\mathfrak{a} + \mathfrak{b}$. Since $\mathfrak{a} + \mathfrak{b} = \mathfrak{a}$ we can define a one to one correlation between the terms of M and the terms of N . Let this be done, and let P_1 denote the aggregate of terms of M which are correlated with the terms of P in N ; and let P_2 denote the aggregate of terms of M which are correlated with the terms of P_1 in N , and so on; so that $P_{\nu+1}$ denotes the aggregate of terms of M , which are correlated with the terms of P_ν in N . Each of the aggregates P_ν is by definition entirely comprised in M , and each is of cardinal number $\mathfrak{b}^*$.

It only remains to prove that no two of the aggregates P_ν have any term in common. If it be possible, let the term x be common to P_ν and P_μ ($\nu > \mu$), and let y be the term of N with which x as a term of M is correlated. Then, since the correlation is one-one, y must be common to $P_{\nu-1}$ and $P_{\mu-1}$, and if z be the term of N with which y as a term of M is correlated, then z must be common to $P_{\nu-2}$ and $P_{\mu-2}$; by proceeding in this way we prove ultimately that $P_{\nu-\mu}$ and P have a term in common, but this cannot be because P has no term in common with M . This completes the proof.

Mr. Jourdain's proof that $\aleph_1^2 = \aleph_1$ appears to me to contain an unsound inference of a somewhat similar character. He proves that the terms of a double series (α_β) , such that $\alpha + \beta < x$ ($x =$ a number of the second class) can be correlated with the terms of a single series (α_γ) such that $\gamma < x$. We can then choose another number (of the second class) $x_1 > x$, and we can correlate the terms of (α_β) such that $x \leq \alpha + \beta < x_1$ with the terms of (α_γ) such that $x < \gamma < x_1$. We can continue this process indefinitely; and Mr. Jourdain infers from this that we can correlate all the terms of the double series $(\alpha_\beta)_{\beta < \omega_1}^{\alpha < \omega_1}$ with the terms of the single series $(\alpha_\gamma)_{\gamma < \omega_1}$. The conclusion is true, but it does not follow from the premisses †; all that Mr. Jourdain's argument proves is that

* It should be observed that we do not require any step by step process of selection to obtain these aggregates, they are all completely determined as soon as we define the correlation between M and N .

† I may illustrate my argument by observing that if we take a progression we can correlate every alternate term of it with the ordinals less than β_1 and every alternate term of the remainder with the ordinals $\geq \beta_1$ and so on ($\beta_1 < \beta_2 < \dots < \omega_1$). We can continue this process

we can go on indefinitely performing a certain endless process. We have no right to assume that we may regard that process as completed. In fact it would require a non-enumerable infinity of steps to complete it.

In order to complete the proof on the lines indicated by Mr. Jourdain, it is necessary that some rule or formula (analogous to the formula

$$n = \frac{1}{2}(x+y-1)(x+y-2) + x$$

for finite numbers) should be given by which the required correlation can be established once for all. As I could not succeed in constructing such a formula, I adopted a different method of proof.

I have recently discovered that by a slight modification of Mr. Jourdain's method a simple and rigorous proof can be obtained. Instead of considering the double series ${}_a u_\beta$ take the aggregate of couples* of ordinals less than ω_1 , and let them be arranged in the following scheme:—

$$\begin{array}{cccccccc}
 (1, 2)(1, 3)(1, 4)(1, 5) \dots (1, \omega)(1, \omega+1) \dots (1, \beta) \dots & \{\beta < \omega_1\} \\
 (2, 3)(2, 4)(2, 5) \dots (2, \omega)(2, \omega+1) \dots (2, \beta) \dots & \\
 (3, 4)(3, 5) \dots (3, \omega)(3, \omega+1) \dots (3, \beta) \dots & \\
 (4, 5) \dots (4, \omega)(4, \omega+1) \dots (4, \beta) \dots & \\
 \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\
 \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\
 & & & & (\omega, \omega+1) \dots (\omega, \beta) \dots & & & \\
 \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\
 & & & & & & (\gamma, \beta) \dots & \gamma < \beta
 \end{array}$$

The couples in the same horizontal line have the same lesser component, and the couples in the same vertical column have the same greater component.

Reading by horizontal lines the couples form a series of type ω_1^2 , but reading by vertical columns the couples form a series of different type, and by means of the proposition $\aleph_0^2 = \aleph_0$ it is easy to prove that the type of this latter series is ω_1 .

indefinitely, and there is no point short of ω_1 where we are compelled to stop. But we may not infer that we can correlate all the ordinals less than ω_1 with the terms of the progression. For the residue of the progression is enumerable, and the residue of the other series is always non-enumerable. In assuming that his process can be completed Mr. Jourdain really assumes that the residue of his double series has the same cardinal number as the residue of his single series.

* $(\alpha \beta)$ is the same couple as $(\beta \alpha)$ and $(\alpha \alpha)$ is not a couple.

In precisely the same way, assuming the proposition $\aleph_\gamma^2 = \aleph_\gamma$ to be true for all values of $\gamma < \kappa$, we can prove that the aggregate of couples of ordinals less than ω_κ forms a series of type ω_κ when the couples are arranged according to the order of magnitude of their greater components, couples which have the same greater component being arranged *inter se* according to the order of magnitude of their lesser components.

NOTE B.

The definition given in this article of the distinction between limited and unlimited classes must be regarded as provisional. It is not a formal definition of the kind which we require for mathematical purposes. With this definition we cannot formally prove *ab initio* that any class containing a multiplicity of terms is a limited class; it is only by an appeal to intuition that we can justify the statement that finite and enumerable classes are limited. We can prove that any transfinite cardinals which exist must form a well-ordered class; but we cannot determine how far the class extends without the aid of some assumption.

The formal definition proposed by Mr. Jourdain (Phil. Mag. Jan. 1904, p. 67) cannot be accepted as satisfactory, because we cannot define the class of ordinals (or any class ordinally similar thereto) in a manner free from contradiction without first making the very distinction which we seek to define. Before we can determine the proper mode of formally defining the distinction in question, it is necessary to investigate the subject from the standpoint of the informal definition, by assuming as the basis of formal reasoning general principles suggested by intuition. This is what I have endeavoured to do in the present paper. I may remark that when this paper was written I had not succeeded in framing any suitable formal definition. In a paper of later date*, I have discussed the logical relations between the various axioms which we may assume, and have arrived at a formal definition which appears to me to be satisfactory and which obviates the necessity for assuming any axiom. I need only remark here that the axiom regarding the multiplicative class which I have assumed in the present paper, includes a disputable proposition (viz., that $\mathfrak{a}^{\mathfrak{b}}$ is a cardinal number when $\mathfrak{b}$ is transfinite) which it is not necessary for our purpose to assume. As the axiom is only used to prove that there is no greatest cardinal number, any other axiom from which that can be

* Despatched to the Editors of the Phil. Mag. in March before this paper came back to me for revision.

deduced will serve our purpose equally well. We can for instance assume that the class of types of well-ordered series which can be formed from the terms of a limited class is itself a limited class *. We can then prove that there is no greatest cardinal without making any assumption as to the exponential function.

I may point out that the proofs which I have given of Schröder and Bernstein's theorem, and of the propositions that

$$a+a=a, \quad \aleph_0 a=a \quad \text{and} \quad a^2=a$$

do not depend for their validity on the assumptions which I have made. Those propositions are true for all aggregates, and for all cardinal numbers which exist. But we cannot determine what cardinal numbers do exist without the aid of some assumptions.

From the proposition that $a^2=a$ we can infer that the logical sum of an aggregate of mutually similar aggregates is an aggregate ; but we cannot from this alone infer that the logical sum is an aggregate in the particular case where the cardinals of the component aggregates form in order of magnitude a series with no last term, unless we know independently that there is a cardinal greater than any of those cardinals. It is this latter case of the proposition which I have actually made use of in proving that the class of cardinals is unlimited. In my later paper I have expressly treated this proposition as an axiom.

Burdwan, 17th May, 1905.

LIII. *On the Emission of Negative Electricity by Radium and Thorium Emanations.* By Miss J. M. W. SLATER, B.Sc.; Bathurst Student, Newnham College, Cambridge†.

IN a recent communication made to the Cambridge Philosophical Society‡, Prof. J. J. Thomson has described some experiments which show the existence of a new type of rays in connexion with certain active substances. These are negative rays of the same nature as cathode rays; they differ from the ordinary β ray in moving much less rapidly, so that their ability to penetrate aluminium foil and other media is no greater than that of the positive α particles. This accounts for their not having been detected by the ordinary methods of testing for β rays. Prof. Thomson

* This axiom is really assumed in Cantor's argument.

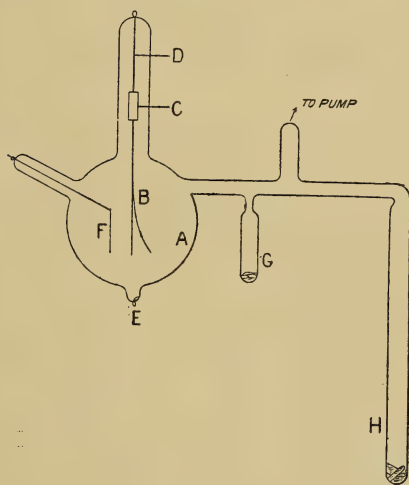
† Communicated by Prof. J. J. Thomson, F.R.S.

‡ Proc. Camb. Phil. Soc. vol. xiii. part 1.

has shown that both polonium and radium give out quantities of these rays; at distances from the active matter exceeding a few millimetres they are found to have entirely neutralized the charge on the α particle. Prof. Thomson suggests that these rays may also be emitted by the emanations of radium and thorium (though they have been shown to give out no rapidly-moving β rays); and that if this is the case, it may offer an explanation of the deposition of excited activity on the negative electrode. The experiments described in this paper were undertaken in order to decide this point.

The method of experimenting and the apparatus employed were essentially the same as those used by Prof. Thomson (fig. 1).

Fig. 1.



The inner surface of the bulb A (7 cm. in diameter) was silvered over, and kept connected through the terminal E to earth. B is a gold-leaf electroscope, mounted on a quartz rod C, and charged by contact with the wire F, which is then earthed. The wire D throughout each observation was kept at the potential to which B was initially charged, thus acting as a guard-ring. In the side-tube G was placed a small quantity of impure radium bromide; H contained hard charcoal, and could be immersed in liquid air, in order to get as good a vacuum as possible.

The apparatus was exhausted to the best vacuum obtainable on the pump, and then sealed off and set up with the tube H in liquid air. The gold-leaf was then charged, and its rate of

collapse observed through a window in the silver lining of the bulb, by means of a microscope with micrometer eyepiece. The potential used was generally ± 200 volts. The average rate of leak when the leaf was positively charged was 6 scale-divisions per hour, the negative leak being only 1.7. It is evident that with the arrangement above described, a large proportion of the radium emanation would distil over into the side-tube H, and have very little effect on the leak in the bulb. To prevent this the liquid air was removed, and the observations repeated. The average $+$ leak was now 47 divisions per hour, the $-$ leak 7.3. Any ionization leak due to air remaining in the bulb would equally tend to discharge both positive and negative electricity, so that this large difference shows clearly that there must be a very considerable emission of slow cathode rays from the radium products in the bulb. It was shown that the effect was not due to the direct action of β rays from the excited activity in the tube G by placing a thick lead screen between this tube and the testing bulb; the leak was not appreciably altered.

These experiments gave no indication as to whether the emission of the negative rays was due to the emanation or to the excited activity, since throughout the experiment both substances must have been present. To test this point, a rather different arrangement was used. (Fig. 2.)

The radium was placed in a small tube (A) ending in a very thin bulb drawn out to a fine point; this was exhausted on the pump and sealed up. It was then placed in a wide tube (B) with a short side branch holding a lump of soft iron; this tube was sealed onto the bottom of the testing bulb, and the whole exhausted as before. The direct effect of the radium in this position was found to be negligible, the leak, both $+$ and $-$, being too small to be measured.

By means of an electro-magnet, the iron was now moved from its place, and made to fall on the little tube containing

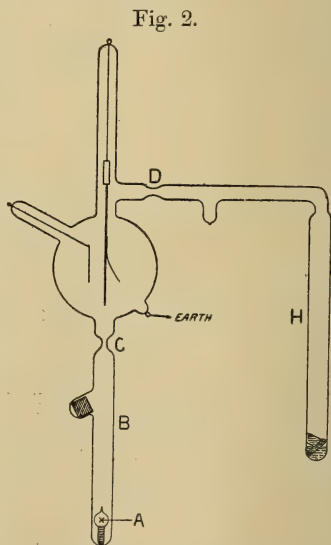


Fig. 2.

the radium; the thin bulb was broken, and the accumulated

emanation was able to diffuse into the testing-vessel. The effect on the leak is shown by the following observations, the zero of time being taken as the moment at which the radium tube was broken.

I. Leaf charged +200 volts. II. Leaf charged -200 volts.

Time in minutes.	Position of Leaf on Scale.	Time in minutes.	Scale-readings.
3	95.0	19	82.9
5	93.6	24	82.7
7	92.7	29	82.5
9	91.0	34	82.2
11	89.6	39	82.8
13	87.9	44	82.1
15	86.5		
Leak per hour, 42.5 scale-div.		Leak per hour, 1.9 div.	

III. Leaf charged +200 volts.

Time in minutes.	Scale-readings.
48	84.1
52	81.2
56	76.9
60	74.7
65	71.0
Leak per hour, 46 div.	

At $1\frac{1}{2}$ hours after the breaking of the bulb, the positive leak reached its maximum value of 52 divisions per hour, and then fell off, the leak after $4\frac{1}{2}$ hours being 39. This was evidently due to the gradual diffusion of the emanation into the branch tubes, and away from the gold-leaf.

The charcoal tube H was now put into liquid air, and an immediate reduction of the positive leak was observed. Measuring time from the moment at which the tube was placed in the liquid air, the readings obtained were as follows:—

Time in minutes.	Scale-readings.
5	70.6
7	70.0
10	68.8
15	68.4
21	67.5
25	67.1
30	66.3
35	65.8
Leak per hour, 9.6 div.	

From these observations it is evident that the greater part of the leak observed was directly due to the presence of the emanation, since for the first 5 or 10 minutes after breaking the radium tube, the amount of excited activity in the bulb must be very small, and the maximum amount would not be produced for several hours. The change caused by cooling the side-tube in liquid air would also occur much less rapidly if the leak observed were mainly due to rays from the excited activity on the walls of the vessel. That very little of the initial leak was due to the presence of ionized gases in the bulb, is shown by the very low value obtained when the gold-leaf was charged negatively; so that the change caused by the liquid air cannot be due merely to an improvement of the vacuum, but must be owing to the condensation of a large proportion of the emanation in the tube H.

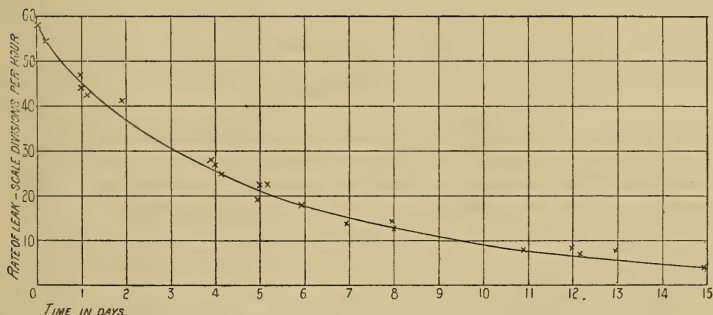
The effect was further investigated by sealing off the tube B at the point C (fig. 2), and observing the rate at which the leak subsequently decreased. This proved to be unexpectedly high, as after two days the rate of leak had fallen to less than half its original value. From this time onwards the rate of decrease was less, having about the normal value for radium emanation, viz. half in 4 days. The rapid fall at first observed may possibly have been due to absorption of the emanation by the charcoal in the side-tube, after the radium was sealed off; moreover, the earlier readings were not very consistent among themselves, so that it was thought desirable to repeat the whole series of experiments under slightly different conditions.

The radium was sealed up in a little tube which was broken as before, and the initial rapid rise in the leak to a maximum, followed by a smaller fall, was confirmed. Then the side-tube H, after being kept for some time in liquid air so as to

get as good a vacuum as possible in the apparatus, was sealed off at D, in order to prevent any variation in the amount of emanation in the bulb owing to diffusion, and absorption by the charcoal. The apparatus was then left for some days, during which the positive leak gradually increased as the emanation in the bulb accumulated. The negative leak also increased slightly, and this was probably in part an ionization leak, due to the evolution of small quantities of gas from the radium; the maximum reading obtained for the negative leak was, however, 7.5, the maximum positive leak being 65, so that the greater part of the latter must still be ascribed to the slowly-moving negative rays given off by the emanation.

The radium tube was then sealed off, and the rate of diminution of the positive leak was measured over a considerable time. No such rapid fall was observed as had been previously obtained; but the rate of leak fell off approximately according to an exponential law, going to half value in four days (fig. 3).

Fig. 3.



It is thus evident that the radium emanation gives off a quantity of slowly-moving negative rays, along with the α particle, during its change to the excited activity.

The action of thorium was similarly examined. A small bulb half full of thorium hydroxide was sealed onto the bottom of the testing vessel, the apparatus exhausted, and the leak determined as before. The mean values obtained were, with a positive charge 11.5 divisions per hour, with a negative charge 2.1 divisions per hour. Here again, therefore, the same slowly-moving negative rays are evidently present.

Later on, the experiment was repeated with a larger quantity

of thorium, so as to increase the effect; and the side-tube, after having been kept some time in liquid air, was sealed off, in order to avoid the irregularities which it had been found to introduce. Immediately before removing the side-tube, the positive leak was 17·2 divisions per hour, the negative 6·0. The liquid-air tube was then sealed off, and the rate of leak at once measured. Starting the observations four minutes after removing the tube, the successive results obtained were—positive leak, 88·5 per hour; negative leak, 33·4 per hour; positive leak, 90 per hour. This large and rapid rise shows clearly that a great part of the effect observed must be directly due to the emanation, which begins to accumulate in the bulb as soon as the cooled side-tube is removed. Owing to its rapid rate of decay (half value in about 1 minute), it only requires a few minutes for the maximum amount to collect.

The bulb containing the thorium was next sealed off, and the positive leak from 6 minutes to 16 minutes afterwards was found to be at the rate of 25·8 divisions per hour. In this space of time the amount of emanation in the testing vessel would have already become almost negligible, and this residual leak was no doubt due to rays given off by the excited activity on the walls. It fell off slowly, at a rate which was approximately that of thorium-excited activity.

From the above experiments, it is evident that in the case of thorium as well as that of radium the emanation gives off a quantity of slow negative rays in changing to the excited activity. In air at the ordinary pressure these rays would be rapidly absorbed; but at the pressures used in these experiments they reach the electroscope and discharge it. The magnitude of the positive leak obtained (compared with the negative) makes it extremely probable that the amount of negative electricity given out by the emanation is considerably greater than the positive electricity emitted in the form of α rays. If this is the case, the residue from which the excited activity is derived must be formed with a positive charge, and its appearance on the negative electrode in an electric field is explained.

In conclusion I have only to express my best thanks to Prof. J. J. Thomson, to whose kind interest and advice during the course of the above experiments, which were carried out at the Cavendish Laboratory, I am greatly indebted.

LIV. *The Union of Hydrogen with Oxygen at Low Pressures caused by the Heating of Platinum.* By the Rev. P. J. KIRKBY, *Fellow of New College, Oxford**.

DURING some experiments on the action of a platinum wire upon a mixture of hydrogen and oxygen at low pressures, I could detect no effect if the wire was left alone. But by heating the wire with electric currents, the gases were readily induced to combine. The rate of combination increased with the temperature of the wire. The combination was gradual, continued as long as the wire was kept heated by the current and ceased directly the current was broken. It followed that the platinum wire when raised to a certain temperature must just begin to set up the chemical reaction in the gas. To determine that temperature for various pressures of the mixed gas was the primary object of this investigation.

The first set of experiments, then, described below were made for the purpose of finding to what temperature a pure platinum wire, placed in a mixture of hydrogen and oxygen at a low pressure and in chemically equivalent volumes, must be heated in order to initiate the chemical union of the gases.

The method adopted was the following :—The hydrogen and oxygen in the equivalent proportions of two volumes to one were simultaneously generated in the same vessel by the electrolysis of pure barium hydrate. The mixed gases were dried and then introduced into the apparatus illustrated in fig. 1. This was in connexion with two McLeod gauges, by means of which any pressure below 40 mm. could be determined accurately.

Fig. 1.



The apparatus of fig. 1 was made from glass tubing about 33 cm. long and about 2 cm. in diameter. Platinum pieces were fused into the glass at A, B. A thin platinum wire, LM, about 20 cms. long had its ends carefully twisted round two other pieces of platinum, PL, QM, so as to make a good electric contact; for the wire was too thin to solder without injuring it. The ends P, Q were then hooked onto the pieces at A, B, and a soldering rod was passed up the glass tube and united the ends of the platinum pieces at P, Q. The wire

* Communicated by the Author.

was then thoroughly cleaned by filling the tube first with a solution of caustic potash, and then with a solution of nitric acid. The tube was then washed out with tap-water and distilled water.

Both the thin wire and the thicker pieces supporting it were of pure platinum, supplied by Messrs. Johnson and Matthey. The thin wire measured $\cdot 112$ mm. in diameter. The thicker pieces supporting the thin wire consisted of wire measuring $\cdot 5$ mm. in diameter. Underneath the whole length of the wire lay pentoxide of phosphorus, which was to absorb water-vapour as soon as it was formed from the gases and which completed the drying of the gases before experiment. This substance is represented by the shaded part of fig. 1.

The glass tube thus prepared was fused together at one end. The other end was fused to the tubing through which the mixed gas had to come.

The mixed gas was generated in a closed vessel. From this it passed through a stopcock into a drying-chamber. A second stopcock admitted the gas into a large mercurial pump, and a third allowed it to pass on into the apparatus of fig. 1 and the McLeod gauges which were permanently connected with this apparatus. The purpose of this third stopcock was to reduce to the smallest dimensions the space where the falls of pressure were to be observed by means of the McLeod gauges. For when a given amount of hydrogen and oxygen combines into water-vapour, and the water-vapour is absorbed, the resulting fall of pressure is, of course, inversely proportional to the space occupied by the mixed gases.

The space in question measured about 400 c.c., and, when put into connexion with the pump by means of the third stopcock, could be rapidly evacuated of its contents.

It should be added that the stopcocks all carried mercury cups so that any leakage of air through the stopcocks into the apparatus was prevented. Moreover, every joint was made by fusing together glass. No pressure tubing was used. Thus every part of the space containing the gas or traversed by the gas was thoroughly air-tight. This was repeatedly verified. When the apparatus had been supplied with dry hydrogen and oxygen at any appropriate pressure, the platinum wire LM was heated by means of an electric current passing through it. During the passage of this current, the difference between the potentials at the points A and B was read on a voltmeter whose terminals were connected to A and B. Meanwhile the current, which

passed through an ammeter, was read at practically the same time. From these two observations and from the known resistance of the voltmeter, it was easy to deduce the joint resistance of the wire and of the platinum pieces which supported the wire. The resistance of those supports was a small quantity which only amounted to a small percentage of the resistance of the wire. Thus the resistance of the wire, and the temperature to which the current had raised it, was known. It was found that as soon as the current was started the wire almost instantly took up a temperature which remained nearly steady but slightly increased. This temperature is determined by an equation between the heat generated per second in the wire by the current, and that lost per second by radiation and by the convection of the gas.

Before each experiment the pressure was ascertained by a McLeod gauge. The current was then turned on, and after running for several seconds (about 30 as a rule) was stopped. The resistance of the wire at the moment when the current was broken was known from readings of the voltmeter and ammeter. The pressure was then again determined. If no fall of pressure was detected, the current, slightly increased, was passed again through the wire and then stopped, and the pressure again observed. In this way the electric current was gradually increased and the observations repeated until a fall of pressure was perceived. The first fall of pressure thus detected was, as a rule, quite unmistakable although extremely small. For if some combination had not taken place in the hydrogen and oxygen, a small temporary *increase* of pressure in the mixed gas, the effect of the hot wire, was noticed just after stopping the current. Moreover, when once the reaction had been started, the temperature to which the wire had to be heated in order to renew the reaction was distinctly lower than the temperature which just initiated the reaction. Thus a repetition of the experiment led to well marked effects, although the chemical combination ceased as soon as the current was cut off.

The temperature of the wire slowly increased during the passage of the current. The current could, therefore, only be permitted to run for a short time if the temperature at which chemical union began was to be fixed within narrow limits. During these experiments the temperature only increased by about two per cent. Hence when chemical action was first detected, the mean temperature, to which the wire had just been raised, might, with an error of only one

per cent. or less, be taken as the temperature necessary to start the reaction.

The latter temperature, θ , of the wire was calculated from its resistance, R , by means of the formula

$$\theta = \theta_0 + \frac{1 + \alpha\theta_0}{\alpha R_0} (R - R_0),$$

R_0 being the resistance which corresponds to the temperature θ_0 . No correction was made, since the "platinum scale" is practically identical with the centigrade scale for the temperatures here determined.

The resistance of the wire was found to be 2.69 ohms at 18° centigrade, and α proved by experiment to be 0.0037, a number which, by the way, testifies to the purity of the platinum. When this number is put for α , and when 2.69, 18 are put for R_0 , θ_0 respectively, the formula becomes

$$\theta = 18 + 107 (R - 2.69).$$

Table I. gives the results of the experiments made with the pure platinum wire. In this

p = the pressure of the hydrogen and oxygen in mm. of mercury ;

C = the current in amperes which heated the wire to the temperature θ ;

R = the resistance of the wire in ohms when the current C was flowing in it ;

θ = the temperature centigrade of the platinum wire, at which it began to cause the chemical union of the gases.

TABLE I.

p .	C .	R .	θ .
39.7	1.29	5.17	283
36.0	1.29	5.2	287
17.6	1.22	5.13	279
9.6	1.12	5.13	279
8.9	1.09	5.03	268
8.6	1.08	5.0	265
4.3	.97	5.17	283
0.86	.64	5.6	329

It has been already pointed out that the method by which θ was obtained might introduce an error of one per cent. into θ . Other errors arise from the use of the voltmeter and ammeter. But the total error to be expected in θ ought not to exceed two per cent. Most of the numbers in the last column may thus be said to be the same. The temperature

which corresponds to the lowest pressure is, however, distinctly higher. All these temperatures are, I believe, well below the temperature of redness; and in view of the large variations of the pressure, it may be said that θ is, roughly, independent of the pressure.

Before making the series of observations from which Table I. was constructed, I had made similar experiments of precisely the same nature with a wire of similar dimensions, but of commercial platinum. It measured about 23 cms. in length and .105 mm. in diameter. Its temperature-rate of increase of resistance was only .00185, just half that of the pure platinum wire. It was thoroughly cleaned as the former one had been, and was fastened into glass tubing in exactly the same way which is described above (see fig. 1), except that the connexions (P, Q in the figure) of the wire with the pieces fused into the glass were not completed by solder.

Instead of the solder, a spring contact was made at the point Q, a little coil in the wire serving as the spring. This contact, although precarious, must have answered very well, for its equivalent resistance evidently, from Table II., remained fairly constant.

In Table II., p has the same meaning as before, but R is the resistance of the wire together with the resistance of its supports, when the wire was just sufficiently heated to start the combination of the gases. The same joint resistance when the temperature of the wire was 18° C. was found to be 5.37. The resistance of the wire alone was nearly 5.1 ohms. Accordingly, if θ is the temperature of the wire corresponding to R ,

$$\theta = 18 + 110 (R - 5.37).$$

TABLE II.

p .	R .	θ .
31.2	7.37	238
29	7.54	257
11.8	7.51	254
7	7.23	223
4.4	7.37	238
4	7.37	238
3.9	7.4	242
2.02	7.47	250

Owing to the defective contact in this apparatus, to which I have alluded, the calculated values of θ in Table II. cannot be considered nearly so accurate as the corresponding

numbers in Table I. Nevertheless, the values of R prove that the temperature of the platinum wire, at which the catalytic action starts, is practically constant for the range of pressures indicated by the tables. In fact, if the numbers θ are affected with a large percentage error, this error is at least a constant one.

To ascertain whether the combination of the hydrogen and oxygen was due to the temperature rather than to the substance of the platinum, a wire of pure silver was substituted for the platinum wire in an apparatus otherwise precisely similar to that illustrated in fig. 1. The diameter of the wire was $\cdot 1$ mm. It was coiled into a spiral; and it was set up in the tube as the platinum wire had been by means of solder. No other arrangements described above were changed, and the method pursued was the same as before.

The pressure of the hydrogen and oxygen was 25 mm. The silver wire, whose resistance at 17°C. was about $\cdot 73$ ohm, was heated for short intervals of time by currents of gradually increasing magnitude until its resistance reached $1\cdot 91$ ohms. After this it gave way, being apparently fused at the point of rupture. Meanwhile no trace whatever of chemical union was indicated by the McLeod gauge. The only change of pressure was a slight rise due to the heating of the gas. If $\cdot 0038^*$ is adopted as the value of the quantity α for this wire, the resistance $1\cdot 91$ indicates a temperature of about 470° centigrade.

It follows that no chemical action is produced by heating silver in hydrogen and oxygen far beyond the temperature to which platinum must be raised in order to cause these gases to unite.

The chemical effect of the platinum is not, therefore, merely produced by heating the gases in the neighbourhood of the wire to a temperature at which their molecular condition becomes unstable and passes into one of greater stability. In other words, the temperature of the platinum which sets up chemical action is far below the temperature at which the gases would of themselves combine into water.

The explanation must be that at the temperature in question the platinum begins to exercise an action upon the hydrogen and oxygen which paves the way for chemical union.

Now it is well known that hot platinum discharges negative

* This number was obtained by experiment upon a pure silver wire of diameter $\cdot 05$ mm., also supplied by Messrs. Johnson & Matthey. It was used for the same purpose as the larger one, but was much too fragile, and broke after being heated to only 180°C. in hydrogen and oxygen at 35 mm. pressure.

ions from its surface. This phenomenon has been the subject of several recent researches. Mr. G. Owen (Phil. Mag. Sept. 1903) investigated it by Mr. C. T. R. Wilson's method of producing condensation-nuclei through sudden expansions. By this means, he detected the discharge of negative ions from platinum at 160° centigrade. The discharge was perceptibly greater when the temperature reached 300° , and became far more copious when the platinum was heated to 400° . Again, I have shown that a proportionate relation exists between the number of molecules of water formed during an electric discharge in rarefied hydrogen and oxygen and the number of ions which pass to the electrodes, so long as the circumstances of the discharge remain unaltered (Phil. Mag. Feb. 1904, Jan. 1905). I suggested in explanation of experimental results, that the molecules of hydrogen and oxygen are dissociated into atoms by the impacts of the ions moving swiftly under the electric force, and that the liberated atoms group themselves anew into molecules of water. Is it not more than possible, in view of Mr. Owen's experiments, that such also is the explanation of the phenomena described in this paper? That when the platinum is heated to a definite temperature, about 280° C., corpuscles are discharged from it with sufficiently high velocities to dissociate the molecules which they strike, and are discharged in sufficient numbers to reveal the chemical change following upon this dissociation.

If this is the process at work, a certain amount of chemical action is doubtless produced by the platinum when its temperature is far lower than that at which the action is perceptible; for even then, according to Mr. Owen, the platinum is discharging ions. But if the chemical action does not absolutely begin when it becomes apparent, it must at least then undergo an abrupt increase.

It therefore becomes necessary to suppose that the corpuscular discharge from platinum makes an abrupt change in extent or character—a change, *e.g.*, in the number or sign of the ions, or in the distribution of their velocities—at the moment when a slight increase in the temperature of the platinum carries the metal up to that critical temperature at which the catalysis is first perceived.

Connected with this temperature is another circumstance, which has been mentioned above. After the platinum had been heated up to this temperature and a little water vapour had been formed, the chemical action could be afterwards renewed by heating the platinum up to a temperature distinctly lower than that which was needed to initiate the action. This

lowering of the catalytic temperature, although considerable, was, nevertheless, limited.

Thus, in the case of the pure platinum wire (see Table I.), the reaction began in the mixed gas at pressure 9.6 mm., when the resistance of the platinum reached 5.13 ohms and its temperature 279° C. The quantity of water formed was small, for the pressure only diminished by about 2 per cent. The platinum wire was then heated by a series of gradually diminishing currents to a series of gradually diminishing temperatures. Each of these currents was passed through the wire for about a minute; and, after each was stopped, the formation of water was detected so long as the resistance of the wire exceeded 4.73 ohms, that is, so long as its temperature exceeded 236°. The whole amount of water formed during these observations was small and only reduced the pressure by about 5 per cent. The gas was then left for about two hours, after which the catalytic temperature had risen to 268°, as recorded in Table I. This observation was confirmed on the following day and was again confirmed eleven days later, the gas meanwhile having been left drying. On account of the slight discrepancy between this result and the temperature 279°, obtained at the pressure 9.6 mm., the gas was then replaced by fresh gas at the pressure 8.6 mm., and fresh observations were taken. The result once more supported the smaller temperature, giving 265° as the catalytic temperature.

The same phenomenon was observed with the impure platinum wire (see Table II.). The chemical action at the pressure 31.2 mm. did not begin until the wire's resistance reached 7.37 ohms. The pressure was then immediately reduced to 4.4 mm., at which the reaction was renewed with a resistance of 6.98 ohms. Next day no combination was recorded until the resistance again reached 7.37 ohms. Thus, the setting up of the chemical reaction was followed by a lowering of the catalytic temperature from about 238° to about 195°, but only for a time. The effect soon passed off. But there is no reason to suppose that the catalytic temperature might not be further reduced, at least temporarily, by first raising the wire to a higher temperature than was done in the present case, or by other means.

These observations prove that the starting of the chemical reaction is bound up with some circumstance which facilitates the renewal of the reaction.

It is difficult to think that this phenomenon is due to the presence in the mixed gas of the water-vapour which is produced by the reaction. For this water-vapour was formed

round the wire, and must, almost as soon as it was formed, have diffused to the pentoxide of phosphorus which was contained in the apparatus, had a large surface, and lay under the whole length of the wire (fig. 1).

The phenomenon is more probably due to another cause. It has been pointed out that some discontinuity in the state of the platinum must be reached when the chemical combination sets in. The effect of this probably persists for a time: probably the platinum does not immediately recover its former condition, and only slowly returns to that condition. Such a hysteresis as this is not uncommon in molecular phenomena. But a more precise conjecture may be hazarded. The chemical reaction observed when the platinum attains a definite temperature has been explained by the assumption that a sudden increase takes place in the corpuscular discharge from the platinum at the moment when the platinum reaches that temperature. Is it not likely that such an eruption would temporarily modify the surface of the platinum, and thus make a similar eruption possible at a temperature rather lower than before?

The experiments recorded in this paper were not carried out under ideal conditions of purity; and indeed under such conditions they would have been impossible. Nevertheless, every reasonable precaution against impurities was taken. The electrolyte (barium hydrate) insured the purity of the hydrogen and oxygen. The platinum wires and the tube which carried them were thoroughly cleaned, and the whole apparatus was made thoroughly airtight. The mixed hydrogen and oxygen never, of course, became wholly free from air. But the proportion of air in the mixture was always very small and steadily diminished; and the consistency which, while this diminution was in progress, continued to characterize the observations shows that the results are the same as if the air had been wholly eliminated.

Summary.

The general conclusions to be drawn from the experiments described or alluded to in this paper may now be summarized.

Platinum in the presence of hydrogen and oxygen at a low pressure does not cause these gases to combine perceptibly unless its temperature exceeds a certain critical value.

That critical value is approximately independent of the pressure of the mixed hydrogen and oxygen, so long at least as the pressure lies between 40 mm. and 2 mm., and is about 275° centigrade for pure platinum.

The same property may belong to impure platinum, and the corresponding critical temperature may be even lower than that of pure platinum.

When the platinum is heated to the critical temperature in question the gases begin to combine, and if the platinum is heated still further, the rate of combination thereby set up in the gases increases with the temperature of the metal.

After heating the platinum sufficiently to start the reaction of the gases, the reaction may be temporarily renewed by raising the platinum to a temperature distinctly lower than that which was required to start the reaction.

The reaction between the gases is not produced by heating them to a temperature at which they combine of themselves, but is probably connected with the corpuscular discharge which is known to be emitted by platinum.

The experiments mentioned in this paper were carried out in the laboratory of Professor Townsend, the Wykeham Professor of Physics, to whom I wish to express my thanks.

LV. *The Electrical Conductivity of Flames.* By HAROLD A. WILSON, M.A., D.Sc., *Fellow of Trinity College, Cambridge, and Lecturer on Physics in King's College, London* *.

(1) *Flames containing no Salt Vapour.*

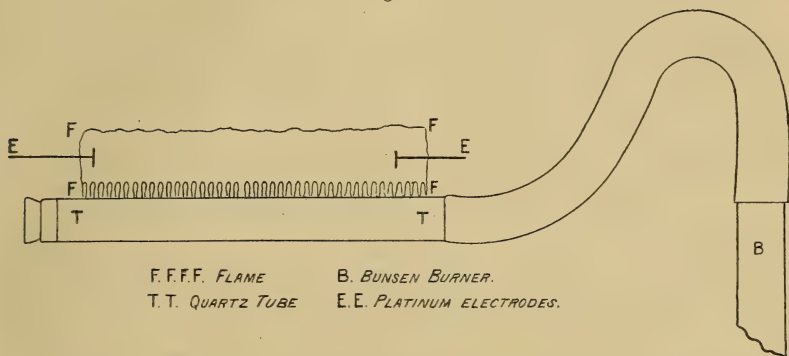
THE following paper contains an account of a series of experiments on the conductivity of a coal-gas flame for electricity between platinum electrodes immersed in the flame †. In order to study the variation of the current with the distance between the electrodes and the fall of potential along the flame, a special burner was used to produce a long narrow flame. The burner consisted of a fused quartz tube 25 cms. long and 2 cms. in diameter, having a single row of holes each 1.5 mm. in diameter parallel to its axis. There were 40 holes uniformly distributed into a row 20 cms. long. One end of the tube was closed with an indiarubber stopper, and the other was connected by means of an indiarubber tube to a large Bunsen burner. The mixture of coal-gas and air supplied by the Bunsen burner was burnt at the row of holes, and formed a fairly uniform flame 20 cms. long and about 4 cms. high. As electrodes two parallel disks of platinum were used, each 1 cm. in diameter. One of these

* Communicated by the Physical Society : read June 16, 1905.

† An excellent account of the present state of our knowledge of this subject by F. L. Tufts is contained in J. Stark's *Jahrbuch der Radioaktivität und Elektronik*, 1 Band, 1 Heft (1904).

was supported near one end of the flame and the other was fixed to a stand sliding in grooves parallel to the flame, so that it could be moved along horizontally in the flame and so placed at any desired distance from the fixed electrode. Since the quartz tube was a good insulator, a current could be passed horizontally from one end of the flame to the other without fear of any of it going through the tube instead of the flame. This arrangement of apparatus is shown diagrammatically in fig. 1.

Fig. 1.



The current through the flame between the electrodes was measured by means of a moving-coil galvanometer, and the potential-difference between the electrodes by means of an electrostatic voltmeter. The electrodes were bright red hot. The variation of the current with the distance between the electrodes for different potential-differences will be first considered. The following table gives the observed galvanometer deflexions. Each number is the mean of several observations ($1 = 8.8 \times 10^{-9}$ ampere).

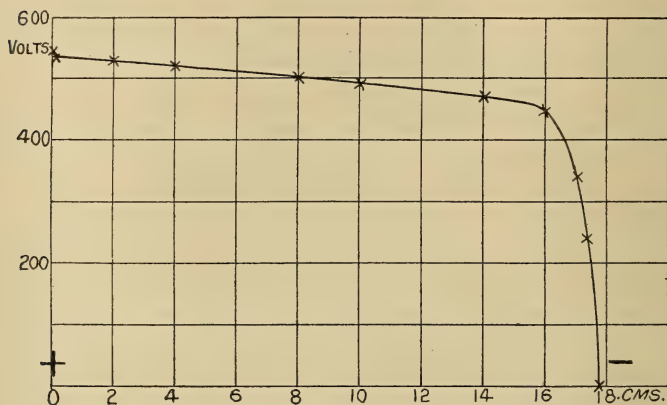
Potential Difference.	Distance between Electrodes.				
	1 cm.	4.5 cms.	9 cms.	13.5 cms.	18 cms.
600 volts.....	310	304	295	280	270
400 „	255	247	240	230	215
200 „	175	170	165	155	143
120 „	130	125	115	104	90
40 „	67	64	57	53	48
20 „	42	39	35	32	29
10 „	22	20	16	14	13
4 „	9	8	7	6	5.3
2 „	5	4.7	4	3.5	3

It will be seen that in every case the current falls off slowly with increasing distance between the electrodes.

The fall of potential between the electrodes was examined by means of a third electrode, consisting of a fine horizontal wire which was put in between the disk electrodes and could be moved along from one to the other. Its potential was measured by means of a Kelvin's multicellular electrostatic voltmeter.

Fig. 2 shows the variation of the potential along the flame obtained as just described. The distance between the disks was 17.7 cms. and the P.D. used 550 volts. It will be seen that there is a sudden fall of potential near each electrode and a uniform gradient in the space between the electrodes.

Fig. 2.



Let n_1 = number of positive ions present per c.c.

n_2 = " negative " " "

k_1 = velocity of positive ions due to one volt per cm.

k_2 = " negative " " "

X = electric intensity in volts per cm.

e = charge on one ion.

i = current density.

Then we have the well-known equation

$$i = Xe(k_1n_1 + k_2n_2).$$

Where X is uniform $n_1 = n_2$, so that $i = Xen(k_1 + k_2)$.

According to this equation we should expect the current through the flame to be proportional to the potential gradient existing between the electrodes. To test this, two small electrodes were put in the flame 0.5 cm. apart, and the P.D. between them was measured by means of a quadrant electrometer. It was found that the P.D. between them was very

nearly proportional to the current through the flame. The following are some of the results obtained :—

Current. (C).	P.D.	P.D. ÷ C.
270	4.0 volts.	0.0148
54	0.8 „	0.0148
18	0.25 „	0.0139

The P.D. between the two small electrodes varied to some extent as they were moved along the flame together, owing to the non-uniformity of the flame, but at any one point not very near either electrode it was always proportional to the current. Near the negative electrode the P.D. became too large to be measured on the quadrant electrometer, and very close to the positive electrode it also became large.

The fall of potential (V) between the disk electrodes may be regarded as made up of three parts—the sudden fall near the negative electrode, say V_2 ; the fall near the positive electrode, say V_1 ; and the fall in the uniform gradient, which is proportional to the current and to the distance between the electrodes. Hence approximately

$$V = V_1 + V_2 + ACd,$$

where A is a constant, C the current, and d the distance between the electrodes.

The following table gives the values of $(V_1 + V_2)$ got by subtracting ACd from V , using the results given above. The value of A was got from the measurements of the electric intensity between the electrodes by means of the equation $ACd = Xd$.

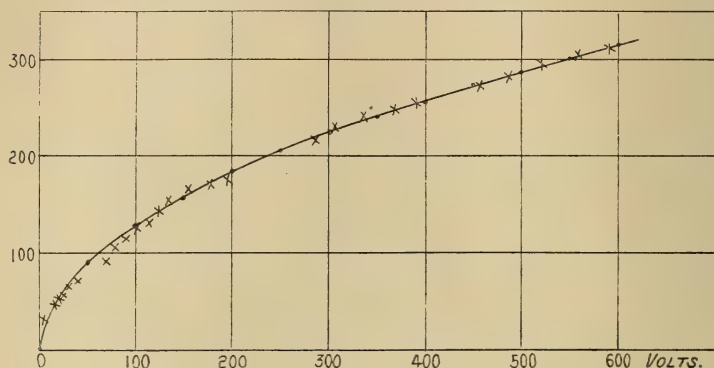
We have $\frac{X}{C} = A = 0.03$, the current being expressed as before in terms of the corresponding galvanometer deflexion. Hence

$$V_1 + V_2 = V - 0.03 Cd.$$

P.D.	1 cm.	4.5 cms.	9 cms.	13.5 cms.	18 cms.
600 volts.....	591	559	520	487	454
400 „	392	367	335	307	284
200 „	195	177	155	137	123
120 „	116	103	89	78	71.4
40 „	38	31.4	24.6	18.6	14.0
20 „	19	14.7	10.6	7.0	4.3
10 „	9	7.3	5.7	4.3	2.8
4 „	4	2.9	2.1	1.6	1.1
2 „	2	1.4	0.9	0.6	0.4

In fig. 3 these values of $(V_1 + V_2)$ are plotted against the corresponding values of the current. The curve drawn is the parabola $C = 12.8\sqrt{V_1 + V_2}$, and it will be seen that nearly all the points lie near this curve. It appears therefore

Fig. 3.



that the current varies approximately as the square root of $(V_1 + V_2)$. Hence we have $C = B\sqrt{V_2 + V_1}$ nearly, where

$$B = 12.8 \text{ for the flame used, or } V_1 + V_2 = \frac{C^2}{B^2}.$$

But

$$V = V_2 + V_1 + ACd,$$

so that

$$V = \frac{C^2}{B^2} + ACd.$$

Prof. J. J. Thomson* has given the theory of the variation of V with C in a uniformly ionized gas. He finds that when the current is far from its maximum possible value so that there is a region between the electrodes where the electric intensity is uniform, then the relation between V and C is of the form $V = aC^2 + bCd$, where a and b are constants. The significance of a and b in Prof. Thomson's formula is moreover precisely that which has been deduced above for $\frac{1}{B^2}$ and A .

It appears therefore that the conductivity of the flame is due to approximately uniform ionization taking place throughout the region between the electrodes, and, further, that the current is always very far from its maximum possible value even when a P.D. of 600 volts is applied to electrodes only 1 cm. apart in the flame.

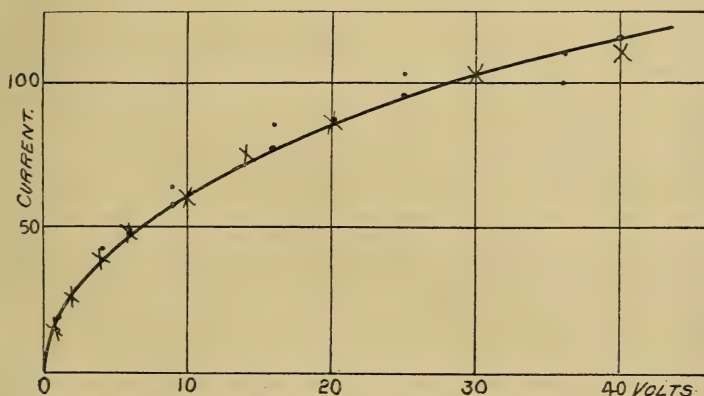
* 'Conduction of Electricity through Gases,' p. 73.

To further test the applicability of the formula

$$V = \frac{C^2}{B^2} + ACd$$

to flames, the current between two vertical parallel platinum disk-electrodes placed symmetrically in an ordinary Bunsen flame was measured. One of these electrodes was 3 cms. in diameter, and the other consisted of a disk 1 cm. in diameter surrounded by a guard-ring 3 cms. in diameter. The annular space between the disk and ring was 0.5 mm. wide. The current between the large disk and the small one was measured, the guard-ring being kept at the same potential as the small disk. The results are shown in fig. 4 for a distance

Fig. 4.



of 1 cm. between the electrodes. The current was very nearly independent of the distance between the electrodes, which means that ACd in the formula $V = \frac{C^2}{B^2} + ACd$ was negligible. Consequently we should have $C^2 = B^2V$. The curve drawn is the parabola $C = 19.25\sqrt{V}$, and it will be seen that the points fall very near to it. The term ACd in this experiment was small, partly on account of the smallness of d and partly because the flame was very hot, which of course diminishes A .

In previous experiments on the variation of C with V with the electrodes near together, no guard-ring was used, so that the relation obtained was probably complicated by the non-uniformity of the field at the edges of the electrodes.

(2) *The Effects due to introducing Salt Vapours into the Flame.*

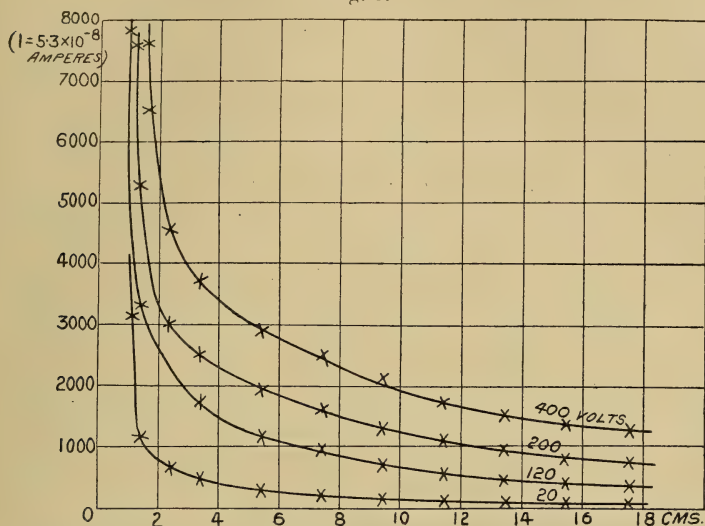
The quartz-tube burner enables the effect of putting salt into different parts of the flame to be easily studied. A bead of an alkali salt on a platinum wire was put into different parts of the flame, the electrodes being about 18 cms. apart. It was found, in agreement with earlier results, that the current through the flame was not affected unless the salt vapour came in contact with the negative electrode, when a large increase in the current occurred. This result was previously explained by supposing that the salt vapour is only ionized when in contact with the hot electrodes, but it now appears that the true explanation is that the salt vapour is ionized anywhere in the flame, and that the absence of effect except close to the cathode is due to the non-uniformity of the potential gradient.

Two wires connected to a quadrant electrometer were supported in the flame so that the electric intensity between them could be measured. In one experiment, using a P.D. of 700 volts between the principal electrodes, the electric intensity was 1.6 volts per cm. at about midway between the electrodes. On bringing a bead of K_2CO_3 into the flame just below the two wires, the P.D. between them fell to a very small fraction of a volt, but the current through the flame was not appreciably increased. The part of the flame occupied by the salt vapour is thus a much better conductor than the rest of the flame. In the experiment just described, the potential fall at the cathode was $700 - 18 \times 1.6 = 671$ volts, and, as we have seen, $C = A\sqrt{V_2}$. Now the effect of putting in the salt is to diminish the electric intensity to practically zero in the part of the flame occupied by the salt, which was a length of about 2 cms. of the flame. Thus putting in the salt must have increased the cathode fall by 3.2 volts, which according to the formula $C = A\sqrt{V_2}$, since V_2 is 671 volts, should have increased the current by one part in 400. Now the deflexion due to the current in this experiment was about 200 mms., so that putting in the salt ought to have increased the deflexion to 200.5 mms.; but since the deflexion was never perfectly steady owing to small oscillations of the flame, so small an increase could not have been detected. It thus appears that the absence of effect on putting in salt, except close to the cathode, is not inconsistent with the view that the salt is ionized anywhere in the flame.

Fig. 5 shows the variation of the current with the distance between the electrodes for several P.D.'s when some K_2CO_3

was put on the cathode. The K_2CO_3 on the cathode fused and volatilized at a very constant rate, so that the current remained almost constant as long as any salt was left.

Fig. 5.



It will be seen that the current falls off rapidly as the distance between the electrodes is increased, this effect being much more marked than in the case of the flame alone.

Fig. 6.

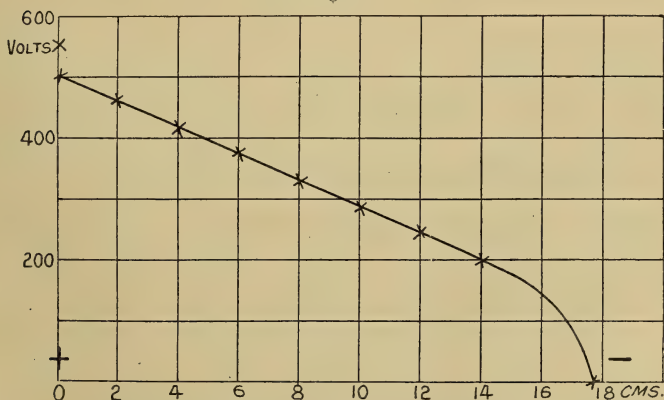


Fig. 6 shows the effect of putting salt on the cathode on the potential gradient. It will be seen, on comparing with fig. 2, that the cathode fall is greatly diminished, while the uniform gradient between the electrodes is increased.

When the distance between the electrodes is large, the current with K_2CO_3 on the cathode is about 50 times greater than the current when no salt is present. The electric intensity in the region between the electrodes, however, is only three or four times greater, so that it seems as though putting salt on the negative electrode increased the conductivity of the rest of the flame. This effect is of a rather unexpected character, and the writer hopes shortly to carry out further experiments with the object of elucidating its real nature.

With salt on the cathode, most of the fall of potential occurs in the uniform gradient between the electrodes, so that the current which is proportional to the gradient varies roughly inversely as the distance between the electrodes and directly as the potential-difference.

It has been shown recently by F. L. Tufts* that coating the negative electrode with calcium oxide greatly diminishes the fall of potential at this electrode, and so increases the current. This effect is clearly exactly of the same nature as the effect of putting an alkali salt on the electrode. Tufts also finds that when the negative electrode is coated with CaO , then putting a bead of salt anywhere in the flame between the electrodes increases the current considerably. It was consequently to be expected that with salt on the negative electrode a similar effect would be obtained, and this was found to be the case. With the electrodes 18 cms. apart and K_2CO_3 on the cathode, putting a bead of K_2CO_3 anywhere in the flame increased the current about 10 per cent., and the same effect was obtained with beads of other alkali salts. Two beads put in at the same time in different positions increased the current about 20 per cent. The portion of the flame occupied by the salt becomes a much better conductor than the rest of the flame, so that the electric intensity in it becomes very small. The electric intensity in the rest of the flame is consequently increased, and the current is proportional to this intensity. The salt from one bead occupied about 2 cms. of the flame, so that with the electrodes 18 cms. apart an increase of $\frac{1}{8}$ in the current ought to have occurred, and with two beads $\frac{2}{7}$. With the electrodes nearer together, the effect of putting in a bead was greater, as was to be expected. With the electrodes 9 cms. apart, putting in a bead increased the current about 20 per cent. It appears, therefore, that the salt vapour is ionized anywhere in the flame, and not only when in contact with the electrodes.

In an earlier paper (Phil. Trans. A. vol. xcii. 1899) the

* F. L. Tufts, *Phys. Zeitschr.* v. p. 76 (1904). See also F. L. Tufts, & J. Stark, *Phys. Zeitschr.* v. p. 248 (1904).

writer suggested that the salt vapour was only ionized when in contact with the hot platinum electrodes. This view enabled a great many experimental results to be simply explained, but it appears now that the true explanation of these results is the smallness of the potential gradient in the flame, except close to the electrodes. It was supposed previously that when the P.D. used was large, so that the current only increased slowly with increasing P.D., that then the current was nearly at its maximum possible value, and so was a measure of the number of ions formed between the electrodes. It is clear now that this supposition was incorrect, and that the current is really always very far from its saturation value. It is of course probable that some surface ionization on the hot electrodes does occur, but, at any rate, when the electrodes are not very near together, it does not appear to be sufficient to appreciably influence the conductivity of the flame.

The view that the salt vapour is ionized throughout the flame was adopted by Arrhenius (*Wied. Ann.* xlii. p. 18, 1891), E. Marx (*Ann. de Phys.* ii. pp. 768, 798, 1900), and by F. L. Tufts and Stark in the papers referred to above.

In conclusion, I wish to say that my best thanks are due to Prof. J. J. Thomson for valuable advice and kind interest shown during the carrying out of these experiments.

LVI. *Contact with Dielectrics.* By ROLLO APPLEYARD*.

OBJECTS.

To examine:—

- (1) Whether tinfoil electrodes, pressed against a sheet of dielectric by indiarubber disks, enable accurate determinations of dielectric-resistance to be made.
- (2) The effect upon dielectric-resistance of change of load on such tinfoil electrodes, in the case of press-spahn.
- (3) The effect upon dielectric-resistance of increase or decrease of voltage in the case of press-spahn between tinfoil electrodes.
- (4) The rate and direction of the change of deflexion in direct-deflexion tests on press-spahn, and to determine in how far these changes result from surface conditions, and in how far from internal stresses.
- (5) The effect of reversals of voltage upon dielectric-resistance.
- (6) The effect of prolonged "electrification."
- (7) To indicate the probable limits of accuracy with mercury electrodes.
- (8) To point out that Price's guard-wire can be used in sheet tests to eliminate leakage over the sheet surface, as well as over the instruments.
- (9) The retentive force between electrodes and dielectrics.

* Communicated by the Physical Society: read June 16, 1905.

CONCLUSIONS.

(a) Except in the case of homogeneous dielectrics, it is misleading to deduce specific values referred to unit cube of the material, from the results of tests on sheets.

(b) With tinfoil electrodes, the apparent resistance of press-spahn diminishes as the load increases, and it attains a fairly constant value at a load of about 400 grammes per cm.²

(c) If, with tinfoil electrodes, the load is gradually diminished after a load of 543 grammes per cm.², the resistance gradually rises, but the rise is less rapid than the diminution in the former case (b).

(d) When the full load with tinfoil electrodes is again restored the resistance falls to its minimum value.

(e) For small loads, with tinfoil electrodes, the 2nd-minute deflexion is in general greater than the 1st-minute deflexion. As the load increases, a point is reached at which these deflexions become approximately equal. For loads greater than about 360 grammes per cm.², the 1st-minute deflexion is in general greater than the 2nd-minute deflexion.

(f) Increase of voltage, with tinfoil electrodes, especially with small loads, behaves like increase of load, apparently increasing the contact area, and diminishing the observed dielectric-resistance. Load, voltage, and the normal effect of "absorption" thus combine to determine the ratio of the 1st-min. deflexion to the 2nd-min. deflexion.

(g) When mercury electrodes are used, the dielectric-resistance, as measured at different voltages, is sensibly the same, even for abrupt and great changes of voltage.

(h) When mercury electrodes are used, the 2nd-minute deflexion is in general never greater than the 1st-minute deflexion. The inference is that when, with tinfoil electrodes, the converse is the case, it arises from imperfect contact, and not from the material itself.

(i) When mercury electrodes are used, the dielectric-resistance, as measured with a voltage applied in a given direction, is sensibly the same as that measured with the voltage reversed, and this equality appears to become greater after a few reversals.

(j) There is a critical load at which tinfoil electrodes yield fairly accurate results. With greater loads there is danger of crushing the material. With a less load the contact is faulty.

(k) With mercury electrodes under the application of 750 volts, with earthing and reversing, respectively, at every fifth minute, the resistance of a sample of press-spahn fell from 43.2 to 41.7 megohms in 51 minutes, thus indicating the limits of precision of resistance tests on such material on a prolonged test where the conditions have time to change.

(l) The earth-readings, in such an experiment as that described under (k), fall steadily towards zero, and their pairs of maximum values are approximately equal, corresponding to the two directions of voltage. These maximum earth-readings are only about 0.2 per cent. of the maximum readings with the battery. Consequently, if only 1 minute is allowed for a sample to recover after the application of a given voltage, the degree of precision of the results will be of the order 0.2 per cent., on account of the previous charge left in the sample.

(m) The retentive force between a disk of tinfoil and a sheet of dielectric, forming part of a condenser, increases rapidly with the voltage; and it vanishes, or becomes extremely small, when the voltage falls to zero.

(n) Small variations between the observed capacity of a condenser tested at different voltages may be due to more intimate contact between the electrodes and the dielectrics at the higher voltages.

IN the course of some experiments described by Mr. E. H. Rayner*, the electrical resistance between the faces of sheets of dielectric was measured, using electrodes of thin tinfoil. The dielectric sheet to be tested was laid between two circular soft indiarubber disks, each disk being covered, on its contact-face, with tinfoil, and the whole was surmounted by a brass disk of the same diameter, upon which lead weights could be placed. This interesting arrangement may be regarded as a compromise between the use of unyielding contact-plates and mercury contact. It is here proposed to investigate the capabilities of this compromise; and as the material known as "press-spahn" was examined by Mr. Rayner in his tests, it is here selected as a convenient dielectric for the present purpose.

Specific Values.—Mr. Rayner reduces his results to kilomegohms per cm.³, but it is perhaps better to state the total megohms measured in each case, without reducing them to specific values. The reason for this is that many materials of this class have a varnished surface, and this varnish has a higher resistance than the middle layers, so that thin sheets yield higher specific values than thick sheets, and specific values cannot be said to be representative of the material generally. Specific values should therefore be avoided, except in the case of homogeneous dielectrics.

Size of Electrodes.—The disks used by Mr. Rayner were 50 cm.² This corresponds to a diameter of 7.98 cms. (3.14 ins.). The electrodes used throughout the following tests are 153.3 cms.², corresponding to a diameter of about 14 cms. (5.5 ins.). The soft indiarubber disks are of the same thickness as those employed by Mr. Rayner, *i. e.* 1 cm.

Load on Electrodes.—The weight used by Mr. Rayner to press upon the electrodes was 20 kilogrammes, so that the average load was 400 grammes to the cm.² In order to give an idea of this load expressed in British units, it may be noted that it is almost exactly the same mean pressure as would be exerted by 1 cwt. distributed over a 5-inch disk. To obtain the same mean pressure on a 5½ in. disk, it had to be loaded with 135.2 lbs. (61.3 kilogrammes). It is clear that with some dielectrics a load of this magnitude is likely to reduce the thickness of the sample or alter its texture, and so invalidate the results; as press-spahn is fairly hard, it is probably not seriously affected, but it will be necessary to examine the effect of changes of load.

* "Report on Temperature Experiments carried out at the National Physical Laboratory." By E. H. Rayner. Journ. Inst. Electrical Engineers, vol. xxxiv. p. 613, May 1905.

Change of Resistance with Volts.—Mr. Rayner observed considerable change of resistance according to the volts applied. For example, using successively 200, 1000, and 200 volts he obtained respectively 22,000, 9600, and 18,500 megohms with a sample of press-spahn. These changes appear to be excessive, and it is necessary to consider whether they are in any way to be associated with the method of applying the test.

Absorption.—Another question of great importance is the change of resistance with time, when the volts are constant. Mr. Rayner, following ordinary usage, calculated the resistance from the deflexion observed after “electrification” had proceeded for 1 minute, and he states that the deflexion after 2 minutes did not differ by more than about 5 per cent. from the deflexion obtained after 1 minute. In view of the valuable information to be derived from observations of the rate and direction of the change of deflexion, the actual readings at the 1 minute and 2 minutes, respectively, should be recorded. In what follows, this will be done.

Surface Leakage.—In the apparatus employed for the following tests, every part of the system was provided, where necessary, with a Price’s guard-wire. A preliminary test was made to examine whether there was surface leakage between the electrodes. For this purpose the edge of a sheet of press-spahn was covered with tinfoil, and this was connected to the guard-wire. It was found, however, that this did not affect the deflexion. The surface* leakage was therefore negligible.

Temperature.—The temperature of the room during all the following tests varied from 15° C. to 16° C.

Humidity.—Press-spahn, as is proved by Mr. Rayner’s results, is very hygroscopic; consequently the resistance cannot be regarded as a fixed quantity. Each set of tests must therefore be considered as distinct from those which precede or follow it. It is assumed that the conditions in regard to moisture remain constant during a short test.

Method of Testing.—The direct-deflexion method was employed throughout, the readings being recorded at successive minutes. In order to indicate the rate of change of deflexion, the actual readings, or their equivalent, are in each case tabulated. The dielectric-resistance is worked out from the 1 minute reading, and is expressed in megohms.

* It may be pointed out that when I first put a guard-wire into practical use, at Mr. Price’s suggestion, he mentioned to me that one of its advantages would be that in sheet-tests the surface leakage could be eliminated in the manner here described.

Experiment 1. (Tinfoil Electrodes. 150 Volts.)

TABLE I.

Load in grammes per cm. ²	Shunt, ohms.	Deflexion, 1 min.	Deflexion, 2 min.	Total, megohms.
22.3	1000	345	369	26.8
31.5	"	379	383	24.4
61.3	"	450	453	20.6
76.0	700	345	347	19.5
90.8	"	358	359	18.7
105.3	"	367	369	18.3
119.8	"	378	378	17.8
197.2	"	430	430	15.6
226.7	500	328	329	15.0
256.1	"	335	335	14.7
282.1	"	340	340	14.4
311.5	"	343	343	14.3
361.1	"	357	356	13.8
390.5	"	360	359	13.6
449.1	"	365	363	13.5
543.3	"	368	368	13.3
449.1	"	373	370	13.2
390.5	"	370	368	13.3
315.0	"	368	367	13.3
256.1	"	366	365	13.4
196.8	"	362	360	13.6
149.7	"	345	344	14.2
90.8	"	335	334	14.7
31.7	"	285	287	17.2
22.3	"	258	259	19.0
197.2	"	349	348	14.1
361.0	"	367	367	13.4
544.1	"	381	379	12.9
22.3	"	266	268	18.5

(The above table shows the effect of increase of load. The electrical test at each load was begun within 1 minute of adjusting the load.)

Comparing the first and fifth columns of Table I., the resistance is seen to diminish from 26.8 to 13.5 megohms as the load increases from 22.3 grammes per cm.² to 449.1 grammes per cm.²; and it is seen to attain a fairly constant value at that load, which is not far from that used by Mr. Rayner (400 grammes). Mr. Rayner took the precaution of allowing 5 minutes for the load to settle before taking a test, so that his results would probably be rather more uniform than these. The increase of resistance as the load diminishes takes place more slowly, and the value 19.0 megohms at minimum load is considerably less than the initial value. The increase of load at this point to 361 grammes per cm.² lowers the megohms again to 13.4; and the resistance is further reduced to 12.9 megohms when Mr. Rayner's load is exceeded. When the minimum load is

suddenly restored, the megohms rise once more to 18·5. It is especially interesting to note that for small loads the 2nd min. deflexion is always greater than the 1st min. deflexion, whereas for loads from 119·8 to 90·8, in the order shown in Table I., the 1 min. and 2 min. readings are at first equal, then the first is greater than the second, and finally they are again equal. The same general result will be noticed in the tests with tinfoil electrodes, which follow. Increase of voltage, as well as increase of the load, appears to have the effect of improving the contact when contact is imperfect. With small loads, it may be assumed that the surfaces are partially out of contact, and that the effect of the current is to improve the contact, so that the resistance *diminishes*, and the deflexion increases. Meanwhile the normal effect of "absorption" tends to produce an apparent *increase* in the resistance, and a point is reached at which no change of deflexion is observed, the effects being balanced. Finally, with the greater load, the films are in more intimate contact and the absorption effect preponderates, the second-minute reading being then less than the first-minute reading.

Experiment 2.—In order to examine the change in resistance with change of voltage, using tinfoil electrodes, the experiment represented by Table II. was carried out. The dielectric was short-circuited for about one minute between each change of voltage.

TABLE II.—*Tinfoil Electrodes.*

Load in grammes per cm. ²	Volts.	Shunt, ohms.	Deflexion, 1 min.	Deflexion, 2 min.	Megohms.
22·3	150	2000	272	285	31·0
"	300	600	288	290	20·2
"	450	500	424	426	17·3
"	600	300	382	383	15·7
"	750	200	341	342	14·8
"	600	300	386	386	15·6
"	450	500	442	443	16·6
"	300	600	316	317	18·4
"	150	2000	344	347	24·5
"	750	200	336	336	15·0
"	150	2000	342	345	24·6
543	150	1000	334	334	14·1
"	300	400	306	304	13·0
"	450	300	354	352	12·8
"	600	200	325	324	12·5
"	750	140	292	291	12·2
"	600	200	326	324	12·4
"	450	300	360	356	12·6
"	300	400	308	308	12·9
"	150	1000	346	345	13·6
"	750	140	292	290	12·2
"	150	1000	348	347	13·5

In the above table it should be observed that with the small load the first-minute readings are never greater than the second-minute readings ; and that the apparent resistance of the dielectric diminishes as the testing-voltage increases. With the greater load, the first-minute readings are never less than the second-minute readings, and although there is a diminution of resistance with increased voltage, the effect is far less marked than in the case of the small load. Increase of load is again seen to have a similar effect to increase of voltage.

At the end of these tests the dielectric sheet was tested between mercury surfaces, proceeding exactly as when using tinfoil electrodes. The results are shown in Table III. For this purpose the dielectric sheet is held in a vertical position symmetrically between two flat rings of ebonite, faced on each side with soft indiarubber. Disks of iron, forming in effect the jaws of a large ebonite vice, are then clamped one against the outer face of each ring. Mercury is poured into each of the hollow spaces between the iron disks and the dielectric, through holes in the top of each disk. The temperature can be conveniently read by placing a thermometer in the mercury.

TABLE III.

Mercury Electrodes.

Volts.	Shunt, ohms.	Deflexion, 1 min.	Deflexion, 2 min.	Megohms.
150	700	325	325	10·5
300	400	387	386	10·3
450	200	299	298	10·2
600	200	397	396	10·2
750	140	350	348	10·2
600	200	396	394	10·2
450	200	298	297	10·2
300	400	387	385	10·3
150	700	328	327	10·4
750	140	348	345	10·3
150	700	330	328	10·3

This table renders it doubtful whether the true resistance of press-spahn varies greatly with the voltage, and it shows that the results obtained with tinfoil electrodes may be misleading when imperfect contact is interpreted as high resistance of the dielectric. In confirmation of the foregoing results, the sheet was left for a day, and was then tested again between mercury electrodes, the current being reversed as indicated in Table IV.

TABLE IV.

Mercury Electrodes.

Volts.	Shunt, ohms.	Deflexion, 1 min.	Deflexion, 2 min.	Megohms.
+150	700	287	284	11·9
-150	700	294	294	11·6
+300	400	342	335	11·6
-300	400	351	350	11·3
+450	200	269	261	11·3
-450	200	273·5	271	11·2
+600	200	358·5	346·5	11·3
-600	200	364·5	360	11·1
+750	140	319	317·5	11·2
-750	140	324	319	11·0
+600	200	365·5	355	11·1
-600	200	367·5	362·5	11·0
+150	700	303·5	302	11·2
-150	700	304	302	11·2

After leaving the sheet for four hours, it was again placed between the tinfoil electrodes, with the following result:—

TABLE V.

Tinfoil Electrodes.

Load in grammes per cm. ²	Volts.	Shunt, ohms.	Deflexion, 1 min.	Deflexion, 2 min.	Megohms.
22·3	+300	1000	459·5	466·5	20·1
"	-300	1000	498	494	18·6
"	+450	400	366	367·5	16·3
"	-450	400	377·5	380	15·8
"	+600	200	291	286	13·9
"	-600	200	292·5	293	13·9
"	+750	200	393	384	12·9
"	-750	200	395	393	12·8
"	+150	1000	232·5	232	20·2
"	-150	1000	217	212	21·6
543	-150	1000	408	408	10·0
"	-750	100	249	246	10·3
"	-150	1000	423	422	9·7

The final three tests with the greater load were intended to indicate the true resistance, the current therefore was not reversed; they are seen to be fairly uniform, and they justify the use of tinfoil electrodes for rough tests where mercury cannot be obtained. The fact that these three values are all lower than those given with the mercury apparatus on the same afternoon (Table IV.) is probably accounted for by the

reduction of thickness due to the heavy load. It was $1\frac{3}{4}$ cwts. on a $5\frac{1}{2}$ inch disk.

The next experiment was made with a view to testing the resistance with tinfoil electrodes, as nearly as possible under the same conditions as those adopted by Mr. Rayner. For this purpose the sheet was allowed to rest for $17\frac{1}{2}$ hours. It was then put into the apparatus, and the weights on the disk area of $153\cdot3$ cm.² were adjusted to give a mean load of 400 grammes per cm.² Five minutes was then allowed, as in Mr. Rayner's experiments, for the conditions to become steady, before applying the voltage. Only one minute was allowed between the successive voltages. The readings and results were as follows:—

TABLE VI.
Tinfoil Electrodes.

Volts.	Shunt, ohms.	Deflexion, 1 min.	Deflexion, 2 min.	Megohms.
+150	1000	266·5	266·5	17·6
+750	140	240·0	238·0	14·9
+150	1000	284·5	283·5	16·5
+750	140	237·0	235·5	15·0

The results are fairly uniform, but the diminution of resistance with increase of volts is still marked.

For purposes of comparison, the dielectric sheet was then immediately placed in the mercury apparatus, and tested in precisely the same way, with the following results:—

TABLE VII.
Mercury Electrodes.

Volts.	Shunt, ohms.	Deflexion, 1 min.	Deflexion, 2 min.	Megohms.
150	1000	342	339	13·7
750	140	259	255	13·8
150	1000	332	331·5	14·1
750	140	255	252·5	14·0

A comparison between Table VI. and Table VII. seems to indicate that although the tinfoil electrodes under a load of 400 grammes to the cm.², and with an allowance of 5 minutes

for settling down, are brought into fair contact with the dielectric, the contact is not so perfect as when mercury is employed. It is curious to note here, as in all these experiments, that where tinfoil electrodes are used the resistance appears to decrease with the increase of voltage, whereas with mercury contact this effect practically vanishes. I have shown (Proc. Physical Soc. vol. xiii. p. 157, 1894) that with celluloid sheets between unyielding metal plates, the change of apparent resistance with voltage is remarkably great. It is a matter of considerable importance in the study of dielectrics, and I have purposely described the experiments in detail so as to endeavour to establish the fact that what is often called change of dielectric resistance with voltage is, as a rule, merely a surface effect, and that it can be greatly reduced, if not entirely eliminated, by ensuring perfect contact with the electrodes. Moreover, the effect is generally found associated with so-called "negative electrification," the second-minute reading being greater than the first-minute reading.

In order to see how little the true resistance is affected by abrupt changes of voltage, an examination may be made of Tables III., IV., and VII., where only one minute was allowed between each change for the dielectric to lose its charge. The residual effect can be estimated by switching off the current at the end of a given minute, and immediately connecting the insulated electrodes through the galvanometer to earth. With a view to avoiding too lengthy a table illustrating this, I have calculated the values of $[(\text{deflexions}) \times (\text{multiplying power of shunt})]$ in the following case, omitting shunts, and omitting megohms, from the tabulated figures. It may be stated, however, that the megohms, worked out from the 1st, 11th, 21st, 31st, 41st, and 51st mins., are respectively 43·2, 42·6, 42·3, 42·2, 42·0, and 41·7. The procedure was to apply the battery for 5 mins., and at the end of the 5th minute to switch the battery out of circuit and connect the insulated electrode to earth through the galvanometer, taking earth-readings for five minutes. At the tenth minute the battery was reversed, and the same process was continued in a cycle of reversals and earthings up to the 53rd minute. In the following table the reversals and earthings are sufficiently indicated by the signs of the deflexions, and by the smallness of the earth-readings in comparison with the battery readings. The fall after each change of connexions was perfectly steady, and in no instance was there anything approaching a rise after the deflexion corresponding to a given change had been recorded. On another day, after allowing the sheet to rest for 12 hours;

a similar test was applied, using 150 volts. The resistance calculated from the 1st min. (+) was 46·8 megohms. The resistance calculated from the 11th min. (−) was 46·5 megohms, a change of only 0·3 megohm against the change of 0·6 in the corresponding test with 750 volts. The earth-readings at the 6th min. and 16th min. were respectively −3·5 and +2, as against −10·5 and +7·5 with 750 volts. The earth-readings therefore are not directly proportional to the applied voltages originating them.

TABLE VIII.
Mercury Electrodes (750 volts).

Min.	Deflexion.	Min.	Deflexion.	Min.	Deflexion.
1	+4630	19	+3	36	+7·5
2	+4589	20	+2	37	+5
3	+4548	21	+4721	38	+3·5
4	+4524	22	+4679	39	+3
5	+4499	23	+4638	40	+2·5
6	−10·5	24	+4606	41	+4762
7	−7·5	25	+4565	42	+4729
8	−6·0	26	−9	43	+4696
9	−5·0	27	−6	44	+4655
10	−4·5	28	−5	45	+4622
11	−4696	29	−4	46	−9
12	−4679	30	−3·5	47	−6
13	−4671	31	−4737	48	−5
14	−4630	32	−4713	49	−4
15	−4606	33	−4696	50	−3·5
16	+7·5	34	−4663	51	−4787
17	+5	35	−4639	52	−4762
18	+3·5			53	−4729

The maximum earth-reading is 10·5 with 750 volts. The effect of this residual charge upon subsequent readings is only 0·23 per cent.; or in terms of resistance say 0·1 of a megohm in the 42 megohms here concerned. In comparison with the effects of changes of humidity, it is probably negligible. The uniformity of the earth-readings in the above table is instructive. The (+) current applied at the 1st min. strikes the dominant note, and gives an earth-reading which at the 6th min. is −10·5. At the 11th min. the (−) current is applied, and gives an earth-reading which at the 16th min. is +7·5, *i. e.*, 3 divisions less than the corresponding earth-current of the previous charge; and we may assume that if the earth-current at the tenth minute had been allowed to act alone it would have fallen from −4·5 at the tenth to −3 at the sixteenth minute. At the 21st min. the (+) current is again applied; the corresponding earth-current at the 26th min.

is -9 . And it is seen, taking the readings at the 6th, 16th, 26th, 36th, and 46th min., that we obtain the sequence $-10\cdot5$, $+7\cdot5$, -9 , $+7\cdot5$, -9 ; a further indication that with mercury electrodes the apparatus conditions are trustworthy. These earth-readings were taken without a shunt, so that they are actual records of direct deflexions.

Retentive Force.—It seemed desirable to obtain positive evidence as to the magnitude of the retentive force between the tinfoil and the press-spahn, and to determine whether it was proportional to the voltage. For this purpose, a flat circular disk of gutta-percha, of the same size as the electrodes used in the foregoing experiments, was perforated over about half its surface with holes 1 cm. in diameter. A smooth sheet of thin tinfoil, not perforated, was attached to one side of this disk by a little chloroform. This arrangement allowed a certain amount of freedom of movement to the tinfoil, so that it could conform to the surface of a sheet of press-spahn when placed in contact with it. The disk was then suspended above a sheet of press-spahn, in a horizontal plane, from the beam of a balance, and its weight was counterpoised. Beneath the sheet of press-spahn was a second sheet of tinfoil connected to earth, so that the combination formed a condenser, the upper electrode of which was thus in balanced contact with the dielectric. The weight necessary to counterpoise the disk was first determined with no voltage between the electrodes, and the weight necessary to relieve it from contact was then measured at successive voltages. Check readings of the weight, taken between the various applications of voltage, never exhibited a difference greater than 1 centigramme for the normal (no-voltage) counterpoise. If there was residual coherence, it was therefore very small. The readings in a characteristic case were:—

TABLE IX.

Volts.	Counterpoise. Total grammes.	Retentive force, grammes.	Ratio of Retentive forces.	Ratio of Volts.
0	22·04	0	0	0
750	72·00	52	104	5
0	22·04	0	0	0
600	50·00	28	56	4
450	25·00	3	6	3
300	23·00	1	2	2
150	22·50	0·5	1	1
300	23·00	1	2	2
450	25·00	3	6	3
600	49·00	27	54	4
750	66·00	44	88	5

The retentive force between the tinfoil and the press-spahn increases far more rapidly than the voltage, the contact must therefore be more perfect with the higher voltage; this accounts in some measure for the changes of apparent dielectric-resistance with voltage shown in Tables II. and V. It is possible that a similar effect may account for small differences of capacity observed with different voltage with some tinfoil condensers, and it may also serve to explain to some extent certain abnormal "electrifications" with cables, where for any reason there is imperfect contact between the conductor and the dielectric.

LVII. Dewar's Method of producing High Vacua. By Lord BLYTHSWOOD, LL.D., and H. S. ALLEN, M.A., B.Sc.*

§ 1. **I**T has long been known that charcoal, freed from gas by heating to redness, is capable of absorbing gases in large quantities. The subject was first investigated by Saussure, and a detailed examination of this property of charcoal was made by Hunter (Phil. Mag. [4] vol. xxv. p. 364, 1863, & xxix. p. 116, 1865). Of all the charcoals he examined, that made from the cocoanut had by far the greatest absorbing power, one volume of the charcoal absorbing 171·7 volumes of ammonia, 17·9 of oxygen, 15·2 of nitrogen, and 4·4 of hydrogen. In 1875 Professor Dewar employed this property of charcoal to improve the vacuum in a vessel exhausted by a mercury-pump. His recent discovery that when the charcoal is cooled to the low temperatures now available it is capable of absorbing still larger quantities of gas, is of great importance. This result promises to be of the greatest service in scientific research, and it is even possible that it may have important commercial applications.

§ 2. Sir James Dewar† has described experiments to test the amount of exhaustion reached by the use of a given weight of cocoanut charcoal. He found that 30 grams, cooled to the temperature of liquid air, absorbed so much air that the pressure in an electric-discharge tube of 1300 c.c. capacity fell from 760 to 50 mm. of mercury. Starting with the tube initially at half an atmosphere, the exhaustion reached was now beyond the striæ stage. A further experiment, starting with one-fourth of an atmosphere, gave a vacuum through which no discharge passed.

* Communicated by the Authors. A preliminary account of some of the experiments described below has been given by one of us in a paper read before the Royal Philosophical Society of Glasgow, March 22nd, 1905.

† Proc. Roy. Soc. lxxiv. pp. 122-131 (1904).
Phil. Mag. S. 6. Vol. 10. No. 58. Oct. 1905.

§3. Experiments carried out by the Authors show that it is only necessary to increase the size of the charcoal receptacle in order to produce a high degree of exhaustion in a large discharge-tube without the use of a pump. The first attempt made (Jan. 30, 1905) was to exhaust an old X-ray bulb, three inches in diameter (capacity 250 c.c.). This was attached to a drying-tube containing phosphorus pentoxide, and to a bulb of capacity 200 c.c. containing about 65 grams of charcoal*. The apparatus was set aside for some days before it was used so that the air might be dried. Before the charcoal bulb was cooled, both it and the discharge-tube were heated strongly to drive off as much gas as possible. The apparatus was then sealed, and when the charcoal bulb was sufficiently cool it was placed in liquid air. During the process of exhaustion the focus tube was heated, and during the later stages the discharge was passed through the tube to drive off gas from the electrodes.

The tube was sealed off after the charcoal had been cooled for one hour, and the vacuum produced was so good that the tube had to be heated in order to allow the discharge to pass through it.

A second focus tube, four inches in diameter (capacity 675 c.c.), was successfully exhausted with the same charcoal bulb (Jan. 31).

§4. In the next experiment the drying-tube was discarded altogether, and a new X-ray bulb, five inches in diameter (capacity 1150 c.c.), was attached to the charcoal absorber (fig. 1). The following notes show the progress of the exhaustion :--

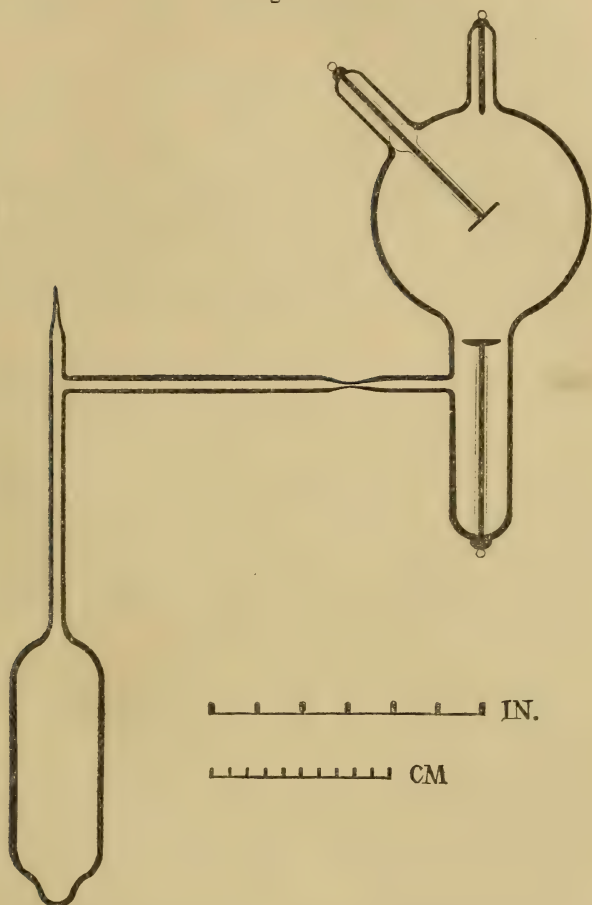
- 11.25 A.M. Commenced to heat discharge-tube and charcoal.
- 11.35. Apparatus sealed.
- 11.45. Charcoal bulb placed in liquid air.
- 11.50. Discharge begins to pass.
- 12 NOON. Both electrodes covered with a velvet glow.
- 12.7 P.M. Discharge fills the whole bulb.
- 12.9. Some green fluorescence.
- 12.12. Shadow of the electrodes thrown on the tube by cathode rays.
- 12.30. X-rays beginning.
- 12.50. Discharge-tube sealed off.

The result of this experiment was a somewhat "soft" X-ray tube.

Several similar X-ray tubes were exhausted, and exhibited the same succession of changes at practically the same time

* The charcoal used in the experiments we have carried out was of two kinds, the first made from *lignum vite*, the second from the shell of the cocoanut. The material was packed into an iron pot closed with a lid provided with a small vent hole, and was heated in a gas furnace for some hours.

Fig. 1.



from the beginning of the experiment. The amount of liquid air used in the course of the exhaustion was determined by weighing the flask in which it was stored at the commencement and conclusion. Two distinct experiments with the charcoal receptacle already described gave 340 grams and 295 grams as the amount of liquid air evaporated in exhausting a five-inch bulb. About 800 grams of liquid air were required to cover the charcoal absorber, which was placed in a cylindrical Dewar's vessel 10 centimetres in diameter.

§ 5. Another experiment with a larger absorber, of 550 c.c. capacity, containing 216 grams of charcoal, gave penetrating X-rays after the charcoal had been cooled for an hour and a

half in liquid air. In this case 420 grams of liquid were evaporated, but the amount required to cover the bulb at the outset was only 570 grams.

§ 6. In order to obtain a still higher vacuum in a large discharge-tube without the use of a pump, *two* charcoal receptacles were employed. At first No. 1 was cooled in liquid air, whilst No. 2 was strongly heated to drive off as much gas as possible. Then No. 1 was sealed off with the blowpipe, and No. 2 was cooled. One hour was devoted to each operation. After the X-ray tube was sealed off and had been allowed to cool, it was found that the discharge from a large induction-coil would pass across a five-inch spark-gap rather than go through the tube.

§ 7. These experiments show that it is practically possible to apply Dewar's method to the exhaustion of large receivers, and that this method requires but a moderate amount of liquid air. It is particularly useful when it is desirable to avoid the presence of mercury vapour in the vacuum-tube, as in Geissler tubes for spectroscopic analysis. It is possible to dispense with drying-tubes, though this procedure is not recommended in general as the charcoal is rendered less efficient for subsequent work. The method is a rapid one, and a further recommendation to the amateur glass-blower is the simplicity of the apparatus required.

EXPERIMENTS TO DETERMINE THE AMOUNT AND RATE OF ABSORPTION OF AIR BY CHARCOAL COOLED TO THE TEMPERATURE OF LIQUID AIR.

(i.) *Absorption by Charcoal in Presence of an Excess of Air.*

§ 8. A number of experiments were carried out to find the absorption by charcoal in the presence of a volume of air greater than the charcoal could absorb when cooled to the temperature of liquid air.

A small quantity (2 grams) of charcoal was placed in a glass bulb in communication with a barometer-tube. Glass vessels of known volume could be attached to the apparatus in order to determine the effect of altering the initial volume of air experimented upon.

Let v denote the volume of the bulb which is cooled to the temperature of liquid air,

V denote the volume of the connecting tubes and the attached vessel.

Initially we have a volume $V + v$ at pressure P and temperature T . Suppose that when the small bulb is cooled to a temperature t , without any charcoal in it, the pressure in the apparatus is reduced to p' .

Then we have a volume V at pressure p' and temperature T , and a volume v at pressure p' and temperature t .

So that

$$V \frac{p'}{P} + v \frac{p'}{P} \frac{T}{t} = V + v ;$$

or

$$\left(V + v \frac{T}{t} \right) \frac{p'}{P} = V + v. \quad . \quad . \quad . \quad . \quad (i.)$$

When the bulb contains charcoal the final pressure reached will be less than when it contains air, in consequence of the absorption of air by charcoal. Denote this pressure by p . Then the volume of air *not* absorbed by the charcoal (measured at pressure P and temperature T) is

$$\begin{aligned} V \frac{p}{P} + v \frac{p}{P} \frac{T}{t} \\ = (V + v) \frac{p}{p'} \quad \text{from (i.)} \end{aligned}$$

Therefore the volume of air absorbed by the charcoal

$$= (V + v) \left(1 - \frac{p}{p'} \right). \quad . \quad . \quad . \quad . \quad (ii.)$$

§ 9. The following table gives the results of experiments carried out in this way :—

TABLE I.

	Total Volume. $V + v$.	p' .	p .	Volume of Air absorbed by Charcoal.
I. ...	33.5 c.c.	393 mm.	180 mm.	18.1 c.c.
II. ...	70.8 c.c.	520 mm.	386 mm.	18.2 c.c.
III. ...	93.4 c.c.	558 mm.	428 mm.	21.7 c.c.
IV. ...	125.0 c.c.	598 mm.	495 mm.	21.5 c.c.
V. ...	674.0 c.c.	726 mm.	702 mm.	22.3 c.c.

The figures in the last column show that the volume of air absorbed by the charcoal is, to a first approximation, independent of the quantity of air present in the apparatus at the commencement of the experiment.

The small differences observed in the figures giving the amount of air absorbed may be attributed to the influence of pressure*. In accordance with the principle of Least Action the effect of raising the pressure is to make the reaction take

* Mellor, 'Chemical Statics and Dynamics,' p. 435.

place in that direction which is accompanied by a decrease in volume, that is in this case to increase the volume of air absorbed.

§ 10. For comparison with these results, the amount of air absorbed by the charcoal at atmospheric pressure was determined by direct experiment. The charcoal bulb was immersed in liquid air for six minutes, as it was considered that this time was long enough for the charcoal to absorb its full supply of air from the atmosphere. The bulb was then allowed to regain the temperature of the room, and the air expelled was collected over water in an inverted burette. The mean of three readings gave 69.3 c.c. as the total volume. A blank experiment was then carried out without the charcoal. The mean of three readings was 41.2 c.c. The difference, 28.1 c.c., gives the volume of air absorbed by 2 grams of charcoal when cooled to the temperature of liquid air*.

§ 11. Observations were made with the apparatus already described to determine the *rate* at which air was absorbed by the charcoal. Readings of the height of the mercury column were taken at intervals of a quarter of a minute. For one minute after the application of the liquid air, the readings were irregular in consequence of the cooling of the bulb, but at the end of that time the liquid air had ceased to boil violently, and regular readings could be obtained. The mercury in the barometer-tube reached a steady height within from six to ten minutes after the commencement of the experiment.

The readings obtained in Experiment No. 1 are given in the following table (Table II.) :—

TABLE II.

$\left. \begin{matrix} t. \\ \text{(mins.)} \end{matrix} \right\}$	1.	1 $\frac{1}{4}$.	1 $\frac{1}{2}$.	1 $\frac{3}{4}$.	2.	2 $\frac{1}{4}$.	2 $\frac{1}{2}$.	2 $\frac{3}{4}$.	3.	3 $\frac{1}{4}$.	3 $\frac{1}{2}$.	3 $\frac{3}{4}$.	4.	Final.
h	400	440	475	500	520	535	546	556	565	571	575	580	583	593
p	373	333	298	273	253	238	227	217	208	202	198	193	190	180
$p-p_0$...	193	153	118	93	73	58	47	37	28	22	18	13	10	...

* This result illustrates a point frequently observed in the course of these experiments. If the charcoal is left exposed to the atmosphere it rapidly deteriorates in its capacity for absorbing air, probably through the absorption of moisture. In an experiment made eight weeks previously over 100 c.c. of air were collected from the same bulb. The power of absorption may be partially restored by heating the charcoal, and driving off the condensed moisture, but it appears never to regain its original value. Charcoal bulbs intended for the exhaustion of vacuum tubes should always be kept sealed when not in use.

The readings in the first row (t) give the time in minutes from the first application of the liquid air, those in the second row (h) the height in millimetres of the mercury in the gauge. The values in the third row (p) are obtained by subtracting the figures in the second row from 773, the atmospheric pressure in millimetres of mercury. In the last row are the values of $p - p_0$, the excess of the pressure at any instant over the final pressure.

These last figures were plotted as ordinates on semi-logarithmic paper, with the corresponding values of the time as abscissæ (fig. 2, p. 504). It was then found that the points lay on a straight line, showing that the pressure was connected with the time by the relation

$$\log(p - p_0) = A - \lambda t, \quad . \quad . \quad . \quad . \quad : \quad (\text{iii.})$$

where λ is a constant whose value in this experiment is 0.428.

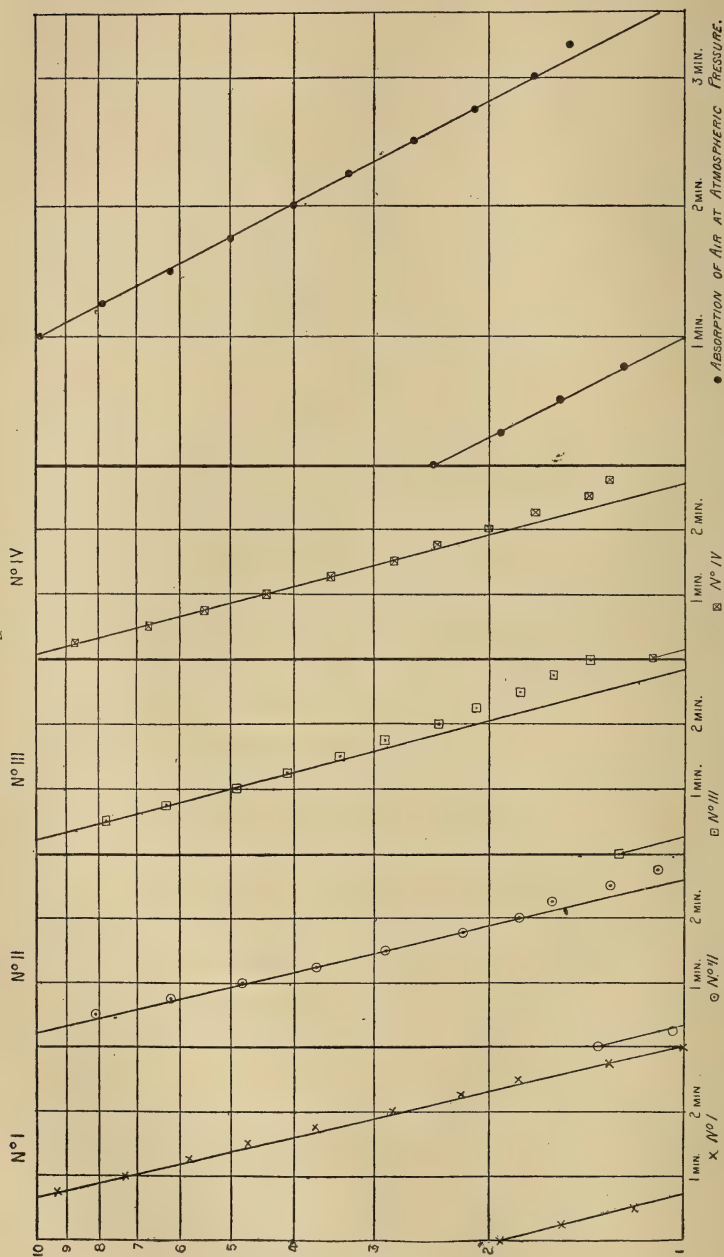
Similar results were obtained in the experiments numbered 2, 3, & 4. It is not necessary to give in full the readings in these cases, as they are represented in the diagrams (fig. 2), and the results in all four cases are summarized in Table III. The values of λ were found from the inclination to the axes of the straight lines in the diagrams. It will be noticed that the value of λ is approximately the same in each experiment, the mean value being 0.406.

TABLE III.

	Initial Volume.	Reading after 1 min.		Reading after 4 min.		Final Reading.		A.	λ
		$h.$	$p-p_0.$	$h.$	$p-p_0.$	$h.$	$p_0.$		
I. ...	33.5 c.c. at 773 mm. }	400	193	583	10	593	180	2.714	.428
II. ...	70.8 c.c. at 772 mm. }	250	136	377	9	386	386	2.555	.421
III. ...	93.4 c.c. at 772 mm. }	218	126	330	14	344	428	2.480	.380
IV. ...	125 c.c. at 772 mm. }	165	112	265	12	277	495	2.443	.394

h = height of mercury column in mm.
 p = pressure in apparatus in mm.

Fig. 2.



Rate of Absorption of Air by Charcoal.

§12. In all the experiments which have been described, the relation between the pressure in the enclosed space and the time since the commencement of the absorption is given by the formula

$$\log (p-p_0)=A-\lambda t.$$

By differentiation

$$\frac{dp}{dt} = -\lambda(p-p_0). \quad \dots \dots \dots \text{(iv.)}$$

But we have already seen that the volume of air absorbed by the charcoal (which we may denote by x)

$$=V'\left(1-\frac{p}{p'}\right),$$

where V' is the total volume of the exhausted space.

So that the rate at which air is absorbed by the charcoal is

$$\begin{aligned} \frac{dx}{dt} &= -\frac{V'}{p'} \frac{dp}{dt} \\ &= \frac{V'\lambda}{p'} (p-p_0) \\ &= \lambda(\xi-x), \quad \dots \dots \dots \text{(v.)} \end{aligned}$$

where ξ is the amount of air absorbed when equilibrium is established.

§ 13. An experiment was also made with the same sample of charcoal used in Experiments Nos. 1-5, in order to determine the rate of absorption of air at constant (atmospheric) pressure. For this purpose the bulb containing the charcoal was connected through a T-piece with the stopcock of a burette, inverted over water. During the preliminary cooling of the bulb, which lasted three-quarters of a minute, the third limb of the T-piece was left open to the external air. At the end of this time the aperture was closed and the charcoal commenced to absorb the air in the burette. Readings of the volume of air absorbed were taken every 15 seconds, the pressure exerted by the column of water in the burette being neglected. The observed volumes were subtracted from the final reading obtained when the absorption was complete, and the resulting values (representing the volume of air still to be absorbed before equilibrium is established) were plotted on semi-logarithmic paper with the corresponding times as abscissæ. The points were found to fall on a straight line, showing that $\log (\xi-x)$ is connected with t by a linear equation. On differentiation we obtain as before

$$\frac{dx}{dt} = \lambda(\xi-x).$$

The value of λ in this case is 0.380.

TABLE IV.

t (mins.) ...	1	$1\frac{1}{4}$	$1\frac{1}{2}$	$1\frac{3}{4}$	2	$2\frac{1}{4}$	$2\frac{1}{2}$
$\xi - x$ (c.c.) ...	23.5	19.2	15.5	12.4	9.9	7.9	6.2
t	$2\frac{3}{4}$	3	$3\frac{1}{4}$	$3\frac{1}{2}$	$3\frac{3}{4}$	4	$4\frac{1}{4}$
$\xi - x$	5.0	4.0	3.3	2.6	2.1	1.7	1.5

The results are plotted in fig. 2.

§ 14. This value for λ is very nearly the same as the values found in the previous experiments in which the pressure altered as the absorption proceeded. The constancy in the value of λ leads to the important conclusion that, within the range of the observations, the rate of absorption is practically independent of the pressure.

The rate of absorption will be different for different samples of charcoal, and there is a wide field awaiting investigation in the determination of this constant for various kinds of charcoal, and what is more important the determination of the rate of absorption of pure gases by a particular sample of charcoal.

(ii.) *Absorption of a Limited Volume of Air by a large quantity of Charcoal.*

§ 15. In order to trace the course of the absorption at low pressures, a charcoal bulb was sealed to a McLeod gauge and barometer-tube. The capacity of the McLeod gauge was 232.4 c.c., the capillary portion being 10 cm. long and having a capacity 0.125 c.c. The internal volume of the barometer-tube and the connecting tubes was estimated at 142 c.c.

The charcoal bulb first used contained about 65 grams of charcoal, its volume being 200 c.c.

The vessel containing liquid air was brought up underneath the charcoal bulb and gradually raised until the whole was immersed. This process generally occupied about two minutes. At the end of this time, readings of the height of the mercury column were commenced and noted at every half minute, until the motion became too slow to give trustworthy results. In ten minutes the difference between the height of this column and that of the barometer was less than one millimetre.

No readings of the McLeod gauge could be obtained until about half an hour after the commencement of the experiment.

As soon as the mercury could be got to enter the capillary of this gauge (corresponding to a pressure of about .08 mm.) readings were taken every five minutes. The process of filling the gauge with mercury, taking the reading, and emptying it again occupied almost exactly a minute. During this interval of time, the air contained in the gauge was cut off from communication with the charcoal.

§ 16. The results obtained with this absorber are recorded in Table V.

TABLE V.

Charcoal Bulb containing 65 gms. of Charcoal.

		Initial Pressure.	Final Pressure.	Values of λ .	
				Barometer.	McLeod.
April 8th...		763—153 mm. =610 mm.	·0109	·258	·0468
April 10th...	Charcoal bulb had been strongly heated.	757—380 mm. =377 mm.	·0163	·363	·0476
April 12th...	P ₂ O ₅ drying-tube inserted. Charcoal bulb again heated.	766—403 mm. =363 mm.	·0214	·315	·0467

The figures in the first two columns show the pressure in the apparatus at the commencement and at the conclusion of the experiment. It is noteworthy that the final pressure attained is greater each time the charcoal is used, illustrating the deterioration that takes place when the charcoal is exposed to a damp atmosphere even though the exposure is of short duration.

§ 17. The pressures in the apparatus, as determined by the height of the mercury in the barometer-tube, were plotted on semi-logarithmic paper with the times as abscissæ, and the resulting curve was found to approximate very closely to a straight line. The pressure at any instant may accordingly be represented by the formula

$$\log p = A - \lambda t,$$

which is of the same form as that already found (iii.), p_0 being now so small that it may be neglected in comparison with p . The values of λ from these experiments are given in the Table in the last column but one.

The pressures recorded by the McLeod gauge were treated in a similar manner, but in this case p_0 can no longer be

neglected. Consequently the relation between the pressure and the time must be expressed by the more general equation (iii.). This was found to agree well with the experimental results. The values of λ are given in the last column of the Table.

§ 18. It will be observed that the rate of absorption (as measured by λ) is about eight times as large in the earlier portion of the experiment as it is in the later part, when the value is deduced from the readings of the McLeod gauge. If we compare the pressures under which the absorption is taking place, we find an immensely greater difference. In the first part of the experiment, the pressure is of the order of 200 mm., in the second part it averages only .04 mm., so that while the ratio of the pressures is of the order of 5000 to 1, the ratio of the corresponding values of λ is only about 8 to 1. This result emphasizes the conclusion arrived at from the experiments formerly described, that the rate at which absorption of gas takes place is influenced to only a very slight extent by the pressure of the gas.

§ 19. It must be remembered that in the experiments described we are dealing with a mixture of gases, so that the results are more complicated than they would be if we had to do with a pure gas. Dewar* has shown that one cubic centimetre of coconut charcoal can absorb 155 c.c. of nitrogen, or 230 c.c. of oxygen at a temperature of -185° C. Consequently, in the present set of experiments the percentage of nitrogen in the unabsorbed gas will increase as the absorption proceeds, so that the velocity coefficient deduced will not be constant. Both the alteration in the composition of the gaseous mixture and the effect of diminution of pressure on the velocity, will tend to give a smaller coefficient of velocity as the experiment advances.

§ 20. Three separate experiments were carried out with the large charcoal receptacle already referred to (§ 5) sealed to the McLeod gauge. By means of a Fleuss pump the whole apparatus was exhausted until the mercury stood at 740 mm., the barometric height being 780 mm. After the charcoal had been cooled to the temperature of liquid air, the pressure in the apparatus fell for over three hours; the final pressure registered being .0009 mm. The readings of the McLeod gauge obtained in experiments on three consecutive days showed remarkable agreement, and it has been thought worth while to tabulate these not only to illustrate the regularity of the absorption, but also to strengthen the confidence that may be placed in the readings of the gauge (Table VI.).

* Proc. Roy. Soc. lxxiv. p. 124 (1904).

TABLE VI.
Experiment with McLeod Gauge.
 $p_0 = \cdot 0009$ mm.

t .	Values of $p - p_0$.			
mins.	April 18th.	April 19th.	April 20th.	Mean.
0	·0271	·0295	·0271	·0279
5	·0192	·0185	·0179	·0185
10	·0131	·0131	·0121	·0128
15	·0100	·0086	·0081	·0039
20	·0069	·0061	·0061	·0064
25	·0053	·0046	·0046	·0048
30	·0040	·0033	·0040	·0038
35	·0031	·0026	·0027	·0028
40	·0028	·0022	(·0022)	·0024
45	·0025	·0019	·0018	·0020
50	...	·0016	·0016	·0016

§ 21. The mean values of $p - p_0$ have been plotted as ordinates, with the corresponding values of the time as abscissæ in fig. 3, p. 510. It will be seen that the points so obtained fall approximately on a straight line, so that in this case also the relation between the pressure and the time is of the form

$$\log (p - p_0) = A - \lambda t.$$

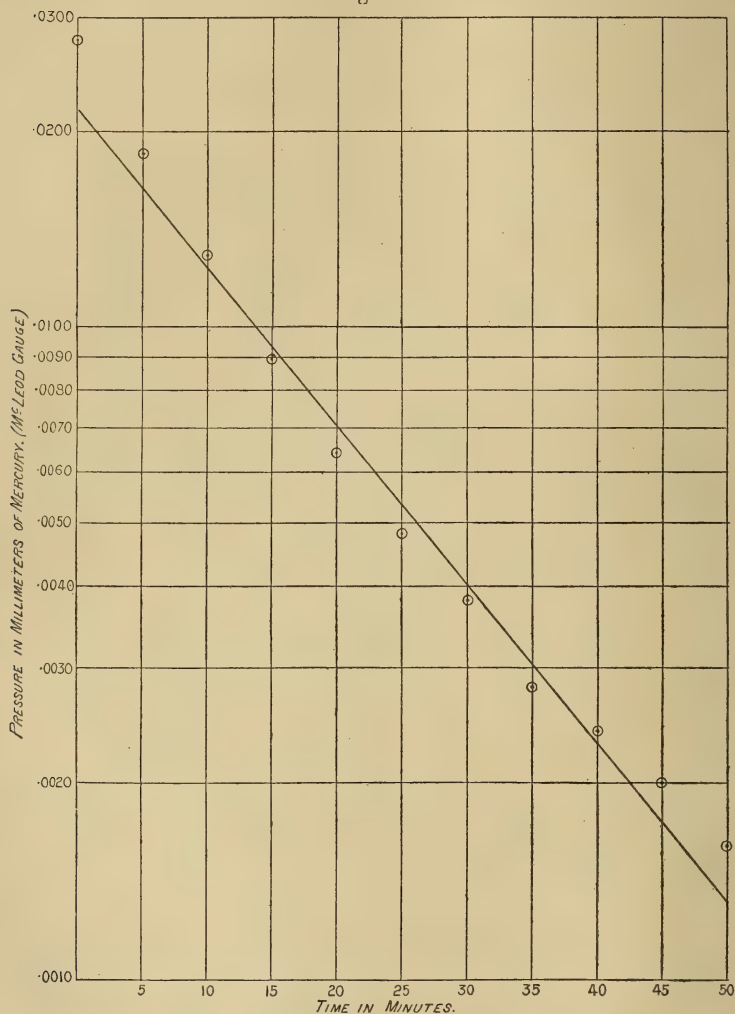
The value of λ in the equation is $\cdot 0245$.

TABLE VII.
Charcoal Bulb containing 216 gms. of Charcoal.

		Initial Pressure.	Final Pressure.	Values of λ .	
				Baro- meter.	McLeod.
April 17th.	New bulb attached, strongly heated and sealed.				
April 18th.	Morning.....	780—603 mm. =177 mm.	·0095	·313	} Mean of three ·0245
	Afternoon. Exhaust appa- ratus with Fleuss pump.	780—740 mm. =40 mm.			
April 19th.		780—768 mm. =12 mm.	·0009	...	
April 20th.		777—763 mm. =14 mm.	·0009	...	
April 21st.	Let in air to atmospheric pressure.	775— 0 mm. =775 mm.	·0093	·220	} Mean of two ·0216
April 22nd	Mercury stands at	783—136 mm. =647 mm.		...	
May 8th.	Apparatus left sealed since April 22nd.	785—155 mm. =630 mm.	·0097	·206	

§ 22. Two experiments were made with the same absorber starting from a pressure that was nearly atmospheric. The results of these and the former experiments are summarized

Fig. 3.

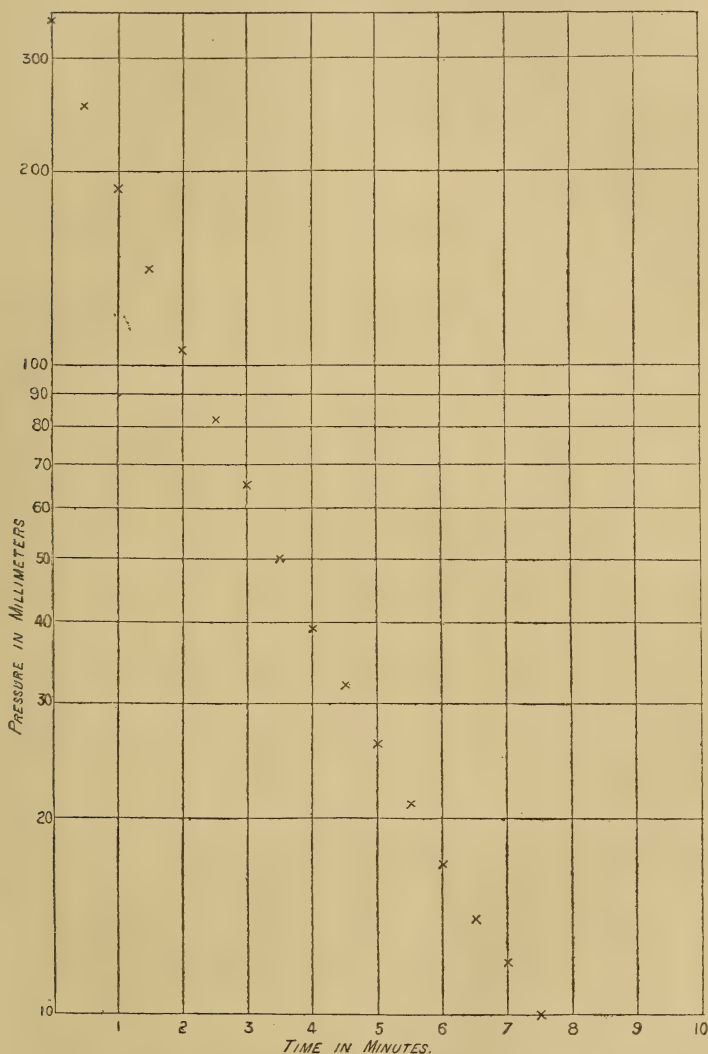


*Rate of Absorption of Air by Charcoal. Experiment with
McLeod Gauge.*

in Table VII. and in figs. 3 and 4; and it is unnecessary to enlarge upon them. We need only note that the rate of absorption is practically constant, and that the final pressure

attained is ten times as great as that reached in the experiments in which the apparatus was first of all exhausted by

Fig. 4.



Rate of Absorption of Air by Charcoal (April 21st).

means of the Fleuss pump. The latter result has an important practical application, for it shows that if we desire to obtain the best possible vacuum in any vessel, the vessel

with its attached charcoal absorber should be exhausted to as high a degree as possible before the charcoal is cooled to the temperature of liquid air.

§ 23. Reviewing the results so far obtained, we see that they may all be included in a relation of the form given in equation (v.). This expresses the fact that the rate of absorption is proportional to the difference between the total amount of air absorbed and the amount which has been absorbed at the instant in question. In other words, the rate of absorption stands in a constant ratio to the quantity of air that will still be taken up by the charcoal. The constant is but little affected by alterations in the pressure under which the absorption takes place.

The simplest explanation of the result appears to be to consider each element of the charcoal (probably a superficial rather than a volume element) capable of taking up a definite amount of gas. When the element has received this quantity it becomes useless so far as further absorption is concerned. On this hypothesis it follows that a given sample of charcoal can absorb a definite amount of gas, the amount being independent (or almost independent) of the pressure. Moreover, the rate at which the absorption is proceeding at any instant will be proportional to the number of elements which have not yet taken up their quantum of gas; that is to say, to the quantity of gas that the charcoal is still capable of absorbing.

This hypothesis is sufficient to account for the facts observed in the simpler cases, but it may require some modification when we are dealing with a limited quantity of air. For we know that in this case the whole of the air is not absorbed by the charcoal, but the pressure tends to a definite limiting value*.

It is to be expected that further light would be thrown on the value of this limiting pressure, by a study of the absorption of pure gases by charcoal. In the case of air, the residual pressure is due to some considerable extent to the less easily absorbed gases, hydrogen, neon, and helium. Consequently, if an extremely high vacuum is required, it is advisable to remove as large a proportion of these gases as possible by a preliminary exhaustion.

Blythswood Laboratory, Renfrew.

* In the experiment made on April 12th (Table V.) the final pressure recorded was not altered by keeping the charcoal bulb in liquid air for a further period of three and a half hours.

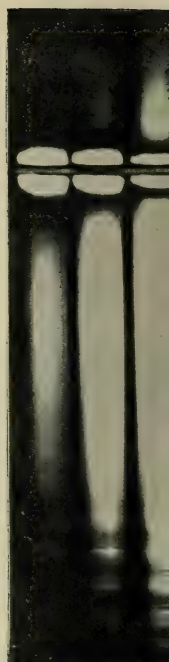
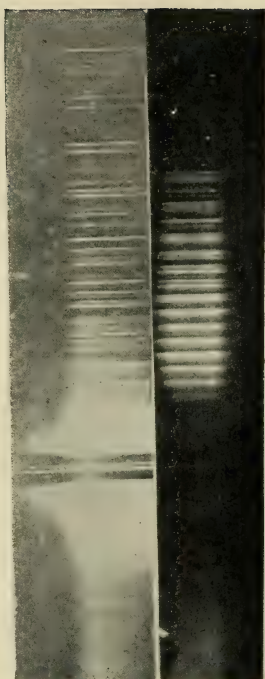


Fig. 3.



Abs.

Mag.

Rot.

Fig. 4.
 D_1D_2



Red.

Green.

Blue.

Fig.

2964

1005

705

6105



Fig.

D_2D_1

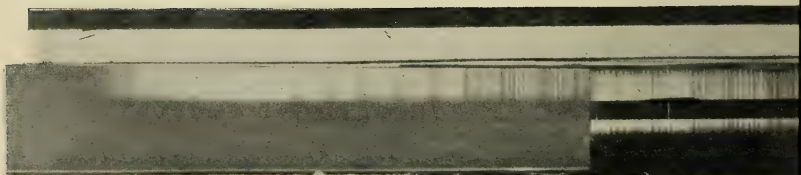


FIG. 1.

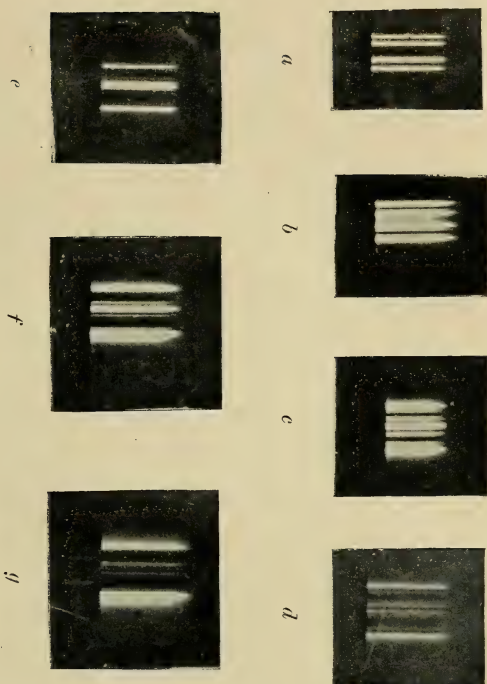
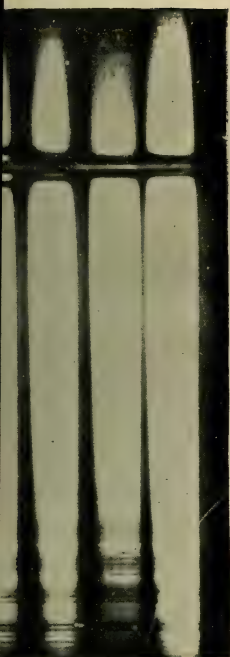


FIG. 2.



Absorption
Mag. Rotation.



Mag. Rotation.
Absorption.

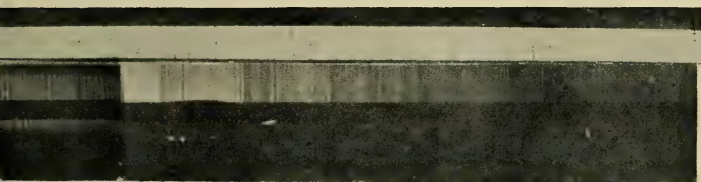
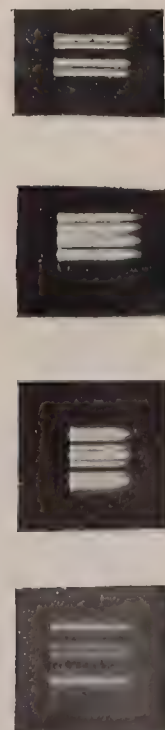


FIG. 1.



a

b

c

d

e

f

g



FIG. 2.



FIG. 3.

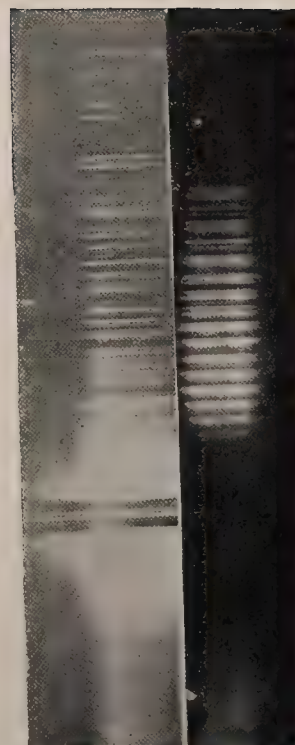


FIG. 4.



Red.

Green.

Blue.

FIG. 5.

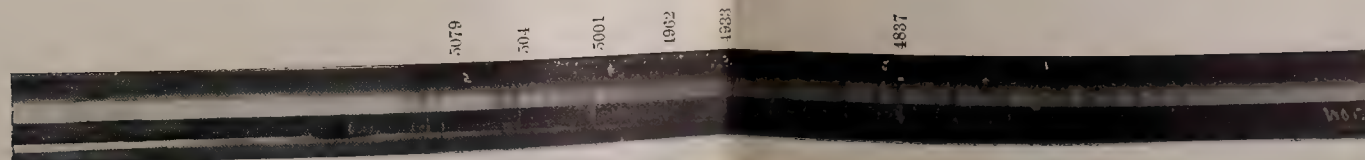
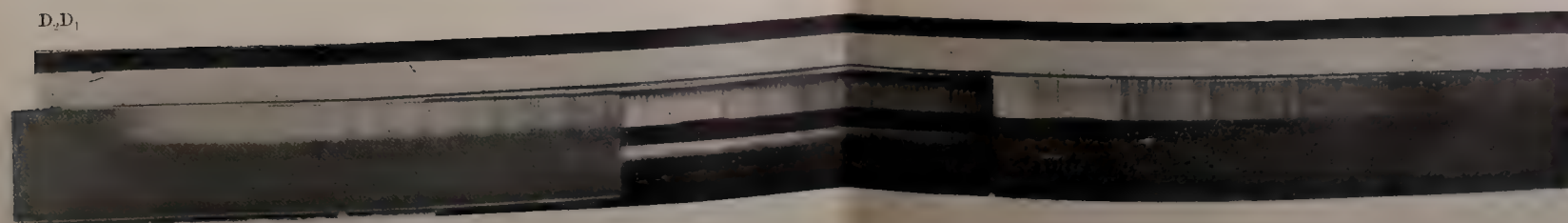


FIG. 6.



Mag. Rotation.
Absorption.

FIG. 1.

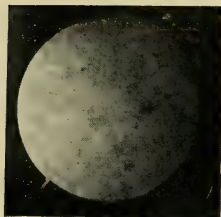


FIG. 2.

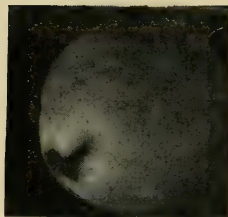


FIG. 3.

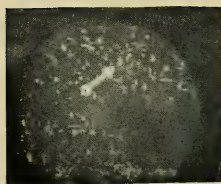


FIG. 4.



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PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[SIXTH SERIES.]

NOVEMBER 1905.

LVIII. *The Fluorescence of Sodium Vapour and the Resonance Radiation of Electrons.* By R. W. WOOD, Professor of Experimental Physics, Johns Hopkins University*.

[Plate VII.]

IN the modern theories of absorption we find frequent use made of the conception of a re-emission of radiant energy by an electron which is vibrating in unison with the incident light-waves. The emitted energy should, however, be of the same wave-length as that of the exciting waves, and while we have plenty of examples of photoluminescence, it seems probable that in these cases the phenomenon is extremely complicated, for the emitted radiation consists of a heterogeneous mass of waves, usually of slower period than that of the incident light. Repeated efforts have been made by various investigators to detect a lateral emission of yellow light by sodium vapour when in the act of absorbing sodium light. These efforts have been unsuccessful for reasons which will be presently given. The phenomenon has at last been observed: a dense mass of non-luminous sodium vapour radiating a brilliant yellow light when illuminated by the light from a very intense sodium flame. The same phenomenon has been observed in the region of the channelled absorption; in this case, however, radiations of other wave-lengths are emitted, as well as those of the same period as that of the exciting light. This seems to be the first case found of the phenomenon, which it may perhaps be well to

* Communicated by the Physical Society: read June 30, 1905.

style *resonance radiation*, to distinguish it from fluorescence. The intimate connexion between fluorescence and phosphorescence, and the almost indisputable evidence that the latter phenomenon is associated with chemical changes produced in the substance, makes it appear probable that fluorescence and resonance radiation are two entirely different phenomena, though the former is doubtless caused in some roundabout way by resonance.

In the present paper, however, I shall, for convenience, use the term fluorescence, though we may eventually have to distinguish between fluorescence spectra and resonance spectra.

The very remarkable fact has been ascertained that the stimulation of the vapour with approximately monochromatic light, furnished by the monochromatic illuminator (spectroscope), results in the emission of light, the spectrum of which consists of a number of more or less regularly spaced bright lines of different intensities. The slightest change in the wave-length of the exciting light results in the disappearance of these lines, and the appearance of another set; the phenomenon indicating that the molecule contains a number of groups of electrons, the excitation of any one of which sets the entire group in vibration, without however disturbing the other groups. As the wave-length of the exciting light is slowly changed, the fluorescent spectrum presents a most remarkable appearance. The lines appear in rapid motion, the luminous bands moving in a rippling manner, like moonlight on water. The motion is of course an illusion, due to the continual disappearance and reappearance of the bright lines, the phenomenon reminding one forcibly of the scintillations produced on a screen of zinc sulphide by the electron bombardment produced by radium.

In the earlier work on the fluorescence of the vapour, carried on by the author in collaboration with Dr. Moore, the metal was vaporized in a tube of seamless steel. Owing to the distillation of the metal to the cooler parts of the tube, it was necessary to open the tube and introduce a fresh charge after two hours' heating. This was a great gain on the exhausted glass bulbs used by Wiedemann and Schmidt, which blackened, and became useless in half a minute or less, but something better was obviously necessary for the large number of long exposures necessary for a careful study of the remarkable changes in the fluorescence spectrum, which accompanied changes in the wave-length of the exciting light. For purposes of demonstration a plain tube of steel, or even brass, is all that is required. Steel is preferable, for

it is probable that the sodium would eventually eat its way through brass. The latter metal can, however, be used if a large steel tube cannot be procured. The tube should be 2 or 3 inches in diameter and about 2 feet long. A small piece of brass tubing is brazed into one end, through which the tube can be exhausted to a pressure of a few millimetres, for it is only at low pressures that a fluorescence of any brilliancy can be obtained. A piece of clean sodium, obtained by melting the metal in a crucible and pouring it out on a plate, is introduced into the tube, and the ends closed with plate glass, cemented on with sealing-wax. One window should be attached before the introduction of the sodium. The following method gives the best results:—Heat the end of the tube and apply the melted sealing-wax smoothly to the rim, building it up to a depth of about 2 mms. Stand the tube on end, heat the glass plate, wave the flame over the wax until the surface melts, and then immediately apply the plate. The wax must be in optical contact with the metal and the glass, otherwise the joint is sure to leak. It is a good plan to go around the joint with a very minute pointed gas-flame, heating the wax until it runs freely over the surfaces. A mercurial or mechanical pump can be used for the exhaustion. If the pump leaks ever so slightly, a glass stopcock should be put between it and the tube, and all the joints made of glass and sealing-wax. With a little practice the whole apparatus can be set up in fifteen minutes; and the tube, once prepared, can be put away for future demonstrations, care being taken to close it hermetically with wax.

Sun or arc-light is focussed in a slightly oblique direction, by means of a lens, directly over the lump of sodium, which should be brought to the centre of the tube. On heating the tube, the brilliant spot of green fluorescent light can be observed through the opposite end, care being taken to keep the eye out of the emergent beam of white light. Tubes of this description, while perfectly satisfactory for demonstration, are unsuitable for long continued spectroscopic study, and various devices were accordingly tried.

The apparatus in its final form consisted of a seamless tube of thin steel 3 inches in diameter and 30 inches long, with a steel retort at its centre in which a large amount of sodium could be stored. The retort was made by fitting two circular disks of steel to a short piece of tubing, just large enough to slip snugly into the larger tube. The circular ends of the retort were provided with oval apertures as shown in fig. 1 (p. 517). The retort was half filled with sodium, the molten

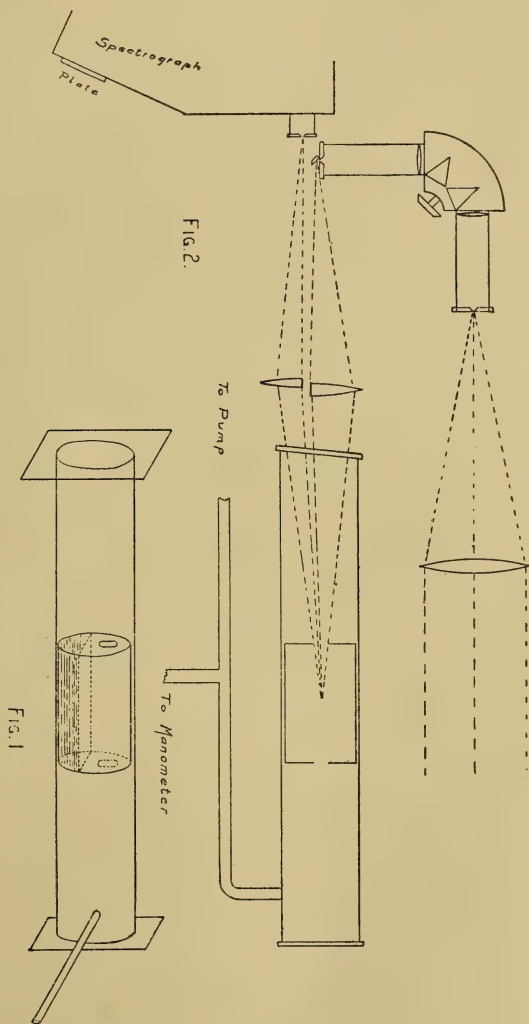
metal being poured in through one of the apertures. It was then introduced into the tube and pushed down to the centre, after which the plate-glass ends were cemented on as shown in the figure. This arrangement prevented the rapid diffusion of the vapour, and enabled a large supply of metal to be kept at the centre of the tube. The tubes used in the earlier work required re-charging after two hours' continuous operation, while the retort-tube could be operated for several hundred hours on a single charge.

The illuminating beam was focussed just inside one of the oval apertures in the retort, being received by the opposite wall just to one side of the other aperture. The end of the large tube was covered with a black cloth, by which device it was possible to view the fluorescent spot against the dead-black background of the second aperture.

A large three-prism spectrograph was constructed for photographing the spectra. The prisms were of clear dense flint 4 inches in height, and the focal length of the lenses 36 inches.

The monochromatic illuminator, which furnished the approximately homogeneous light by which the fluorescence was provoked, was arranged as shown in fig. 2. A small strip of silvered glass 2 mm. wide was mounted in front of the second slit of the instrument, by which the emergent beam was thrown into the sodium tube. By means of a split lens the light was focussed upon the aperture, and an image of the fluorescent spot thrown upon the slit of the spectrograph. It was found necessary to split the lens and separate the two halves a trifle, as otherwise the image of the fluorescent spot was thrown back upon the small mirror. The arrangement figured was found to be the only one capable of yielding an image of the fluorescent spot upon an absolutely black background. It was extremely important to accomplish this, as otherwise it would have been impossible to ascertain whether or not the fluorescent light contained the same wave-lengths as the exciting light. For the same reason it was important to get rid of all traces of floating particles of oxide. These gave a good deal of trouble in the earlier work, but in the new tube it was found that after a half-hour's operation all trace of them disappeared. To prove the purity of the atmosphere in the tube and the blackness of the background, it was only necessary to adjust the prisms of the monochromatic illuminator for some wave-length incapable of exciting fluorescence. The brightest part of the spectrum in the yellow region, not far below the sodium lines, answered the purpose admirably.

By means of two right-angled prisms it was possible to substitute a powerful beam of sunlight for the monochromatic beam, by which the spectrum of the fluorescence provoked by white light could be easily photographed or observed.



With white light illumination the fluorescence spectrum is made up of a large number of hazy lines, which in the yellow and yellow-green region are arranged in groups or

bands, which lie close together in the vicinity of the D lines, widening, however, as the blue region is approached. Coincident with the D lines there appears a hazy band, the surrounding region being nearly devoid of light, which, if the vapour is not too dense, can be resolved into a double line, the components of which coincide with D_1 and D_2 . These lines only appear when the vapour is stimulated with light of the wave-length of the sodium lines, *i. e.* the fluorescent light can be regarded as an emission of light by the electrons, in virtue of the vibrations excited by the incident light. This was proven in two ways: first, by illuminating the vapour with the light of a very intense sodium flame, which provoked a bright yellow fluorescence; secondly, by illuminating it with light from a spectroscope, and varying the wave-length continuously. The yellow band only appeared when the spectroscope furnished light of the wave-lengths of the D lines. Further work, it is hoped, will show whether the D_1 vibration is independent of that giving rise to D_2 . To solve this interesting question, it will be necessary to illuminate the vapour with the light of D_2 only, and ascertain whether both lines are present or not in the fluorescent spectrum, a difficult observation, but one which can doubtless be made. There is in addition a red fluorescence, the spectrum of which Wiedemann and Schmidt believed to be devoid of bands or lines. This, however, is not the case, for the spectrum in the red was found to be crossed by a large number of dark bands. They are much more difficult to observe than the green bands, and are only very conspicuous when the density of the vapour is considerable, which accounts for their having been overlooked previously. The general appearance of the fluorescence spectrum stimulated by white light is shown in Pl. VII. fig. 1. In this photograph the red region and the double band at the D lines are not shown.

In the blue and green-blue region the lines and bands of the fluorescent spectrum are irregularly spaced. From $\lambda=5050$ to $\lambda=5071$ we have a regularly spaced system of lines, which get closer and closer together as we approach the less refrangible end of the spectrum. These lines collectively form a system of bands, the spectrum having a fluted appearance. The spacing between the bands also becomes less as we approach the more refrangible end. With white-light illumination the bands cannot be distinguished in the yellow region, but by exciting the fluorescence with blue-violet light, the bands can be traced up to the very end of the spectrum ($\lambda=571$). The bands change their position slightly as the wave-length of the exciting light is altered, but the lines of which they are composed remain

fixed in position. What actually happens appears to be a redistribution of intensity among the individual lines.

The yellow-green-blue fluorescence spectrum can be divided roughly into three parts, which we will designate A, B, and C. Region A, comprised between wave-lengths 571 and 500, is made up of regularly spaced lines which collectively form a system of bands, owing to the periodic fluctuations of intensity. Region B, between 500 and 477, is made up of irregularly spaced lines, often hazy on one side, and of varying width. This region contains the first three bands of the group already alluded to, though special precautions are necessary to bring them into view. With dense vapour, and illuminating light of the proper wave-length (about 460), the region is seen to break up into three very broad patches; in other words, there is a periodic fluctuation of intensity as in region A. Region C between wave-lengths 477 and 468 is made up of very broad lines, spaced with a fair amount of regularity and arranged for the most part in pairs.

These three regions are indicated by brackets in fig. 1, Pl. VII.

The positions of the bands, which apparently have their origin in the periodic fluctuations of intensity of the lines, are recorded in the following table:—

4791	5415
4882	5465
4971	5510
5050	5551
5120	5585
5185	5620
5246	5645
5316	5665
5361	

The centre of the bands cannot, of course, be very exactly located, especially in the case of the first three which are found in region B.

These bands apparently obey the same law as the oxygen bands in the solar spectrum, *i. e.* if we plot their wave-lengths as abscissæ, and take ordinates which increase by equal amounts, we obtain a curve which is approximately a parabola. In this way we can calculate the position of the head of the group, which was found to fall at wave-length 5715. Two bands were therefore missing, one of which at 5676 was subsequently found in the spectrum obtained with blue-violet excitation, dense vapour, and long exposure: still longer exposures would probably record a trace of the first band at 571.

As has been said, these bands shift their position as the wave-length of the exciting light changes. The shifts are

greatest in the region between $\lambda=515$ and $\lambda=545$, where it may amount to the full width of the band or 25 A.E. The bands in the vicinity of $\lambda=555$ remain fixed in position, as is probably true of all the less refrangible ones. It was at first thought that the head of the group remained fixed, the group opening out, or expanding so to speak, but this appears not to be the case; and the law governing the motion of these bands, if one exists, cannot be determined until a larger number of photographs have been obtained. The probable cause of the motion will appear presently. The wave-lengths of the lines which collectively make up the fluted bands were determined from the calibration curve of the spectrograph, and are recorded in the following table. It was at first thought that they would obey the law of constant second differences, but such was not the case. It has not yet been possible to measure the lines above $\lambda=555$, though the bands extend to 571.

Wave-lengths of bright lines in Fluorescence Spectrum of Na Vapour.

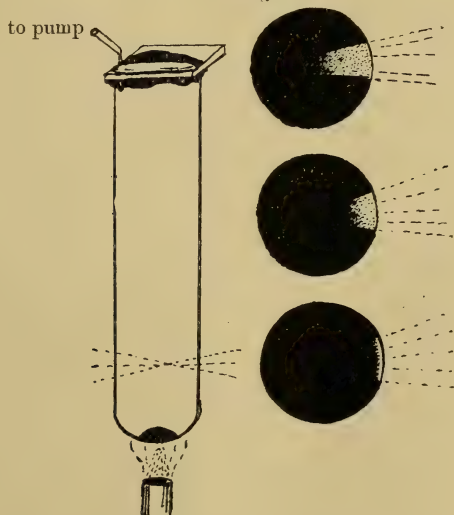
5553	5315	5040
48	09	34
43.5	02	25
39	5296	5000
35	89	4995
31	83	88
14	76	80
10	70	64
05	64	59
5499	58	34
95	52	25
91	46	12
86	39	04
81	32	4895
77	26	85
62	19	75
58	13	67
53	06	60
47	Faint 52	50
42.5	5194	39
37	88	21
31	80	12
26	73	03
21	66	4795
5399	Faint 59	85
93	49	78
87	41	69
81	34	59
76	27	53
70	20	43
64	13	39
59	03	33
Faint 51	5096	28
44	88	16
38	80	04
32	72	4695
27	49	84
21		

Stimulation with Monochromatic Light.

Having described briefly the general appearance of the spectrum when the fluorescence is provoked with white light, we will now consider the case of monochromatic stimulation. In all of the earlier work with Mr. Moore, and during the first stages of the present investigation, no trace of any emission-band at the D lines was observed, neither was any fluorescence whatever observed when the prisms of the monochromatic illuminator were adjusted so that the instrument delivered light of wave-length approximately that of the D lines. A bright band at the D lines had, however, been observed by Wiedemann and Schmidt, and independently by the author in the case of sodium vapour formed in exhausted glass bulbs, which observations have been confirmed by Pucianti, who has recently published an interesting paper on the subject*.

It seemed probable that in the case of a large mass of vapour in a steel tube, the radiations capable of exciting the D line fluorescence were absorbed in the outlying mass of comparatively rare vapour before coming to a focus in the dense mass. As it was found difficult to observe the phenomenon in glass bulbs on account of the formation of a sodium dew on the walls, and the speedy discoloration of the glass,

Fig. 3.



the arrangement shown in fig. 3 was adopted. A small piece of clean sodium was placed in a large test-tube, hermetically

* *Accad. Lincei, Atti*, xiii. pp. 430-440 (1904).

sealed with a glass plate, and furnished with a tube for exhaustion. Since a much more powerful D line radiation can be obtained from a bright sodium flame than from a selected portion of the spectrum, the monochromatic illuminator was abandoned, and a small oxy-hydrogen flame, heavily charged with sodium, substituted for it. An image of this flame was formed by means of a lens, at the centre of the tube immediately above the lump of metal. On gently heating the tube and viewing the interior through the cover-glass, a faint cone of yellow light was observed which, however, only extended to the centre of the tube, owing to the inability of the effective radiations to penetrate to a greater depth. This stage is shown in the upper diagram of fig. 3. As the temperature rose and the density of the vapour increased the fluorescence increased in brilliancy, the region retreating towards the wall, until only a bright skin of yellow light remained, which lined the wall of the tube at the point where the exciting radiations entered, as shown in the lower diagram. This experiment proved conclusively that sodium vapour is capable of re-emitting a yellow light, when in the act of absorbing the light of the soda flame. The appearance of the yellow cone and its behaviour as the vapour-density increased proved that it was a true re-emission of absorbed light, and not a scattering of light by floating particles of oxide.

An attempt was next made to observe the same phenomenon in the steel retort-tube. On focussing an image of the flame upon the window of the retort a distinct spot of yellow light was observed, though it proved too faint to admit of satisfactory observation in the large spectrograph. Returning once more to the monochromatic illuminator, I was surprised to find no difficulty at all in obtaining a bright emission-band at the D lines, which proved to be double when the vapour-density was not too great. The band was of considerable brilliancy when direct sunlight was used. The failure to observe this band in the previous experiments may have been due to an insufficiently high vacuum, or to insufficient density of the sodium vapour. It has been found that long-continued work results in a steady improvement of the spectrum as to brilliancy, though it is quite impossible to ascertain the precise reason. It is probable that the proper conditions are unconsciously found, and the detrimental ones discarded, by what Prof. James would call the "Sub-conscious-self." The band at the D lines only appeared when the light from a corresponding part of the spectrum fell upon the vapour, showing that the electron system which produces these lines is independent of the

system which gives rise to the channelled spectra. Whether D_1 and D_2 are connected has not yet been determined. To do this it will be necessary to free the illuminating beam from the D_1 radiations, and observe whether or not the fluorescent band is double. An attempt will be made to accomplish this.

We will now consider more in detail the appearance of the fluorescence spectrum, when the illuminating light consists of a more or less monochromatic beam from the illuminator. As has been said, the yellow end of the spectrum appears only when the stimulating light is blue-violet. It is probable that the yellow light will also be radiated when the stimulating light is of the same wave-length, though such a radiation has not yet been detected. The absorption for radiation in this region ($\lambda=571$ to 560) is very small, and the vapour appeared practically black in all cases thus far observed. The region of maximum fluorescence moves down the spectrum as the stimulated region moves up, as is well shown in the series of photographs in fig. 3 (Pl. VII.). In this series of photographs the stimulated region is indicated by the luminous region to the left. The lowest one, *e*, shows very clearly that when the stimulation is at the violet end only the first five bands at the yellow end appear. In spectrum "*a*" the stimulation is at the centre. The change in the position of the groups or fluted bands is best shown in figs. 4 and 5. The same thing can be seen in fig. 3. by moving a vertical straight edge along the series. That the spectra are in perfect "register" can be seen by noting the position of the D lines at the right. These were recorded by holding a soda flame in front of the slit for a few seconds during each exposure. Radiations both above and below the excited region are present, showing that Stokes's law is not obeyed. This is especially noticeable in fig. 2 (Pl. VII.), where the excited region is indicated by a white rectangle. The plate-holder of the spectrograph was arranged so that it could be moved up and down, and as many as seven spectra could be photographed on a single plate if necessary. After recording a fluorescent spectrum, the plate was elevated and the beam from the monochromatic illuminator thrown back into the instrument by means of a mirror. In this way it was possible to obtain a record of the extent of the region of fluorescence *below* the excited region. It was thought desirable to obtain a more homogeneous type of exciting light than that furnished by the monochromatic illuminator, and a mercury arc in a quartz tube was accordingly tried. Though the light was extremely intense, and though its radiation contained a number of lines in the region desired, it was found impossible to obtain the slightest trace of

fluorescence. No one of the periods happened to be quite right; and the electrons remained quiet. The light of the cadmium spark, though much less intense, excited the fluorescence; but it has not yet been ascertained to what degree the "continuous background" of its spectrum was responsible for the phenomenon. Work along these lines will be continued during the coming year.

The shifting of the bands, which results in all probability from a change in the distribution of the intensity among the individual lines, when the wave-length of the exciting light is altered, has been already alluded to. In the earlier stages of the investigation, it was necessary to work with the slits of the monochromatic illuminator wide open. Later on, as the methods improved, it was found possible to narrow them by degrees, until finally a fluorescent spectrum of sufficient intensity for observation was obtained with the slits not much wider than a hair. A remarkable phenomenon was at once observed: the slightest change in the wave-length of the exciting light caused the spectrum-lines to flutter about in a most extraordinary manner. It was quite impossible to follow the changes by the eye, they were so complex. As a line disappeared or appeared, the sharp and shaded edges often changed place, the appearance reminding one of a flag flying first on one side of the mast and then on the other. Obviously a vast amount of information regarding the nature of spectra is to be obtained by a careful study of these changes, which can only be done by photography. I have estimated that fifty or one hundred photographs will be necessary for a complete record of the changes which occur when the wave-length of the exciting light is shifted over the range 450 to 550, and as each picture requires an exposure of four or five hours, the task is seen to be no small one. If the spectra were then printed in succession on the film of a kinematograph and projected on a screen, it would be possible to follow at leisure the wonderful changes which occur.

At the time of writing this paper only half a dozen photographs have been obtained, three of which are reproduced in fig. 6, Pl. VII. Much of the detail is lost in the process of printing and reproduction, and on this account a careful drawing has been made on coordinate paper of all the details which are to be made out on the original negatives, fig. 7, Pl. VII. The white-light fluorescent spectrum A is recorded on the lower line, and the spectra obtained with nearly homogeneous radiation above, as is the case in the photograph as well. A number of the lines in the complete spectrum shown in fig. 6 have been lettered A, B, C, D, etc. These letters correspond with the letters in the diagram, fig. 7. The

letters at the left-hand side enable the spectra to be identified with the diagrams.

The line at which the excitation occurs is indicated by a double-headed arrow. This line always appears in the fluorescent spectrum. For example, in spectrum B the excitation is at wave-length 4962. In this case three lines of shorter wave-length appear, as well as the line at 50, and the lines at 504 and 5071, which belong to the groups of lines or bands already referred to, indicated by brackets. In spectrum C we are stimulating the vapour at 4935, and we obtain the second line in each of the first two groups. In spectrum D we stimulate another member of the same series, and also get the second line in the first two groups; while immediately above (E) by stimulating 4866 we obtain the third line in the second and third groups, this line being wanting in the first group, even when the vapour is excited by white light. In spectrum F, with a stimulation at 4839, we get another set of lines. The cause of the shift of the bands previously observed is at once apparent, the individual lines belonging to different groups of electrons. The number of groups has not yet been ascertained, but the photographs indicate that there are at least four, and probably more. It has not yet been possible to study the minute details which accompany the appearance or disappearance of a line, which are even more interesting, but another year's work on the subject will doubtless yield fruitful results. Longer exposures will doubtless enable these relations to be traced throughout a wider range of wave-lengths, but from what is already known it is clear that the shift in the apparent position of a group of lines results from a change in the distribution of the intensity among the individual lines constituting the group.

These experiments show in a striking manner the complexity of the piece of machinery which we will call the sodium molecule. Professor Rowland once said that a molecule is much more complicated than a piano. In most cases, all that we have been able to do is to strike the entire keyboard at once, but in the case of sodium it seems possible to strike one key at a time. A study of the fluorescent spectra of other vapours will doubtless do much to clear up the mystery of the mechanism of molecular radiation.

These investigations have been made possible through substantial aid received from the Rumford Fund of the American Academy, before which the paper has been presented. Much assistance has been rendered by Mr. A. H. Pfund, whose services were secured through a grant made by the Carnegie Institute.

Baltimore, June 1905.

LIX. *On the Method of Transmission of the Excited Activity of Radium to the Cathode.* By WALTER MAKOWER, B.A., B.Sc.; John Harling Research Fellow in the University of Manchester*.

1. Introduction.

IT has been shown by Rutherford† that when a negatively charged rod is exposed to the emanation from thorium in a closed vessel, the quantity of excited activity deposited upon the rod in a given time is independent of the pressure of the gas with which the emanation is mixed as long as this pressure exceeds a certain value, but that below this limit the quantity of excited activity deposited on the negative electrode diminishes, at first slowly, and then more rapidly, as the pressure of the gas is reduced.

This phenomenon gives reason to suppose that at the moment of its formation the excited activity is uncharged, and that it is only by some subsequent secondary action upon the gas in which it is produced that it acquires the positive charge in virtue of which it is carried to the negative electrode. It was with a view to investigating the nature of this secondary action that the present experiments were undertaken.

No experiments regarding this point seem as yet to have been carried out with radium emanation; for this reason, and on account of the rapid rate of decay of thorium emanation and the consequent inconvenience of working with this substance, radium emanation was employed in the present research.

The variation with pressure of the amount of excited activity deposited in a given time on a negatively charged rod when exposed to a constant quantity of emanation was carefully investigated, and it was found that at low pressure the excited activity deposited on the rod depended, not only on the pressure of the gas, but also on the distance between the positive and negative electrodes.

As the result of some preliminary experiments it was shown that the activity acquired by the rod was independent of the difference of potential between the electrodes over a considerable range. In these experiments a potential-difference of about 60 volts was used throughout.

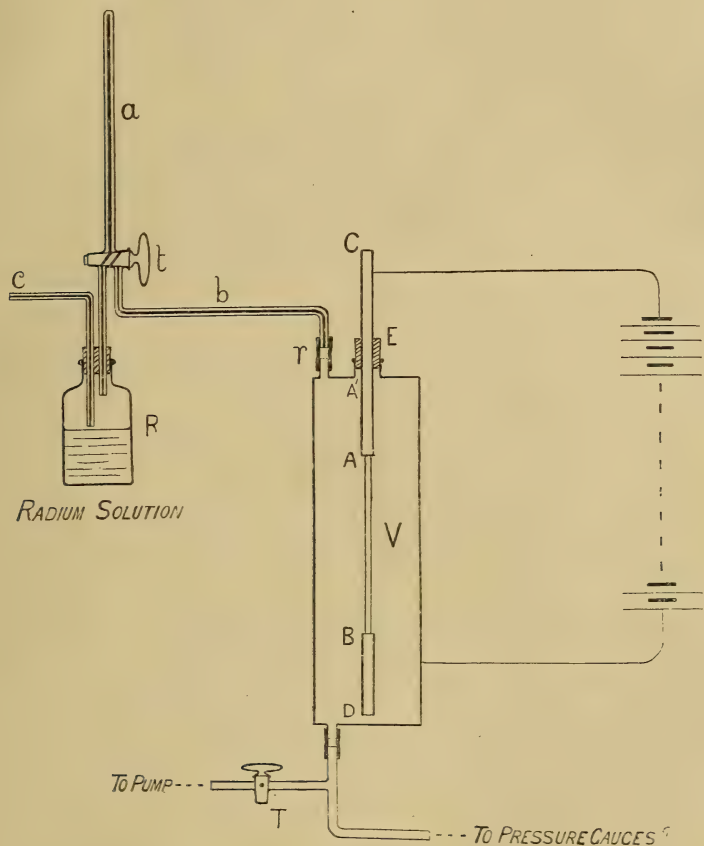
* Communicated by the Physical Society.

† Phil. Mag. [5] vol. xlix, (1900).

2. Description of Apparatus.

The cylindrical metal vessel V (fig. 1), which could be filled with emanation when desired, was connected to the

Fig. 1.



positive pole of a battery of thirty small storage-cells. The wire AB (14.5 cm. long) was screwed into a stouter rod AC connected to the negative pole of the battery, and passing through a rubber stopper E fitting air-tight into the neck of the vessel V. The portion AA' of the rod AC projected about 6 cm. into the vessel V, and another rod BD 6 cm. long was screwed to the lower extremity of AB, which was therefore exposed to a uniform electric field.

To obtain a constant supply of emanation, a strong solution of radium salt was placed in the glass bottle R provided with

a stopper, through which passed a long capillary tube C reaching to the surface of the solution but not dipping into it.

The air inside R was thus always kept at atmospheric pressure, and by making C sufficiently long the rate of escape of emanation from the bottle by diffusion was rendered small. After standing for some weeks, the concentration of the emanation in the air above the radium solution was found to have become sensibly constant.

To transfer a definite volume of gas from R into V, the closed limb *a* of the three-way capillary stopcock *t* was connected by the capillary *b* with the vessel V, to which it was attached by rubber *r*. By means of a pump the whole was exhausted to a low pressure, when the pump was disconnected by closing the tap T. The stopcock *t* was then turned so as to connect *a* with R, allowed to stand for a short time and then again turned into its original position. In this way the volume *a* (about .2 c.c.) of gas was transferred from R into V, and by repeating the operation any required volume of emanation could be introduced into V.

As the volume of *a* was always small compared with that of V, practically all the emanation in *a* was discharged into V at low pressures; at higher pressures it was easy to apply a small correction for the quantity of emanation left in *a*.

The pressure in V was registered on a mercury manometer at the higher pressures, and on a McLeod gauge at the lower pressures.

The rod AB was exposed for 30 minutes to the emanation, and then transferred to another metal cylinder in which its activity was tested in the usual manner by means of a quadrant electrometer. As under these conditions the activity of the rod remains constant for about 10 minutes*, it was possible to take several observations of the activity, the means of which are recorded in Tables I., II., and III.

3. Observations and Results.

Experiments were performed with three vessels V of diameter 1.1 cm. (vessel I.), 2.6 cm. (vessel II.), and 8.3 cm. (vessel III.) respectively. The pressure of the air with which the emanation was mixed was varied and the activity acquired by the rod in 30 minutes recorded.

With vessel I. it was found difficult to prevent the rod CD from touching the sides of the cylinder. The arrangement described above was therefore slightly modified by

* Miss Brooks, Phil. Mag. [6] vol. viii. p. 380 (1904).

making the cylinder V somewhat shorter and fixing the rod BD into a rubber stopper fitting into the end of the cylinder.

In the case of vessel I. it was necessary to work with very small quantities of gas in order to get to sufficiently low pressures, and the capillary *a* was discharged once into V. With vessels II. and III., *a* was discharged twice and three times respectively.

The maximum value to which the activity deposited on AB tends at high pressures is not the same for the three vessels, on account of the difference in their dimensions and of the variation in the quantity of emanation used.

The results obtained are given in Tables I., II., and III., in which the pressures are given in cm. of mercury and the quantity of activity on the rod in arbitrary units.

TABLE I. (Vessel I.) diameter 1.1 cm.

Pressure in cm. mercury.	Activity on rod.
15.3	180
8.9	174
6.35	167
4.3	161
2.65	134
1.7	120
0.95	93

TABLE II. (Vessel II.) diameter 2.6 cm.

Pressure in cm. mercury.	Activity on rod.
9.3	209
8.7	219
6.2	224
5.0	214
3.3	198.5
2.7	183
2.6	198
2.2	191
1.7	177
1.3	182
0.95	158
0.8	150
0.44	100
0.32	87
0.12	50

TABLE III. (Vessel III.) diameter 8.3 cm.

Pressure in cm. mercury.	Activity on rod.
5.3	394
5.15	410
4.15	413
2.0	409
1.7	398
1.4	385
1.05	386
0.8	367
0.41	332
0.38	346
0.17	268
0.10	178
0.086	170

4. Discussion of Results.

The experiments described above show that, at low pressures, the excited activity produced from radium emanation contained in a closed vessel is not confined to the cathode, as in the case at high pressures, but is distributed over the walls of the containing vessels and appears both on the anode and cathode, even in a strong electric field. This is precisely what has been previously found with thorium emanation*, and shows that the method of transmission is probably the same.

It will be further noticed from the results given above that in a large vessel the influence of pressure on the concentration of the excited activity on the cathode becomes appreciable only at low pressures, whereas with smaller vessels this influence is noticeable at much higher pressures. If, therefore, we assume that at the moment of its production the excited activity is uncharged, it is evident that the proportion of particles of excited activity which can traverse the gas in which it is formed, and reach the anode without becoming charged, is a function of the length of its path through the gas. As has been pointed out by Rutherford†, there are two ways in which the excited activity may be supposed to acquire a positive charge.

(1) The excited activity condenses on the positive ions existing in the gas and is thus transferred to the cathode; and

(2) The excited activity possesses the property of expelling

* Rutherford, *loc. cit.*

† Rutherford, *Phil. Mag.* [6] vol. v. p. 111 (1903).

negatively charged particles, and so becomes positively charged.

To decide between these two hypotheses, Rutherford performed experiments in which the number of ions in the vessel containing the emanation was increased by external means, and found that the amount of activity deposited on a cathode in a given time was not increased by this means. For this reason he rejected hypothesis (1) in favour of (2).

The experiments were, however, carried out at *atmospheric* pressure; and it is possible that, under these conditions, the number of ions produced by the emanation is sufficiently great to remove the excited activity as fast as it is formed, in which case any further increase of the ionization would be without further effect. To test this point still further, some similar experiments have been made at a *low* pressure, using vessel III. The walls of the vessel were made the cathode and the central rod the anode; 20 volumes of the capillary *a* (fig. 1) of emanation were introduced into the vessel and allowed to stand for $3\frac{1}{4}$ hours, the pressure being atmospheric. The emanation was then completely pumped out as quickly as possible, and a new inactive rod AB introduced and the field reversed. One volume of the capillary *a* was then introduced into the vessel, and the rod exposed to the emanation at a pressure of 1 mm. of mercury. Although the ionization in the vessel must have been increased about five times by the excited activity deposited on the walls of the vessel during the previous exposure to the emanation, the activity collected on the rod was found to be the same as when the walls were inactive. In a subsequent experiment 7 milligrams of pure radium bromide were brought close up to the vessel during the exposure of the negatively charged rod, without affecting the quantity of excited activity deposited on the cathode in 30 minutes. Supposition (2) is therefore the only one capable of explaining the observed phenomena.

It is of interest to speculate as to the mechanism by which a negatively charged particle is expelled from the excited activity during its passage through the gas. At the moment of formation of excited activity, the emanation from which it is produced expels an α particle* whereby the residue must itself be projected with considerable velocity. In their flight

* The negative charge left on the excited activity by the expulsion of an α particle may be neutralized by a simultaneous expulsion of slowly-moving electrons similar to those which Prof. J. J. Thomson (Proc. Camb. Phil. Soc. vol. xiii. part 1, p. 49) has shown to be given off by radio-tellurium.

these particles of excited activity would collide with the molecules of the gas in which they are produced, and it is suggested that in a certain percentage of these collisions a negative particle is expelled, leaving the excited activity positively charged. To explain the results obtained in this investigation on this view, it is necessary to assume that the particles of excited activity, when mixed with air at 1 cm. pressure, travel a distance comparable with 1 cm. before becoming charged. This distance is great compared with the mean free path of a molecule of air at the same pressure, and therefore only a very small fraction of the collisions can be effective in causing a negative particle to be expelled by the excited activity.

I am indebted to Professor Schuster both for placing the resources of his laboratory at my disposal and also for much valuable criticism during the course of these experiments.

LX. *Note on a Paper by W. Makower entitled "On the Method of Transmission of the Excited Activity of Radium to the Cathode."* By W. H. JACKSON, M.A., Assistant Lecturer in Mathematics in the University of Manchester*.

AS a result of some experiments on the method of transmission of the excited activity of radium, Mr. Makower has arrived at the following hypothesis:—The molecules of excited activity, initially uncharged, are projected from radium emanation with velocities large compared with those of gas molecules, and at a certain percentage of encounters with the gas molecules a negative electron is shot off, leaving the activity positively charged.

It gives additional force to his argument to further show that the deviation of the experimental results obtained by him from those required by the above theory are actually less than the errors of observation.

Since the number of ions present in a gas ionized by a small quantity of radium emanation is exceedingly small compared with the number of molecules of gas, it is safe on this view, even though the positive ion influences a much larger field of force than an ordinary molecule, to assume that the importance of encounters with positive ions is negligible.

The amount of activity which, in the experiments described by Mr. Makower, is deposited on the cathode, is to be taken proportional to the number of excited activity molecules

* Communicated by the Physical Society.

which have encounters with air molecules on their way to the sides of the cylindrical vessel.

Consider a number of particles travelling a distance x through air in which the average distance between molecules is λ , and the effective radii of molecules is σ . The distance may be divided into N layers each of thickness λ , in which each layer contains, on the average, $\frac{1}{\lambda^2}$ molecules per unit area, and the proportion, on the average, of encounters in each layer is $\frac{\pi\sigma^2}{\lambda^2}$. Hence the proportion of the particles which have no encounters is $\left(1 - \frac{\pi\sigma^2}{\lambda^2}\right)^N = \left(1 - \frac{\pi\sigma^2}{\lambda^2}\right)^{\frac{x}{\lambda}}$, and this for small values of λ is equal to $e^{-\frac{x}{L}}$, where $L = \frac{\lambda^3}{\pi\sigma^2}$ and is the "mean free path" of the particles in air.

Supposing now that the proportion of encounters effective in displacing a negative electron be R , the above result will be modified and the proportion of particles experiencing no "effective encounters" will be $e^{-\frac{R}{L}x}$.

In the experiments with which we are dealing, the central rod on which the activity was deposited was shielded at both ends, so that the calculation may be simplified by considering the problem as a two-dimensional one, that is by taking the cylinder to be of unlimited length. It is convenient to calculate the proportion of particles which reach the side of the cylinder without having any effective encounters, and then to subtract the ratio from unity.

Consider the number of particles which reach a small area of magnitude A , containing a point O . Use polar coordinates r, θ, ϕ , with O as origin; if P be any point of space let $OP=r$, and if Q be the projection of P on the right section of the cylinder through O , of which C is the centre, let $C\hat{O}Q=\theta$, and $Q\hat{O}P=\phi$.

Let M be the number of particles projected by the emanation per unit volume. The number of particles reaching the area A from an element of volume δV at P is proportional to the solid angle subtended by A at P , which is $\frac{A}{r^2} \cos C\hat{O}P$. In particular, the number reaching A from P is

$$e^{-\frac{R}{L}r} \frac{MA}{4\pi r^2} \cos \theta \cos \phi \delta V.$$

This expression is to be integrated over the infinite cylinder, and then it is clear that the deposit on any portion of the sides is proportional to its area. The area of unit length of the cylinder is πd , where d is the diameter, and the value of δV expressed in terms of r, θ, ϕ is $r^2 \cos \phi \delta \theta \delta \phi \delta r$. Denote the number of particles reaching unit length of the sides by N , we have

$$N = \frac{1}{4} M d \iiint e^{-\frac{R}{L} r} \cos^2 \phi \cos \theta \, d\theta \, dr \, d\phi \quad \dots \quad (1)$$

The limits of integration being

$$\begin{array}{lll} \theta, & -\cos^{-1}\left(\frac{r}{d} \cos \phi\right) & \text{to } \cos^{-1}\left(\frac{r}{d} \cos \phi\right) \\ r, & 0 & \text{to } d \sec \phi, \\ \phi, & -\frac{\pi}{2} & \text{to } +\frac{\pi}{2}. \end{array}$$

Performing the first integration with respect to θ , we have

$$N = M d \int_0^{\frac{\pi}{2}} \int_0^{d \sec \phi} e^{-\frac{R}{L} r} \cos^2 \phi \left(1 - \frac{r^2}{d^2} \cos^2 \phi\right)^{\frac{1}{2}} dr \, d\phi. \quad (2)$$

In this integral substitute $r = x d \sec \phi$, whence

$$N = M d^2 \int_0^{\frac{\pi}{2}} \int_0^1 e^{-\frac{R}{L} x d \sec \phi} \cos^2 \phi (1 - x^2)^{\frac{1}{2}} dx \, d\phi. \quad (3)$$

The maximum value of this integral is obtained by omitting the exponential factor, and corresponds to a vacuum when all the activity would reach the sides. Denoting the maximum value of N by N_0 , we find, as is otherwise obvious, that

$$N_0 = \frac{1}{4} \pi M d^2. \quad \dots \quad (4)$$

Let the ratio $N : N_0$ be denoted by s .

An effective approximation to the integral expression for the value of s may be found by expanding $(1 - x^2)^{\frac{1}{2}}$ in powers of x . Introduce the notation

$$\gamma_n(z) = \int_0^1 e^{-zx} x^n dx, \quad \dots \quad (5)$$

and write

$$\mu = \frac{R}{L} d. \quad \dots \quad (6)$$

Equation (3) may now be written

$$s = \frac{4}{\pi} \int_0^{\frac{\pi}{2}} \cos \phi [\gamma_0(\mu \sec \phi) - \frac{1}{2} \gamma_2(\mu \sec \phi) - \frac{1}{8} \gamma_4(\mu \sec \phi) - \dots] d\phi \quad (7)$$

We also have

$$\left. \begin{aligned} \gamma_0(z) &= \frac{1}{z} (1 - e^{-z}), \\ \gamma_2(z) &= \frac{1}{3} - \frac{1}{4} z + \frac{1}{2!5} z^2 - \frac{1}{3!6} z^3 + \dots, \\ \gamma_4(z) &= \frac{1}{5} - \frac{1}{6} z + \frac{1}{2!7} z^2 - \frac{1}{3!8} z^3 + \dots \end{aligned} \right\} \quad \dots \quad (8)$$

Also $\gamma_n(z)$ decreases continuously as either z or n increases.

If $\mu = \frac{10}{3}$, we have, if $z = \mu \sec \phi$,

$$\frac{1}{z} e^{-z} < \frac{1}{\mu} e^{-\mu} < \cdot 012,$$

$$\frac{1}{2} \gamma_2(z) < \frac{1}{2} \gamma_2(\mu) < \cdot 015,$$

and with an error of order 3 per cent. we may write

$$s = \frac{4}{\mu\pi} \int_0^{\frac{\pi}{2}} \cos^2 \phi d\phi = \frac{1}{\mu} \quad \dots \quad (9)$$

This simplified result should therefore hold if the defect from the maximum deposit of activity on the cathode is less than 30 per cent. Hence, with this restriction, the deposit of activity y should be given by the formula

$$y = N_0(1 - s) = N_0 \left(1 - \frac{L}{Rd} \right) \quad \dots \quad (10)$$

But the mean free path varies inversely as the pressure, and hence, if L_0 be the mean free path for a pressure of 1 cm. of mercury, and x cm. denote any other pressure, we obtain

$$y = N_0 \left(1 - \frac{L_0}{Rd} x \right) \quad \dots \quad (11)$$

This equation affords a means of determining the values of N_0 and $\frac{L_0}{R}$ which best suit each of the Tables I., II., III. obtained by Mr. Makower. Owing to the restriction that the value of y must be greater than $\cdot 7N_0$, it is found that this is an

inexact method of determining the value of $\frac{L_0}{R}$. In the accompanying figure, the maximum activities have all been reduced to 100, and the three curves are calculated by assuming the *mean* value of $\frac{L_0}{R}$ to be .7 cm.

The values of N_0 and $\frac{L_0}{R}$ were determined as follows :—

Assuming that the set of n points in question, *i. e.* $(x_1, y_1), \dots (x_n, y_n)$ belong to a curve of form

$$y = a - \frac{k}{x},$$

we make

$$\Sigma \left(y_r - a + \frac{k}{x_r} \right) = 0,$$

and

$$\Sigma \left(y_r - a + \frac{k}{x_r} \right)^2 \text{ a minimum.}$$

By differentiation this leads to the equations

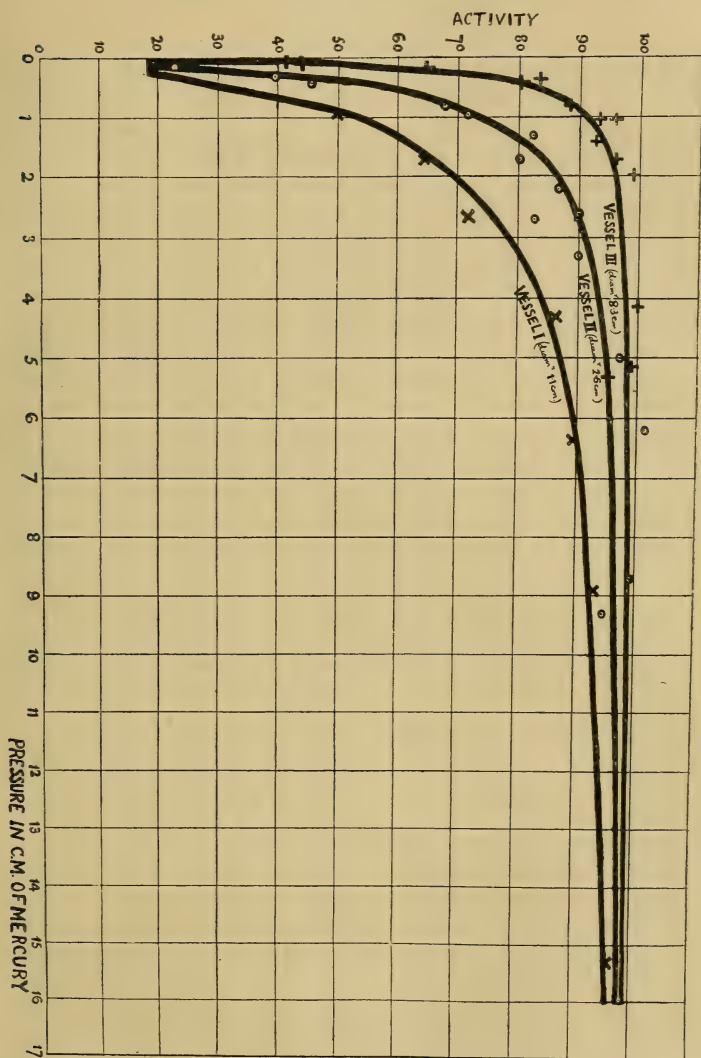
$$\left. \begin{aligned} k &= \frac{\Sigma y \Sigma \frac{1}{x} - n \Sigma \frac{y}{x}}{n \Sigma \frac{1}{x^2} - \left(\Sigma \frac{1}{x} \right)^2} \\ na &= k \Sigma \frac{1}{x} + \Sigma y \end{aligned} \right\} \dots \dots \dots (12)$$

In the curves for vessels II. and III. all the points for which the deposit was greater than 70 per cent. of the maximum were used in the above formulæ.

In the curve for vessel I. the point slightly below this value was used because the next point was clearly inaccurate. The result so obtained was practically identical with that obtained by leaving out both the points in question. On account of the difference in the values of $\frac{L_0}{R}$ caused by omitting even single points, this value is reliable only to one figure. The results are given in the following table :—

		N_0 .	$\frac{L_0}{R}$ in cm.
Vessel	I.	187	.71
Vessel	II.	221	.73
Vessel	III.	414	.64

It has only been possible to test the values of N_0 , as given by equation (4), very roughly, owing to the difficulty of



finding the volume of vessel V and its connexions, with sufficient accuracy to determine the relative values of M in the three sets of experiments.

LXI. *The Deflexion of α Rays from Radium and Polonium.*
 By A. STANLEY MACKENZIE, *Munro Professor of Physics,*
*Dalhousie University, Halifax, N.S.**

[Plate VIII.]

THE following investigation was begun with two aims in view: first, to measure from the deviations of a beam of α rays in a magnetic and in an electrostatic field the value of the velocity v , and of the ratio of the charge e on an α particle to its mass m , for the rays from radium and from polonium; and, secondly, to see if there were any evidence of a change in any of the quantities v , e , or m as the rays travelled in an ordinarily good vacuum.

The former problem for the case of radium was first treated by Rutherford†, using an electroscope to determine the deviations; and later by Des Coudres‡, who let the rays fall directly upon a photographic plate. Both used radium in radioactive equilibrium, and this method was followed in the present case; but it has the disadvantage that the beam of α rays not being homogeneous, it gives only the mean velocity of the rays. The method employed by Rutherford was admittedly not well adapted to give accurate numerical determinations; and Des Coudres states that too much stress must not be laid on the accuracy of his electrostatic experiment. In a paper just published Rutherford§ describes some very suggestive experiments made on the magnetic deflexion of the α rays from radium C, and promises to give later similar results for the deflexion in an electric field. The great advantage gained by the use of a thin coating of radium C on a wire is that the rays all leave the wire with the same velocity, and the beam is accordingly homogeneous. An accurate determination of the values of v and $\frac{e}{m}$ is to be expected from his completed measurements.

An effort was made to find a way of observing the position of the beam of rays coming through two slits without having to let them fall directly upon a photographic plate, since in the case of a path longer than a few centimetres this necessitates the use of a closed vessel and the production of a vacuum each time an observation has to be made. After experimenting with a thin layer of powdered zinc sulphide on glass, and finding that the scintillations were still quite brilliant when observed after transmission through the glass, I adopted

* Communicated by Prof. J. J. Thomson.

† Phil. Mag. Feb. 1903, p. 177.

‡ *Physik. Zeitschr.* iv. p. 483 (1903).

§ Phil. Mag. July 1905, p. 163.

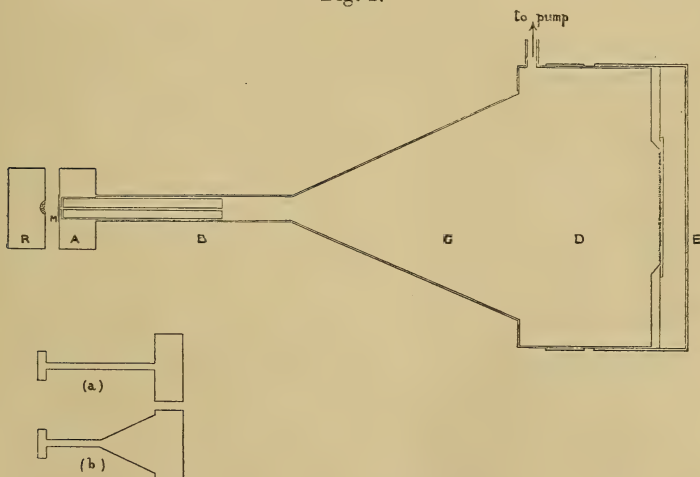
this method of observing the position of the end of the beam of α rays. I learned later that Mr. F. H. Glew of London has used this arrangement in his "scintilloscope," and he kindly consented to make for me the excellent zinc-sulphide screens used throughout these experiments.

It was hoped that the position of the line of scintillations could be observed by means of a microscope, but the amount of light was found to be too faint to enable one to make a setting with a cross-hair. The method adopted was to put the film side of a photographic plate in contact with the glass side of the zinc-sulphide screen, and photograph the scintillations. In order not to increase the width of the line unduly, the thickness of the glass of the screen had to be as small as was consistent with strength to withstand the atmospheric pressure inwards when a vacuum was made in the vessel. The thickness used was from three to four fifths of a millimetre according to the size of the opening to be covered by the screen.

Magnetic Deflexion of α Rays from Radium.

The design of the tube finally adopted for the measurement of the deflexion of the α rays from radium by a magnetic field is shown by figure 1. The vacuum-box is

Fig. 1.



made of brass, and consists of a solid disk A at one end, a hollow cylinder D at the other, and a tube BC joining them. The shape of the latter is shown by the small drawings (a) and (b) in the figure, its dimension parallel to the length of either slit being the same, 9 mm., at all parts to enable the

pole-pieces of the electromagnet to be placed at any part of its length and as close together as possible. The wedge-shaped part of the tube was to allow for the deflexion of the rays; but this expansion of that part of the box and the size of the cylinder D were made large, in order to prevent as far as possible secondary radiations from the walls of the box from affecting the photographic plate. The disk was bored almost through to receive the slit-tube, and the bottom was pierced by a slit 3 mm. long parallel to the slits in the slit-tube when the latter was in its proper position. This slit was covered on the outside by a piece of mica about $\cdot 0006$ cm. thick through which the α rays passed into the vacuous box. The mica was put on with sealing-wax, and was as thin as would safely withstand a difference of pressure of an atmosphere on its two sides. The slits were $\frac{1}{4}$ mm. wide, and 5.75 cm. apart. The circular opening in the end of the cylinder D was covered with the glass zinc-sulphide screen, powdered side inwards, put on with sealing-wax. A light-tight cover E fitting over the end of D served to make this end a sort of camera. The distance from the zinc sulphide to the nearest slit is 15.4 cm.

The radium was contained in, and filled, a shallow depression in a disk of brass, R, of the same diameter as A. This cell was attached to A when required, and the joint covered with soft wax to prevent the escape of emanation into the room. As the radium filled the depression in the cell, it was practically in contact with the mica when it was attached to the box, and thus the distance through which the α rays had to go in air at atmospheric pressure was very small.

A photographic impression of the line of scintillations when undeflected by the magnet could be obtained in two or three hours; but when deflected the exposure had to be increased several times, due in part to the absence in this case of the β and γ rays and in part to the dispersion of the beam of α rays.

In order to see whether the α particles always retain their charge when they travel, and whether the scintillations are in part due to uncharged particles, the magnetic field was applied over lesser and greater extents of the path of the rays and to different parts of the path. For this purpose it was necessary that the path of the rays from their source to the far end of the slit-tube should be always shielded completely from the magnetic force. This was satisfactorily attained by winding round this part of the box many turns of a thin ribbon of varnished soft iron.

Measurements were made with four different arrangements

of pole-pieces and their positions as regards the length of the path, as shown in the following table (two measurements are given for position III. with different magnetic fields). In the first three positions the main part of the field begins at about the same place, but extends over lengths of the path which are as 1:5:10 approximately. In position IV. we have the same pole-pieces as in II., but displaced 4 cm. further along the path of the rays from the slit. The value of the field H at every point of the path of the rays was determined by means of a small throw-bobbin and a ballistic galvanometer in the usual way, and the value of $\int dx \int H dx$ found graphically for use in the equation

$$\frac{mv}{e} = \frac{1}{d} \int dx \int H dx$$

(see J. J. Thomson, 'Conduction of Electricity through Gases,' p. 92), where d is the deflexion to either side of the centre.

Position.	Distance from upper slit to nearest edges of pole-pieces.	Length of pole-pieces measured along path of rays.	Distance from upper slit to centre of pole-pieces.	Value of field where uniform between pole-pieces.	Value of $\int dx \int H dx$.	Amount of deflexion to either side.	Value of $\frac{mv}{e}$ calculated.
.	2.25 cm.	0.80 cm.	2.65 cm.	4377 c.g.s.	119975 c.g.s.	.41 cm.	2.93×10^5
II.	2.15 "	3.75 "	4.03 "	4618 "	240223 "	.80 "	3.00 "
III. {	2.23 "	7.88 "	6.17 "	3062 "	245620 "	.82 "	2.99 "
	2.23 "	7.88 "	6.17 "	3483 "	280708 "	.92 "	3.05 "
IV.	6.15 "	3.75 "	8.03 "	4540 "	170325 "	.56 "	3.04 "

Mean value of $\frac{mv}{e} = 3.00 \times 10^5$ c.g.s. electromagnetic units.

The main error in the measurements comes in when determining d , the deflexion; for the bundle of α rays is very heterogeneous in velocity, and under the action of the magnetic field is spread out into a spectrum. The length of this spectrum is of course determined up to a certain limit by the length of exposure given, as it is strongest in the middle and weak on both sides. The deflexion measured was to the middle of the spectrum. An error of at least 3 per cent. is

possible in this measurement, and it will be seen from the above table that the values obtained for $\frac{mv}{e}$ agree to this approximation for the very different arrangements used. In figure 2 (Pl. VIII.) is given a copy of the negative used in measurement II. of the table, where the dispersion of the beam is about $\frac{3}{8}$ of the mean deviation.

The agreement of the values obtained for $\frac{mv}{e}$ points to the conclusion that the α particles, in a very rare gas of this kind at least, retain their charges unaltered along their path (that is, assuming that their velocities and masses remain unchanged, which is probable at low pressures). As one can mark by the use of a magnifying-glass the position of the scintillations, it is further proved that these scintillations are always deflected by the field, and that they are caused only by the α rays and cannot be due to uncharged particles, if such exist. The fact that with this apparatus one can make eye measurements is of great assistance in roughly checking up one's results and in arranging the conditions for the particular determination in hand.

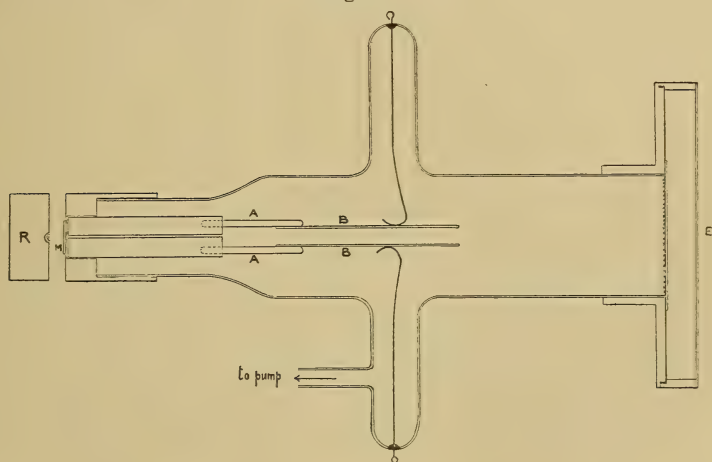
The mean value of the average $\frac{mv}{e}$ found was 3.00×10^5 . The values of the extremes of $\frac{mv}{e}$ as given by fig. 2 (Pl. VIII.) are 2.5×10^5 and 3.7×10^5 approximately. The value of $\frac{mv}{e}$ for radium in radioactive equilibrium found by Rutherford in his earlier experiment was 3.9×10^5 , and by Des Coudres 2.56×10^5 . The value I have found is between the two, and not far from that of Des Coudres. In his paper in this Magazine for July, Rutherford shows the amount of decrease in velocity of the α rays due to their passage through successive layers of aluminium $.00031$ cm. thick. The decrease in the present experiments due to the thin plate of mica through which the α rays have to pass before entering the vacuum box would be equal to that due to two or three of Rutherford's aluminium sheets, and so amount to about 6 per cent. Assuming that $\frac{m}{e}$ is constant this would make the value of the average $\frac{mv}{e}$ for the rays as they left the surface of the radium 3.18×10^5 , and of the extreme ones 2.65×10^5 and 3.92×10^5 . Since the fastest rays are those from radium C the value 3.92×10^5 must belong to them,

and this value is in entire agreement with the value 3.98×10^5 found by Rutherford in his paper just cited. Rutherford found that the power of acting on a photographic plate and of producing phosphorescence ceased for the α rays when their velocity fell to 64 per cent. of the maximum, that is when $\frac{mv}{e}$ is 2.55×10^5 . The minimum velocity which I have found above is 2.65×10^5 approximately, and if the scintillating action ceases at the same time as the rays fail to affect a photographic plate, this value of $\frac{mv}{e}$ would be that expected.

Electrostatic Deflexion of α Rays from Radium.

The general form of the apparatus used in measuring the electrostatic deflexion of the α rays from radium is like that already described, and is shown in figure 3. The radium cell

Fig. 3.



R, the mica window M, and the camera end E are as before. The charged plates BB were 6 cm. long, 1.87 cm. wide, and $\frac{4}{5}$ mm. thick, and were placed .506 cm. apart. They were attached to and supported by the metal slit-tube by means of two quartz rods AA soldered to them. The distance from the end of the slit-tube to the nearest edges of the plate was 1.72 cm., and from the end of the slit-tube to the zinc sulphide was 14.61 cm. The main tube was of glass with side tubes

for the entry of wires to connect the plates with the Wims-hurst machine employed for charging them. The greatest difference of potential used was 10,000 volts, and the following table gives the deflexions observed with that and lower voltages :—

Voltage between plates.	Observed deflexion to either side.	Deflexion to either side reduced to that for 10,000 volts.
10,000	·30 cm.	·30 cm.
10,000	·30 „	·30 „
8,000	·24 „	·30 „
6,000	·20 „	·33 „
6,000	·20 „	·33 „
10,000	·31 „	·31 „

Average deflexion d to either side produced by 10,000 volts = ·31 cm.

If we call F the electrostatic force along the path of the rays in the plane midway between the plates, the value of $\int dx \int F dx$ was calculated graphically after the equations given by Clerk-Maxwell in his ‘Electricity and Magnetism,’ § 202. The value of $\frac{mv^2}{e}$ calculated from the equation

$$\frac{mv^2}{e} = \frac{1}{d} \int dx \int F dx \times 10^8$$

(see J. J. Thomson, ‘Conduction of Electricity through Gases,’ p. 93), is $4 \cdot 11 \times 10^{14}$. The effect of the lines of force beyond the plates is thus to give a value 8 per cent. larger than if we had assumed that the field was bounded by the plates and everywhere uniform. No account was taken of the smaller effects due to the thickness of the plates, the distortion of the field by the metal slit-tube, or to the fact that the beam of rays is deflected from the central line, as they are of the order of magnitude of the other errors necessarily involved in the observations.

The distance d was measured to the centre of the deflected line, but the dispersion of the beam is not nearly so noticeable as in the magnetic experiments. This is in part due to the fact that the deflexions are smaller, and may be also in part due to the exposures used, but I think it is safe to say that the dispersion is not half so great as in the other case. In fig. 4 (Pl. VIII.) is shown a copy of a negative where 10,000 volts

were applied in one direction only ; the inner narrow line is the undeflected beam, and the other narrow line the deflected α beam. Thus it will be seen that the dispersion cannot be much more than 10 per cent. of the deviation. If there were different kinds of particles sent out by the radium, the lesser dispersion in the electric field than in the magnetic field would be explained if the energies of the different kinds of particles were much more nearly the same than their momenta, as these are the physical qualities involved in the two cases respectively.

The wide fuzzy line in figure 4 on the opposite side of the central line from the deflected α rays must be due to β rays. They are deflected about two or three times as far as the α rays. If they are β rays from the radium itself, they must be exceedingly rapid and have a velocity nearly that of light; but they may be secondary β rays. Further investigation is needed before one can state their real source.

We have thus for the average ray $\frac{mv^2}{e} = 4.11 \times 10^{14}$, and $\frac{mv}{e} = 3.00 \times 10^5$; whence the average $v = 1.37 \times 10^9$ cm. per sec., and $\frac{m}{e} = 4.6 \times 10^3$ electromagnetic units.

The extreme velocities would be from the values given above for $\frac{mv}{e}$ in the magnetic deflexion experiments, and assuming no dispersion in the electrostatic observations, 1.11×10^9 and 1.64×10^9 ; or, increased by 6 per cent. to allow for the absorption by the mica, we have for the slowest, the average, and the fastest α rays leaving the surface of a small quantity of radium (12 mgr.) the velocities 1.18×10^9 , 1.45×10^9 , and 1.74×10^9 cm. per sec. respectively, about $\frac{1}{26}$ of the velocity of light. To be compared with my value of $v = 1.37 \times 10^9$ are Rutherford's earlier estimate of 2.5×10^9 and Des Coudres' value of 1.65×10^9 . My value of 1.74×10^9 for the fastest rays observed is decidedly less than that deduced by Rutherford in his paper on the rays from radium C, viz., 2.6×10^9 .

The value of $\frac{e}{m} = 4.6 \times 10^3$ is much smaller than the values previously found by Rutherford and Des Coudres, which lay between 6 and 6.5×10^3 . Assuming that the charge on the α particle is the same as that carried by the hydrogen atom, for which the value of $\frac{e}{m}$ is 10^4 in the units used above, we

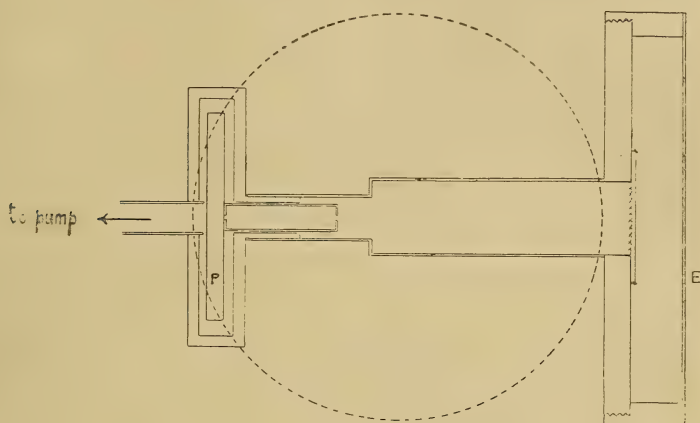
see that the mass of the α particle is 2.2 times that of the hydrogen atom, or about that of the hydrogen molecule. If helium is monatomic its atomic weight must be 4, and the above experiments do not lend any weight to the view that the α particle is either a hydrogen or a helium atom, but suggest that it is a hydrogen molecule. Even if we take the extreme value of $\frac{mv}{e}$ in one direction, viz., 3.7×10^5 , and an extreme value of $\frac{mv^2}{e}$ in the opposite direction, let us even suppose that the dispersion in the electric experiment were great enough to extend to 10 per cent. on either side of the mean and that $\frac{mv^2}{e}$ could be as small as 3.7×10^{14} , the calculated mass would be only 3.7 times that of a hydrogen atom. A still greater dispersion would be necessary in the electric experiment if we were to take the extremes in the reverse way in order to bring the mass equal to that of a hydrogen atom. It may be that there are both hydrogen and helium sent out together from the radium, and that their energies are about the same to give a small electric dispersion, but their momenta not nearly so approximately equal. Such a condition might give the phenomena observed.

Magnetic Deflexion of α Rays from Polonium.

On account of the much feebler α radiation from polonium than from radium, the dimensions of the apparatus for measuring the magnetic and electrostatic deflexions of α rays from polonium have to be smaller than those used in the experiments already described. The polonium plate, P in figure 5, was obtained from Sthamer of Hamburg, and was in the form of a strip 7 mm. wide and 4 cm. long. It was inserted in the centre of a narrow brass box carrying two tubes opposite to each other and perpendicular to the surface of the plate with the polonium coating; one of these tubes was connected to the pump, and the other served for holding inside it the slit-tube, and outside it the brass tube connecting the narrow polonium box with the camera-end E. The slits were about $\frac{1}{3}$ mm. wide, and 2.15 cm. apart, and one was in contact with the polonium. The distance from the polonium to the zinc sulphide was 7.33 cm. The whole arrangement, with the exception of the camera-box, could be put between the poles of the electromagnet, shown by the dotted circle in the figure, so that with the exception of the last 5 mm. at

the camera end, where the field gradually fell off, the whole path of the rays was through a uniform magnetic field H of 6235 c.g.s. units.

Fig. 5.



Photographic plates were exposed with the field first in one direction and then reversed, the exposure for each side requiring from one to two days. There was no noticeable dispersion of the beam of rays, so that the rays given off by polonium have all the same velocity. A copy of one of the negatives from which measurements were made is shown in figure 6 (Pl. VIII.).

The double deflexions observed on two plates were $\cdot 71$ and $\cdot 73$ cm. respectively, giving as the mean deflexion to either side of the centre $\cdot 36$ cm. The calculated radius of curvature R is 53.0 cm.

Hence the value of $\frac{mv}{e} = HR = 3.30 \times 10^5$.

If we assume that the value of $\frac{m}{e}$ is the same for these particles as for those from radium, we see that their velocity is greater than that of the average α particle from radium, but not so fast as that of the fastest. On page 171 of Rutherford's paper in the July number of this Magazine, he calculates that α particles which have a maximum range in air of 4 cm., which is about that of the polonium rays, will have a velocity of 88 per cent. of that of the α rays from radium C. Taking Rutherford's value of 3.98×10^5 as the value of $\frac{mv}{e}$ for radium C, this would give the corre-

sponding value for the polonium rays 3.50×10^5 , which is somewhat larger than the value 3.30×10^5 found above.

The determination of the electrostatic deflexion of these rays is now in progress, and will be completed by my former colleague, Dr. W. B. Huff, to whom I wish to return thanks for assistance kindly given in taking the observations in the electrostatic experiments with radium.

These experiments were carried on at the Cavendish Laboratory under the guidance of Prof. J. J. Thomson, and I wish in conclusion to thank him for the privilege of working at the laboratory, and for his continued kindness and suggestions during the progress of the work.

Cavendish Laboratory,
July 31st, 1905.

LXII. *On the Magnetization and the Magnetic Change of Length in Ferromagnetic Metals and Alloys at Temperatures ranging from -186° C. to $+1200^\circ$ C.* By K. HONDA, *Rigakuhakushi, Lecturer in Physics, Tokyo University*, and S. SHIMIZU, *Rigakushi, Lecturer in Physics, High Normal School**.

[Plates IX.-XII.]

IN the Journal of the College of Science of Tokyo Imperial University (vol. xix. art. 11, 1903), Professor H. Nagaoka and one of us published the results of experiments on the magnetization and magnetostriction of nickel-steels containing different percentages of nickel. The present experiments were undertaken, on the one hand, to extend the above investigation to different temperatures, and, on the other hand, to form a continuation of our former experiments*.

The experiments were made in three separate stages. In the first series of experiments, which extended from February 21 to July 2, 1903, the magnetization and the magnetic changes of length at ordinary and liquid air temperatures were measured; in the second series, extending from January 17 to February 2, 1904, the same measurements were extended so as to include different intermediate temperatures between the ordinary and liquid air temperatures; lastly, in the third series of experiments, which extended from March 10 to May 17, 1904, the magnetizations at high temperatures were measured.

* Communicated by the Authors.

† K. Honda and S. Shimizu, *Phil. Mag.* vol. vi. p. 392 (1903).

Our specimens consisted of five ferromagnetic metals, and twelve specimens of nickel-steels, kindly placed at our disposal by M. Ch. Ed. Guillaume. They were all examined in the form of ovoids (major axis = 20 cm. and minor axis = 1 cm.).

In the first series of experiments, the specimens were first annealed for about 4 hours at 1000°C . to 1100°C . in a charcoal fire, after being well wrapped in asbestos, and were then gradually cooled. These annealed specimens were tested at the ordinary temperature, then at that of liquid air, and lastly again at ordinary temperature. In the second series of experiments, the measurements were always commenced with freshly annealed ovoids. Lastly, in the third series, all specimens were cooled in liquid air for about 15 or 20 minutes. The measurements at the ordinary, and then at higher temperatures, were carried out; measurements were also made at different descending temperatures down to the ordinary. Thus all the measurements, when set down in order, form a complete cycle with regard to temperatures, whose limits lie between liquid air temperature and 1200°C . The methods and the results of the experiments are given in the following pages.

1. FIRST SERIES.

The apparatus for measuring the change of length was similar to that used in our former experiments above referred to. The ends of the ovoid to be tested were soldered to two short brass rods, each end of the ovoid entering about 2 mm.



into the rod. The upper piece is connected with a wire stretched vertically, and the lower piece is screwed to the bottom of the specimen-holder, as shown in the annexed cut. The axis of the ovoid can be adjusted by three small screws. The holder was made of a copper tube, with three long slits, equally distant from one another, along its axis. These slits permit the adjustment of the specimens to the axial line of the tube. The rest of

our apparatus was exactly the same as in the former experiment.

In the present experiments, the magnetization was, at the same time, measured by the magnetometric method. The magnetometer consisted of a bell-shaped magnet suspended

by a quartz fibre in a thick copper case. A magnetizing coil (length = 40 cm., $4\pi n = 394.4$) and a compensating coil of nearly the same dimensions were placed respectively due magnetic east and west of the magnetometer. The magnetometer was placed in such a position that the specimen exerted the maximum effect upon it. The vertical component of the earth's field was compensated for. The deflexion of the magnetometer was measured by means of a scale and telescope.

In measuring magnetization, the following precautions were taken. The verticality of the two coils was tested by means of a level. The line of the magnetometer, the compensating coil and the magnetizing coil, was then tested by a compass needle. The compensation of the earth field and then that of the magnetizing coil were next effected; lastly, the scale and telescope were placed in correct positions.

The precautions above enumerated were especially necessary, as the magnetization and the magnetic hysteresis in strong fields were to be studied with the ovoids placed in vertical positions. Though these precautions were taken with the utmost care, the magnetizations by opposite currents of equal strength were not exactly equal in absolute amounts, so that a small asymmetry of the hysteresis curve was also observed in the strong fields. This difference was, in the most unfavourable case, not greater than 1 per cent. of the total magnetization for a field of 700 c.g.s. This probably arose from a slight deviation of the coils from the vertical line. If the lines of force at the centre of the magnetometer be not vertical, but be in the meridian plane, the field due to the coils may slightly affect the horizontal component of the earth's field, without producing any deflexion of the magnetometer. If the horizontal component be increased by a current in one direction, a current in the opposite direction will diminish it. In the first experiment, therefore, two magnetizing curves for opposite currents were taken. These two curves almost coincided with each other below a field of 200 c.g.s., but slightly deviated above that field. Since the disturbing force is proportional to the magnetizing current, the correction for the intensity of magnetization can be derived from a pair of the opposite magnetizations by equal and opposite currents; hence in the second and third experiments, the magnetizations by equal and opposite currents of the maximum strength were taken, and the correction was found, and applied to magnetization by currents of one direction.

The current was measured by a Siemens-Halske ammeter, which was occasionally compared with a Kelvin ampere-balance.

The experiment was conducted in the following order. The adjustment of the magnetizing and compensating coils having been completed, the specimen-holder containing the ovoid was fixed vertically in the correct position in the magnetizing coil. The ovoid was then vertically stretched upwards by means of a copper wire with a spiral spring, special care being taken to stretch the copper wire in the direction of the axis of the ovoid. The magnetic change of length was then measured in the usual way. The magnetization and the magnetic hysteresis were next observed. Liquid air was next gently poured into the Dewar tube in the magnetizing coil, until the tube was nearly filled with the liquid; then the exposed parts above the magnetizing coil were carefully protected with cotton-wool. Owing to the boiling of the liquid, a small oscillation of the image in the field of the telescope was at first observed, but after some ten minutes the image became almost steady. The change of length was then taken. Next, adding more liquid air to that in the Dewar tube, when necessary, the magnetization and the magnetic hysteresis were measured. Lastly, when the specimen was heated to the temperature of the room, the change of length and the magnetization were again noted. After the experiments, the compensation of the magnetizing current was always tested, and found perfect, except in a few cases.

(a) *Magnetization of Ferromagnetic Metals.*

In Table I. the magnetization in different fields is given, where I is the intensity of magnetization, and H the internal field (external field—demagnetizing force). The temperature of liquid air has been assumed to be -186°C ., but owing to the fractional evaporation of nitrogen, as the experiment proceeds, the actual temperature may, according to circumstances, be greater or less by a few degrees than the above value. The figures of the last row in each column are the residual magnetism when the external field vanishes.

TABLE I.
Swedish Iron.

$t=27^{\circ}5$ C.		$t=-186^{\circ}$ C.		$t=21^{\circ}5$ C.	
H.	I.	H.	I.	H.	I.
1.77	81	2.23	77	1.76	86
3.55	243	3.39	247	2.85	250
4.10	582	4.55	453	3.39	431
7.40	890	5.00	578	4.41	632
16.3	1138	15.4	1085	7.00	858
30.3	1251	44.7	1302	14.1	1102
53.4	1330	135.0	1464	33.7	1264
103.7	1419	221.8	1543	108.8	1424
234.6	1545	283.9	1587	225.4	1536
349	1609	385	1636	390	1623
473	1647	478	1665	485	1648
554	1621	543	1681	547	1660
	33.5		39.7		37.6

Tungsten-Steel.

$t=23^{\circ}5$ C.		$t=-186^{\circ}$ C.		$t=30^{\circ}7$ C.	
H.	I.	H.	I.	H.	I.
2.22	22	1.37	13	2.16	21
8.36	119	5.89	64	4.38	49
10.90	275	19.75	130	6.91	104
12.10	528	12.40	254	9.25	193
15.7	695	16.8	661	10.35	376
20.1	928	28.8	1035	25.8	1064
28.5	1062	62.7	1235	111.2	1326
96.6	1292	140.1	1371	216.2	1413
185.1	1381	272.4	1468	333	1463
282.1	1432	402	1523	397	1483
416	1477	469	1535	483	1499
574	1503	570	1557	557	1509
	123		136		127

Nickel.

$t=13^{\circ}0\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=17^{\circ}0\text{ C.}$	
H.	I.	H.	I.	H.	I.
1.11	14	1.42	9	0.53	7
3.26	88	3.34	26	3.63	91
6.96	177	11.35	187	7.92	192
11.12	237	21.95	298	18.60	309
19.85	322	34.1	357	38.1	400
37.94	394	80.4	418	63.8	439
64.1	438	152.8	443	117.3	469
153.6	478	240.3	462	182.5	483
247.6	490	342	477	332	494
359	496	493	490	484	503
493	503	586	506	639	505
710	510	704	518	703	506

Cast Cobalt.

$t=20^{\circ}0\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=19^{\circ}5\text{ C.}$	
H.	I.	H.	I.	H.	I.
5.1	72	3.90	28	2.58	22
10.7	170	10.57	122	14.14	223
13.9	247	22.65	277	20.03	302
22.8	371	39.3	427	24.85	374
35.7	497	74.7	608	33.1	454
54.5	628	101.9	690	50.6	594
85.9	749	206.6	848	77.3	676
147.7	865	281.1	911	121.3	780
244.3	953	357	956	275.3	941
392	1027	449	999	406	1010
467	1047	570	1038	543	1053
610	1088	668	1063	659	1083

Annealed Cobalt.

$t=18^{\circ}6\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=31^{\circ}0\text{ C.}$	
H.	I.	H.	I.	H.	I.
4.37	6	7.80	9	6.94	8
13.53	26	16.23	20	18.39	29
25.25	67	39.0	64	29.23	56
36.59	115	57.8	102	45.07	108
52.5	171	80.0	148	65.0	164
86.7	257	119.6	221	95.7	232
122.3	320	167.4	282	147.5	313
221.8	433	248.4	358	167.3	415
351	532	347	426	364	508
445	583	496	504	484	572
527	613	561	543	542	603
699	687	702	589	634	664

From these numbers, we see that in Swedish iron, tungsten-steel and nickel, the cooling by liquid air diminishes the magnetization in low fields, but increases it in the strong. In Swedish iron and tungsten-steel the change is very small, amounting at most to 2 or 3 per cent.; but in nickel the initial diminution is considerably larger, amounting to about 10 per cent. in the maximum. The field, in which the effect of cooling changes its sign, is 115 c.g.s. for Swedish iron and tungsten-steel, and 580 c.g.s. for nickel. Fleming and Dewar*, who first thoroughly studied the magnetization of iron and steel in liquid air, did not observe an increase of magnetization; perhaps the field was too weak to indicate such an increase.

In cast and annealed cobalts cooling always diminishes magnetization; the effect is rather greater in cobalt when annealed than in the cast state.

In Swedish iron, tungsten-steel, and nickel the magnetizations before and after the cooling coincide with each other for all fields. In cast and annealed cobalts there is a considerable residual change of the magnetic condition.

(b) *Magnetization of Nickel-Steels.*

The intensities of magnetization at the temperatures of the room and of liquid air are given in Table II.

TABLE II.
Nickel-Steel 70·32 per cent.

$t=21^{\circ}\cdot 1\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=30^{\circ}\cdot 5\text{ C.}$	
H.	I.	H.	I.	H.	I.
0·24	9	0·28	6	0·93	35
1·52	120	2·02	96	1·52	127
2·20	310	2·61	221	1·65	233
3·91	500	3·62	455	2·75	374
11·20	721	10·94	662	6·07	593
17·20	794	20·5	835	11·17	712
32·5	903	31·4	911	16·20	784
63·3	969	45·5	971	36·9	869
91·4	988	81·4	1031	52·9	952
232·1	1004	169·3	1063	101·3	987
381	1008	253·3	1072	186·0	996
492	1008	337	1078	283·6	999
576	1008	439	1088	438	999
672	1009	601	1098	628	999
	22·2				

* Fleming and Dewar, Proc. Roy. Soc. lx. p. 81 (1896).

Nickel-Steel 50·72 per cent.

$t=24^{\circ}\cdot 0\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=32^{\circ}\cdot 0\text{ C.}$	
H.	I.	H.	I.	H.	I.
1·86	58	1·22	33	1·54	55
2·92	207	3·80	197	2·24	112
3·35	324	4·13	333	2·74	191
5·16	569	5·55	509	4·44	498
8·80	727	8·27	712	8·77	736
17·12	946	21·9	1003	19·52	961
36·4	1076	27·2	1051	33·4	1066
51·4	1137	41·6	1133	49·5	1126
73·0	1181	65·8	1207	84·8	1184
146·3	1225	148·0	1285	168·0	1220
251·4	1241	230·6	1302	255·3	1229
328	1244	315	1308	375	1230
426	1246	439	1310	518	1235
604	1247	573	1312	602	1237

Nickel-Steel 46 per cent.

$t=20^{\circ}\cdot 0\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=21^{\circ}\cdot 0\text{ C.}$	
H.	I.	H.	I.	H.	I.
0·29	7	1·59	38	1·54	47
2·30	79	6·00	359	2·79	116
3·02	175	7·90	554	3·74	228
4·08	279	10·85	770	5·55	490
4·92	362	19·90	974	10·49	754
5·88	473	26·60	1059	15·30	883
8·22	684	39·6	1147	29·8	1029
11·10	793	49·0	1187	66·5	1145
17·15	909	72·8	1246	166·4	1206
33·8	1054	100·9	1295	311	1219
91·1	1171	166·6	1338	411	1221
266·7	1214	242·5	1360	489	1222
372·1	1219	455	1377	608	1222
532·9	1222	631	1384		

Nickel-Steel 36 per cent.

$t=13^{\circ}\cdot 0\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=19^{\circ}\cdot 0\text{ C.}$	
H.	I.	H.	I.	H.	I.
1·27	32	2·18	30	1·48	36
2·45	73	6·00	207	3·39	138
4·00	176	8·16	426	5·37	335
7·45	472	11·70	621	9·14	550
9·50	524	16·60	801	12·60	658
11·55	624	26·3	982	16·95	741
16·05	714	52·2	1153	29·4	863
30·4	853	72·8	1210	55·9	939
65·1	953	121·5	1279	102·2	985
110·5	989	169·0	1310	187·4	1002
203·5	1007	248·3	1332	341	1014
350	1012	326	1344	436	1015
473	1013	450	1354	528	1016
662	1014	636	1367	657	1017

Nickel-Steel 29·24 per cent.

$t=23^{\circ}0\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=36^{\circ}5\text{ C.}$	
H.	I.	H.	I.	H.	I.
1·58	41	5·32	33	0·69	5
2·46	66	12·97	146	4·00	27
4·41	92	24·48	382	8·45	63
22·40	138	29·38	464	18·14	180
28·5	144	43·2	573	26·45	316
35·3	147	59·0	658	38·4	394
43·7	150	71·0	696	54·1	404
83·6	156	101·1	793	91·5	544
210·5	171	152·4	886	117·8	592
324	181	235·0	981	210·4	677
440	189	307·4	1030	320	740
504	193	403·1	1080	465	789
592	197	519	1121	551	808
657	201	591	1135	623	887
			192		171

Nickel-Steel 29 per cent.

$t=12^{\circ}0\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=16^{\circ}0\text{ C.}$	
H.	I.	H.	I.	H.	I.
1·00	11	2·24	9	2·20	8
2·95	131	10·13	93	4·52	29
7·60	207	13·39	155	9·35	75
19·6	272	20·74	321	12·92	123
67·1	307	29·5	505	15·95	180
143·7	315	37·6	608	24·72	355
243·6	321	48·0	708	44·2	582
400	328	75·0	858	74·5	708
515	333	139·6	1036	105·7	779
532	333	182·9	1104	174·9	874
738	341	356	1247	279·3	954
		424	1276	378	1004
		517	1306	547	1052
		631	1330	664	1079

Nickel-Steel 28·74 per cent.

$t=22^{\circ}0\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=26^{\circ}0\text{ C.}$	
H.	I.	H.	I.	H.	I.
0·29	6	2·97	14	0·85	4
1·61	70	6·85	35	8·91	77
4·20	156	11·42	106	13·12	128
13·70	242	18·28	201	17·01	193
29·20	278	27·82	378	22·38	325
46·2	292	30·8	427	33·0	453
87·0	305	39·5	549	48·1	566
159·3	314	66·7	730	79·6	658
233·3	319	178·9	991	111·8	746
320	321	235·3	1060	207·7	851
406	327	316	1098	307	911
472	328	375	1153	457	966
627	333	461	1191	543	987
672	335	523	1227	662	1110
	17·3		226		182

Nickel-Steel 28·32 per cent.

$t=17^{\circ}5\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=31^{\circ}7\text{ C.}$	
H.	I.	H.	I.	H.	I.
0·52	6	3·06	23	3·77	24
2·42	23	5·21	54	6·56	51
6·40	37	9·54	107	12·34	134
14·58	48	12·43	156	21·09	262
24·24	53	23·45	375	28·10	342
43·1	59	42·7	558	44·0	452
97·2	69	86·0	759	96·0	609
206·0	84	128·8	866	188·4	726
327	98	175·9	944	264·6	799
493	112	253·4	1029	438	886
596	119	360	1104	504	904
672	126	429	1132	602	931
752	128	515	1161		
		578	1178		
	0		145		141

Nickel-Steel 26·64 per cent.

$t=20^{\circ}0\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=27^{\circ}7\text{ C.}$	
H.	I.	H.	I.	H.	I.
23·2	2	3·57	18	3·87	2
71·6	5	8·72	45	9·03	49
124·3	8	13·90	105	16·47	129
225·0	11	18·10	194	23·89	267
355	14	28·15	369	38·7	488
524	17	48·5	605	50·5	613
635	18	57·2	671	65·2	721
744	20	73·9	764	89·2	836
		107·6	893	145·7	982
		169·8	1023	230·0	1085
		246·0	1118	307·2	1161
		383	1219	422	1231
		466	1260	501	1255
		559	1293	621	1287

Nickel-Steel 25 per cent.

$t=12^{\circ}5\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=15^{\circ}0\text{ C.}$	
H.	I.	H.	I.	H.	I.
...	...	32·9	1·1	...	...
...	...	53·1	2·3	58·3	0·6
...	...	91·8	3·7	94·9	1·4
...	...	128·1	5·4	155·8	2·3
354	0·4	209·5	8·0	344·8	2·9
...	...	313	11·0	401	3·2
516	2·0	498	15·2	529	3·3
		605	17·5	749	3·3
		752	20·3		

Nickel-Steel 24.40 per cent.

$t=18^{\circ}7$ C.		$t=-186^{\circ}$ C.		$t=23^{\circ}5$ C.	
H.	I.	H.	I.	H.	I.
29.5	1.9	6.22	23	1.03	3
71.0	3.4	12.54	60	12.22	78
126.2	5.4	28.15	241	19.16	153
230.4	7.8	40.4	359	26.2	260
412	9.3	62.7	529	37.1	408
541	10.6	93.6	675	47.6	508
653	10.8	120.0	747	63.1	630
769	11.5	183.5	860	83.7	731
		238.1	926	119.6	839
		282.8	989	174.7	933
		370.4	1034	272.8	1024
		448	1067	438	1141
		510	1107	530	1174
		575	1137	644	1210
	2.0		251		232

Nickel-Steel 24.04 per cent.

$t=16^{\circ}0$ C.		$t=-186^{\circ}$ C.		$t=30^{\circ}0$ C.	
H.	I.	H.	I.	H.	I.
9.5	2.6	0.99	3	3.32	11
19.6	7.2	4.35	14	8.28	36
33.8	14.1	11.14	44	16.58	101
52.8	22.2	27.35	191	29.8	295
89.8	33.2	32.9	262	54.4	575
141.3	42.4	61.3	526	67.6	657
226.6	53.1	83.4	643	83.9	741
417	68.4	107.0	725	124.3	861
536	75.6	183.8	870	169.5	942
626	79.9	260.6	968	222.8	1012
763	85.4	376	1053	322	1090
		466	1101	418	1155
		540	1143	512	1191
		591	1152	579	1212
	9.3		280		256

As in the case of iron, tungsten-steel, and nickel, the magnetization of the alloys of nickel-steels containing percentages of nickel greater than 26.64 per cent. is decreased in weak fields and increased in strong, by cooling them in liquid air. In alloys containing lower percentages of nickel, the magnetization always increases on cooling. The amount of the change of magnetization by cooling is considerably large; with the exception of 28.73 per cent. nickel-steel, it increases as the percentage of nickel decreases, up to 26.64 per cent. The magnetization in liquid air of 25 per cent.

nickel-steel, which is almost non-magnetic at ordinary temperatures, is also very small. As the percentage of nickel further decreases, the change of magnetization by cooling again increases. With 26·64 per cent. nickel-steel, which undergoes the greatest change of magnetization in liquid air, the intensity of magnetization increases from 16 to 1275 for $H=600$ c.g.s., or by about 80 times.

The magnetizations, before and after cooling, of reversible nickel-steels containing greater percentages of nickel than 36 per cent., nearly coincide with each other. If, however, irreversible nickel-steels be once cooled in liquid air, the recovery to the initial value becomes less and less as the percentage diminishes; in 24·40 per cent. and 24·04 per cent. nickel-steels, the magnetization after cooling is even greater than that in liquid air. Some of the above results had already been obtained by Hopkinson*, Osmond†, and Dumas‡.

(c) *Hysteresis-loss in Ferromagnetic Substances.*

The hysteresis was studied at the temperature of the room and at that of liquid air. The areas of the hysteresis-loops were carefully measured by a planimeter with the results given in Table III. and in Pl. IX. figs. 1 *a, b*.

TABLE III.—*Ferromagnetic Metals.*

	Ordinary Temperature.				Liquid Air Temperature.			
Swedish Iron.	H	4·0	6·0	15·8	H	1·9	6·4	63·7
	B	4504	10660	17160	B	4792	10680	17400
	W	2236	8195	20960	W	2091	8518	23070
Tungsten Steel.	H	10·7	16·8	91·5	H	12·5	18·3	91·5
	B	3493	10030	16460	B	3223	9668	16520
	W	4610	25790	56520	W	4266	28320	69750
Nickel.	H	6·3	20·9	120·5	H	1·1	26·8	122·2
	B	1996	4405	5931	B	1569	4117	5518
	W	806	3341	3915	W	1589	6763	10240
Cast Cobalt.	H	6·6	12·6	92·4	H	7·4	14·8	96·3
	B	1107	2753	9259	B	960	2425	8676
	W	481	2391	21220	W	441	2272	21880
Annealed Cobalt.	H	43·7	117·2	2180	H	48·3	122·7	224·3
	B	1802	3702	5508	B	1115	2893	4466
	W	5669	22680	35770	W	3244	19970	38710

* Hopkinson's Original Papers, vol. ii. p. 227.

† Osmond, *C. R.* cxxviii. pp. 306 & 1396 (1899); *C. R.* cxviii. p. 532 (1894).

‡ Dumas's *Recherches sur les Aciers au Nickel*, pp. 49–67 (1902).

Here H , B , and W denote respectively the internal field, the magnetic induction, and the hysteresis-loss, all expressed in c.g.s. units. In Swedish iron, the hysteresis-loss is decreased in weak inductions and increased in the strong, by cooling it in liquid air. Fleming and Dewar* found no effect of cooling on the hysteresis-loss of iron. In tungsten-steel, nickel, cast and annealed cobalts, the hysteresis-loss is always increased by cooling. These changes are briefly expressed by saying that the cooling in liquid air magnetically hardens the specimens.

In comparing the hysteresis-loss of different metals, it is to be observed that the hysteresis-loss in nickel is the smallest among them, and that those for tungsten-steel and cast cobalt are greater by about three times than the hysteresis-loss of Swedish iron, and the same relation also holds between annealed cobalt and tungsten-steel.

From the courses of the curves in fig. 1, it is evident that Steinmetz's formula giving the relation between the hysteresis-loss and the maximum induction holds for nickel and annealed cobalt up to an induction of 3000 c.g.s.; it holds for cast cobalt and tungsten-steel up to 8000 c.g.s.; and lastly for Swedish iron, it fails beyond an induction of 18,000 c.g.s. If, however, the specimens are cooled in liquid air, the applicable range of the law for induction is notably extended, as may be seen from fig. 1.

As regards the residual magnetism, the cooling always increases it in a marked degree.

(d) *Hysteresis-loss in Nickel-Steels.*

The hysteresis of nickel-steels was studied at ordinary temperature, and that of seven of them at liquid air temperature. The results are given in Table IV. and in Pl. IX. figs. 2 *a*, *b*.

The hysteresis-loss of nickel-steels at ordinary temperature is generally small compared with that of other ferromagnetic metals. The values for reversible alloys are, however, comparable with those of nickel; but for irreversible alloys they are all very small. Especially nickel-steel of 28·32 per cent. does not almost enclose any area, giving only 16 ergs for an induction of 1200 c.g.s. If the alloy has a greater value for induction, it will be very useful for the construction of transformers. Thus the magnetic state of the irreversible nickel-steels corresponds to that of ferromagnetic metals at high temperatures. As seen from fig. 2 *a*, the Steinmetz's formula does not apply, except for very weak inductions.

* Fleming and Dewar, *loc. cit.*

TABLE IV.
Nickel-Steels.

	Ordinary Temperature.				Liquid Air Temperature.			
70.32 %	H B W	2.1 2675 886	3.4 7069 3510	31.2 11370 5353	H B W	2.5 2628 9841	4.9 6850 4140	30.2 11550 7494
50.72 %	H B W	3.9 4104 2015	4.6 6790 4300	38.9 13910 11160				
46 %	H B W	2.9 1350 202	4.9 3845 1969	7.9 7783 5836	H B W	2.6 919 1026	4.9 3493 1970	6.8 7677 6867
36 %	H B W	4.0 2782 1264	9.5 6910 5290	39.7 11520 9404	H B W	5.8 2581 1654	9.5 6948 8000	31.0 12800 19303
29.24 %	H B W	0.7 749 14.7	6.9 2774 109	78.0 419 155				
29 %	H B W	1.3 1099 106	9.7 2807 395	33.7 3654 393	H B W	5.0 507 79	14.5 2132 2271	22.6 5188 13120
28.74 %	H B W	0.89 1261 58	4.96 3054 189	53.3 4662 322				
23.32 %	H B W	46.5 673 4.5	218.6 1184 16		H B W	11.8 1938 1888	19.3 3964 7439	57.2 8394 28060
26.64 %	H B W	25.3 64 51	62.3 173 276	148.7 354 703	H B W	14.5 1552 1396	23.2 3406 7864	60.2 8076 36900
24.40 %	H B W	35.6 94 153	85.0 215 511	126 295 731				
24.04 %	H B W	30.3 331 1110	102.7 608 2586	244.6 926 4108	H B W	17.1 1180 1309	29.6 3212 10520	89.4 8783 63550

If the alloys be cooled in liquid air, the hysteresis-loss increases. With irreversible alloys, the increase is enormous. The applicable limit of Steinmetz's formula becomes greatly extended. Thus we may say, as in the case of the pure metals, that the cooling in liquid air hardens the specimens magnetically.

As regards the residual magnetism, the cooling considerably increases it.

(e) *Length Change of Ferromagnetic Metals.*

Table V. and figs. 3 *a, b* (Pl. X.) give the observed changes of length at the temperatures of room and of liquid air. Here $\frac{\delta l}{l}$ denotes the elongation per unit of length.

TABLE V.
Swedish Iron.

$t=24^{\circ}5\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=29^{\circ}0\text{ C.}$	
H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$
4.0	0.21	3.0	0.9	4.0	0.7
8.5	2.7	5.3	1.5	5.1	1.6
21.7	3.5	20.0	3.2	11.7	3.4
72.4	3.3	66.5	3.2	37.0	4.2
123.5	2.4	165.0	0.9	104.0	3.3
217.5	0.13	274	-2.0	214	1.1
326	-2.3	396	-4.5	313	-0.8
483	-4.8	565	-7.6	478	-3.3
640	-6.2	...	...	642	-4.5
792	-7.0	814	-10.2	839	-5.1

Tungsten-Steel.

$t=23^{\circ}2\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=30^{\circ}7\text{ C.}$	
H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$
10.0	0.2	17.6	0.9	13.7	1.0
13.9	1.5	30.0	3.1	20.7	3.1
31.0	3.5	91.0	4.3	45.7	4.3
81.0	4.6	174	4.3	112.3	4.8
209	4.4	287	3.4	174.2	4.5
393	3.4	366	3.1	363	3.4
507	2.6	525	2.3	527	2.6
671	2.4	587	2.1	640	2.3
818	2.1	827	1.5	848	1.9

Nickel.

$t=19^{\circ}5\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=14^{\circ}0\text{ C.}$	
H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$
5.4	- 2.3	3.6	- 0.8	5.0	- 1.5
10.0	- 6.5	9.6	- 2.5	7.6	- 4.0
18.3	-12.1	16.0	- 7.4	13.4	- 8.8
32.2	-19.4	31.2	-15.9	29.2	-18.0
75.0	-29.5	74.9	-24.4	59.3	-26.4
145.8	-34.7	138.4	-28.5	110.4	-32.2
243.6	-36.7	252.6	-31.7	215.5	-36.0
353	-37.3	441	-34.9	387	-37.2
492	-37.7	662	-37.9	485	-37.4
796	-38.0	800	-39.4	772	-37.7

Cast Cobalt.

$t=20^{\circ}\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=20^{\circ}0\text{ C.}$	
H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$
14.3	-1.1	9.4	- 0.3	15.6	-1.1
33.2	-4.2	30.0	- 3.3	33.5	-3.5
62.3	-7.5	64.2	- 7.5	63.4	-6.4
106.5	-9.3	126.4	-10.2	124.3	-7.7
167.1	-9.3	234.7	-11.7	231.3	-7.1
217.7	-8.5	350	-10.9	449	-5.7
348	-6.1	465	- 9.8	465	-3.8
498	-4.0	616	- 8.7	616	-1.5
762	-0.2	749	- 7.2	755	-0.1

Annealed Cobalt.

$t=13^{\circ}5\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=25^{\circ}5\text{ C.}$	
H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$
31	-0.1	95	-0.3	59	- 0.2
46	-0.2	167	-1.1	98	- 0.9
86	-1.0	312	-2.7	153	- 1.7
142	-1.8	422	-3.4	268	- 3.3
224	-2.8	512	-4.5	444	- 6.1
347	-4.3	635	-6.1	538	- 7.6
498	-6.1	755	-7.4	717	-10.3
647	-7.9	772	-7.6	869	-12.4
800	-9.8				

In Swedish iron and tungsten-steel, cooling by liquid air decreases the elongation of the metals; the change in tungsten-steel is very small, but in Swedish iron it is relatively large. In nickel, the contraction is diminished by cooling to a field of 670 c.g.s.; but it is increased in stronger fields. In cast cobalt, the contraction is considerably increased, except in weak fields, where a slight decrease of contraction is observed. In annealed cobalt, the contrary is the case; the contraction is always diminished.

With tungsten-steel and nickel, the magnetic changes of length before and after the cooling coincide with each other. But in Swedish iron, the elongation after cooling becomes greater than that before cooling. In cast cobalt, the magnetic contraction after cooling slightly decreases, as compared with the elongation before cooling; but with annealed cobalt, the contrary is the case, except in weak fields.

The above results for Swedish iron, nickel, and annealed cobalt agree with those obtained by us* with rods of these metals. The change of elongation of tungsten-steel in high fields does not coincide with that found in our former experiments. But the change being very small, the discrepancy may be accounted for by taking into consideration the demagnetizing force, with which our former experiments were not concerned.

(f) *Length Change of Nickel-Steels.*

The observed changes of length are given in Table VI. and in figs. 4 a, b, c, d, e, f, g, h, i (Pl. X.).

TABLE VI.
Nickel-Steel 70·32 per cent.

$t=22^{\circ}0\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=26^{\circ}5\text{ C.}$	
H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$
3·0	0·6	3·0	0·7	2·9	0·9
8·3	3·2	19·0	5·2	20·2	5·3
21·1	6·1	32·8	8·6	67·9	10·4
45·9	9·2	85·3	11·1	121·2	11·6
109·4	11·2	106·8	12·1	280·1	11·9
232·6	11·7	228·7	12·8	422	12·0
392	11·8	334	12·9	499	12·0
546	11·9	473	13·0	639	11·9
663	11·7	631	12·9	802	11·9
791	11·6	763	12·6		

* K. Honda and S. Shimizu, *loc. cit.*

Nickel-Steel 50.72 per cent.

$t=26^{\circ}.5\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=27^{\circ}.5\text{ C.}$	
H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$
3.8	0.6	3.8	0.4	5.0	1.0
6.8	2.6	6.8	1.9	9.9	4.3
11.3	5.5	13.0	5.1	44.2	14.5
23.8	10.2	41.0	12.8	102.7	19.8
56.6	16.0	106.0	20.7	236.0	22.8
182.8	22.1	207.2	24.8	363	23.5
280.2	23.0	351	26.4	482	24.1
449	24.0	506	26.7	673	24.7
561	24.2	665	26.7	783	24.9
695	24.3	835	26.8		

Nickel-Steel 46 per cent.

$t=20^{\circ}.0\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=21^{\circ}.0\text{ C.}$	
H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$
4.0	0.3	5.0	0.9	4.0	0.6
7.0	2.8	7.7	2.8	4.8	1.2
15.5	6.2	26.4	9.2	9.4	4.0
76.3	18.5	82.0	20.4	28.1	10.5
163.0	22.7	113.0	22.0	88.7	19.0
255.5	23.8	310	28.6	207.9	22.8
440	24.8	522	29.5	326	23.7
608	25.3	728	30.7	442	24.0
760	25.4			603	24.4
				746	24.4

Nickel-Steel 36 per cent.

$t=20^{\circ}.0\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=14^{\circ}.0\text{ C.}$	
H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$
5.1	0.3	5.6	0.6	5.3	0.3
10.2	2.7	10.4	1.4	13.0	3.3
37.8	8.0	28.2	5.1	27.9	6.6
101.4	13.5	76.3	14.0	71.9	11.8
205.9	16.0	186.4	23.8	174.8	15.5
363	17.4	289.8	26.5	298	16.8
503	18.4	428	23.8	441	17.6
616	19.0	587	29.8	592	18.6
756	20.3	738	30.5	717	19.3

Nickel-Steel 29.24 per cent.

$t=19^{\circ}5\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=23^{\circ}6\text{ C.}$	
H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$
16.0	0.6	13.5	0.3	20.0	0.3
55.0	1.0	26.0	0.9	52.6	1.7
93.3	1.7	66.8	2.7	101.9	3.2
149.8	3.4	142.2	6.0	171.8	4.9
291.8	3.6	264.0	9.6	304	7.7
441	5.3	422	13.1	444	9.8
591	6.7	524	15.0	565	11.5
699	7.8	675	16.5	708	13.1
897	9.7	837	17.8	801	13.9

Nickel-Steel 29 per cent.

$t=11^{\circ}5\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=14^{\circ}0\text{ C.}$	
H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$
15.4	1.1	22.9	0.1	18.8	0.3
36.9	1.9	49.3	2.5	31.8	1.1
83.6	2.7	105.7	7.7	59.6	3.2
147.6	3.4	206.9	12.6	122.2	5.6
323	4.9	313	16.4	349	11.1
472	6.9	412	18.0	470	12.8
692	9.0	555	20.1	633	14.5
850	10.3	681	21.6	770	15.8

Nickel-Steel 28.74 per cent.

$t=22^{\circ}5\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=21^{\circ}5\text{ C.}$	
H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$
11.4	0.6	35.0	1.0	16.0	0.2
46.7	1.7	81.0	4.0	37.0	1.6
92.6	2.4	138.7	6.7	76.0	3.5
155.7	2.7	184.7	8.2	133.2	5.6
281.0	4.2	248.0	10.3	250	8.5
419	5.6	327	11.8	337	10.3
681	7.2	519	15.4	457	12.4
782	8.6	687	17.7	633	14.7
		822	19.4	820	16.7

Nickel-Steel 28.32 per cent.

$t=20^{\circ}0\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=21^{\circ}5\text{ C.}$	
H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$
21.3	0.2	27.0	0.8	15.0	0.2
50.2	0.6	39.0	1.5	41.5	1.5
85.3	0.8	66.2	2.7	92.0	3.0
169.0	1.2	101.0	4.4	144.4	4.9
281.6	2.0	231	9.4	275	8.3
378	3.0	368	12.9	420	11.2
455	3.6	454	14.3	599	14.0
601	4.8	618	16.2	788	16.4
810	6.1	826	18.3	878	17.4

Nickel-Steel 26.64 per cent.

$t=24^{\circ}5\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=21^{\circ}0\text{ C.}$	
H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$
28.7	0	19.6	0.2	20.0	0.3
81.3	0	42.0	1.6	47.2	2.4
118.2	0	92.6	4.8	94.0	5.7
180.0	0.08	145.0	6.9	168	9.3
302	0.13	255	10.8	300	12.8
489	0.15	353	12.9	408	14.9
680	0.17	490	15.4	530	16.9
890	0.21	593	16.9	668	18.2
		774	17.7	778	18.8
		935	19.5		

Nickel-Steel 24.40 per cent.

$t=25^{\circ}0\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=20^{\circ}5\text{ C.}$	
H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$
...	...	22.7	0.1	19.0	0.3
...	...	50.8	1.4	32.0	0.9
...	...	91.0	3.1	78.2	3.4
118	0	156.5	5.2	149	6.8
236	0	295	9.3	240	9.5
367	0.02	405	11.2	362	12.2
494	0.08	544	13.1	484	14.6
705	0.10	640	13.9	621	16.3
886	0.11	839	16.9	795	18.4

Nickel-Steel 24.04 per cent.

$t=21^{\circ}0\text{ C.}$		$t=-186^{\circ}\text{ C.}$		$t=23^{\circ}0\text{ C.}$	
H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$
22.9	0	34.3	0.7	35.3	1.0
76.5	0.1	65.7	2.5	66.7	2.9
196.6	0.2	124.0	5.9	112.0	5.4
281.7	0.3	238.5	9.9	253	10.0
429	0.35	334	13.3	398	12.7
600	0.6	468	17.8	508	14.4
808	0.7	622	21.6	695	15.9
		835	25.6	858	16.9

The effect of cooling on the magnetic elongation in nickel-steels is exactly parallel to the same effect on magnetization. In nickel-steels containing percentages of nickel greater than 28.74 per cent., the elongation is diminished in weak fields and increased in the strong, by cooling them in liquid air; with other nickel-steels, the initial decrease of elongation vanishes.

The ratio of the elongation in liquid air to that at ordinary temperature increases in strong fields, as the percentages of nickel decrease. In 36 per cent. Ni, it amounts to about 1.6 in $H = 500$ c.g.s.; and in 28.32 per cent. Ni to 3.7, and in 24.40 per cent. Ni to 160 for the same field.

For reversible nickel-steels, the elongations after and before cooling coincide with each other. The elongation of other nickel-steels, once cooled in liquid air, is always greater than that before cooling. With 26.64 per cent. and 24.40 per cent. alloys, the elongation is even increased, by heating it to the ordinary temperature.

25 per cent. nickel-steel does not sensibly elongate at ordinary temperature nor in liquid air.

(g) Change of Density by Cooling.

The density of the irreversible nickel-steels at ordinary temperature suffered a permanent change, if they were once dipped in liquid air. This singular fact was first observed by Hopkinson*. The following table contains the observed values of density:—

* Hopkinson's Original Papers, vol. ii. p. 240.

TABLE VII.

Alloys.	28.32 % Ni.	26.64 % Ni.	24.40 % Ni.	24.04 % Ni.
Before cooling ...	8.15	8.16	8.13	8.06
After cooling ...	8.01	7.99	8.06	7.94

Thus the density is diminished by cooling them in liquid air; M. Ch. Ed. Guillaume* specially investigated this point by measuring the coefficient of thermal expansion at low temperatures. He found that the irreversible nickel-steels expand on being cooled in solid carbon dioxide and again expand when heated to ordinary temperatures. Hence the effect of cooling is to doubly diminish the density of the alloys.

II. SECOND SERIES.

To obtain a constant low temperature lying between the ordinary temperature and that of liquid air, a method of slow cooling was applied. The specimen-holder in the former apparatus was covered with a water-tight brass cylinder, and a suitable amount of liquid air was poured into the interspace between the cylinder and the Dewar tube. The temperatures above -15° C. were, however, obtained by dipping the specimen directly into a freezing-mixture (snow and common salt) contained in the Dewar tube. The experiment was commenced with the specimen in the annealed state, and measurements were made at successively decreasing temperatures. During one set of observations, which usually required 7 or 8 minutes, the temperature was fairly constant and its change did not exceed one degree in the most unfavourable case. Since the cooling was very slow and the specimen was doubly enclosed in copper and brass tubes, the temperature of the specimen may be regarded as constant throughout its entire length.

The temperature of the specimen was measured by a thermoelectric couple of platinum and german-silver. The wires were insulated with a thin caoutchouc tube. One of the junctions was brought in contact with the specimen at its middle, while the other was insulated with asbestos papers and inserted in a copper tube. The tube was dipped into the water-bath, and its temperature was observed with a thermometer placed in the bath. The thermoelectric current was measured with a low

* Guillaume, *Bulletin de la Société d'Encouragement*, Mars 1898, p. 273.

resistance galvanometer. The calibration of the galvanometer was made by using a mercury thermometer and a petroleum-ether thermometer.

Since the character of the pure metals and the reversible nickel-steels was not much altered by cooling them in liquid air, the measurements of the magnetization and the magnetic change of length at the intermediate temperatures were confined to only the irreversible nickel-steels, that is, those whose percentage-contents of nickel were less than 29.24 per cent. (excluding 25 per cent. Ni).

(a) *Magnetization of Nickel-Steels.*

The observed values of the intensity of magnetization are given in Table VIII. Here H and I have the same meaning as before.

TABLE VIII.
Nickel-Steel 29.24 per cent.

$t = -1^{\circ}9 \text{ C.}$		$t = -32^{\circ}5 \text{ C.}$		$t = -62^{\circ}5 \text{ C.}$	
H.	I.	H.	I.	H.	I.
0.15	10	0.16	8	0.16	15
0.40	41	0.23	48	0.66	24
0.79	79	0.49	105	2.26	76
1.34	110	1.10	167	5.40	152
3.19	172	2.19	232	9.01	227
6.88	226	3.89	298	17.07	341
12.33	264	11.22	381	22.65	391
22.81	287	12.49	453	33.8	450
46.2	297	67.6	530	61.9	561
75.4	301	120.9	565	120.0	670
151.5	306	188.0	583	234.7	758
313	311	323	590	329	811
448	315	369	596	408	812

Nickel-Steel 29 per cent.

$t = 12^{\circ}4 \text{ C.}$		$t = -37^{\circ}5 \text{ C.}$		$t = -121^{\circ}8 \text{ C.}$	
H.	I.	H.	I.	H.	I.
0.21	18	0.24	24	0.42	8
0.61	69	0.52	52	1.93	53
0.98	109	1.01	107	3.93	107
1.84	164	2.30	203	9.87	215
3.00	214	4.19	287	16.30	309
5.34	273	9.20	405	22.58	376
13.17	352	15.50	475	45.5	506
26.62	375	31.1	521	77.3	614
66.4	382	62.2	539	160.5	740
161.2	387	126.9	544	280.8	813
302	390	261.5	546	395	845
438	392	416	548		
	3.5		5		47

Nickel-Steel 28.74 per cent.

$t = -10^{\circ}4 \text{ C.}$		$t = -54^{\circ}5 \text{ C.}$		$t = -108^{\circ}0 \text{ C.}$	
H.	I.	H.	I.	H.	I.
0.22	16	0.25	13	1.24	12
0.43	42	0.63	62	4.29	64
0.95	95	1.83	153	9.05	153
2.09	154	2.95	205	14.64	261
3.69	210	5.17	275	21.40	359
9.68	317	10.99	382	29.83	424
19.82	383	25.02	481	49.2	536
32.0	402	69.9	542	66.9	607
64.8	415	189.0	550	131.2	741
189.0	419	325	550	218.0	827
298.8	420	420	551	310	874
445	423			380	904
	2.4		3.8		91

Nickel-Steel 28.32 per cent.

$t = -12^{\circ}5 \text{ C.}$		$t = -45^{\circ}0 \text{ C.}$		$t = -79^{\circ}5 \text{ C.}$	
H.	I.	H.	I.	H.	I.
0.31	24	0.16	18	0.45	16
0.86	55	0.50	62	0.96	40
2.63	101	1.41	121	2.79	86
4.73	128	3.33	186	6.24	142
14.98	159	7.00	252	10.98	203
32.9	170	15.46	301	17.85	266
75.3	180	28.78	315	30.2	336
180.6	193	73.4	321	63.2	417
332	205	168.9	325	90.0	451
450	211	345	327	191.4	502
		438	328	305	526
				419	542
	0.5		0.4		19.5

Nickel-Steel 26.64 per cent.

$t = -9^{\circ}6 \text{ C.}$		$t = -36^{\circ}5 \text{ C.}$		$t = -80^{\circ}0 \text{ C.}$	
H.	I.	H.	I.	H.	I.
10.0	2.8	3.95	9	2.15	12
20.2	7.0	11.38	24	5.96	39
39.3	17.2	24.95	58	11.60	92
94.6	32.3	36.6	82	16.43	142
195.7	47.8	55.9	122	31.0	290
312	60.0	129.8	168	50.1	386
470	71.9	222.0	212	63.0	437
		332	247	97.2	524
		446	275	169.5	623
				224.6	726
				321	736
				392	770
	15.3		64		166

Nickel-Steel 24.40 per cent.

$t=6^{\circ}5$ C.		$t=-13^{\circ}9$ C.		$t=-68^{\circ}7$ C.		$t=-105^{\circ}5$ C.	
H.	I.	H.	I.	H.	I.	H.	I.
11.4	1.9	5.1	2.1	3.0	7	2.6	9
16.9	3.2	11.5	6.1	10.4	28	9.2	38
27.2	5.4	19.1	13.0	16.8	57	18.5	108
38.5	7.7	28.2	21.2	26.0	119	25.1	176
55.9	11.1	43.2	33.5	35.8	187	32.1	268
98.9	16.1	64.5	43.7	57.3	283	45.6	339
193.9	24.5	102.7	58.4	69.6	335	75.7	481
291.4	31.5	170.4	75.8	104.9	401	106.8	561
391	37.2	255.5	90.5	177.0	488	166.2	651
		323	99.2	297.9	569	226.4	717
				407	618	320	786
						396	826
	7.3		33.6		192		226

Nickel-Steel 24.04 per cent.

$t=-6^{\circ}2$ C.		$t=-13^{\circ}2$ C.		$t=-60^{\circ}8$ C.		$t=-99^{\circ}0$ C.	
H.	I.	H.	I.	H.	I.	H.	I.
5.9	3.2	4.4	4	4.1	9	2.6	8
13.1	9.4	11.6	11	11.2	32	7.3	24
26.2	26.0	25.2	37	20.6	82	16.1	66
43.7	46.8	40.5	71	34.1	191	21.4	123
74.1	72.8	79.4	123	49.1	285	29.3	223
134.1	95.3	159.5	175	66.2	363	49.2	367
228.8	118.8	295.4	219	101.1	461	62.8	452
342	137.3	393	239	184.8	580	80.4	533
457	148.3	450	250	255.6	661	112.5	629
				400	713	240.8	807
						282.2	842
						323	870
	50.4		87		231		266

In weak fields, the intensity of magnetization gradually increases as the temperature falls till it reaches a maximum, and then gradually decreases. As the field is increased this maximum recedes towards lower temperatures, and beyond 50 c.g.s. the maximum altogether disappears. These changes are common to nickel-steels of 29.24 to 28.32 per cent. Ni. In 24.04, 24.40, and 26.64 per cent. Ni, the maximum does not appear from the outset; *i. e.*, as the temperature falls the intensity of magnetization at first rapidly increases and soon approaches to an asymptotic value for every magnetizing field.

(b) *Length Change of Nickel-Steels.*

The magnetic change of length of nickel-steel is given in Table IX. and in figs. 4 *c, d, e, f, g, h, i* (Pl. X.), from which the curves of the change of length for constant fields are obtained and drawn in figs. 5 *a, b, c, d, e, f* (Pl. XI.).

TABLE IX.

Nickel-Steel 29.24 per cent. *Nickel-Steel 29 per cent.*

$t = -0^{\circ}.3 \text{ C.}$		$t = -70^{\circ}.0 \text{ C.}$		$t = -38^{\circ}.0 \text{ C.}$		$t = -123^{\circ}.3 \text{ C.}$	
H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$
4.7	0.3	4.2	0.2	3.4	0.7	10.8	0.2
18.6	1.5	15.3	0.8	12.3	2.8	40.5	1.8
65.1	2.0	46.1	2.2	36.9	4.7	116.5	5.6
118.6	2.7	61.1	3.8	72.3	5.6	241.2	9.3
251.6	4.0	177.7	7.7	175.8	6.5	321	10.6
333	4.8	287.6	9.8	302	7.0	394	11.8
469	5.7	404	11.8	415	7.7		

Nickel-Steel 28.74 per cent. *Nickel-Steel 28.32 per cent.*

$t = -8^{\circ}.3 \text{ C.}$		$t = -53^{\circ}.0 \text{ C.}$		$t = -45^{\circ}.2 \text{ C.}$		$t = -77^{\circ}.5 \text{ C.}$	
H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$
6.8	0.8	2.7	0.3	15.8	1.4	11.1	0.2
17.2	2.1	9.9	1.6	56.0	2.2	25.7	1.0
72.9	3.5	53.2	5.1	149.3	3.2	34.9	1.4
217.3	4.8	175.3	6.7	248.3	4.0	79.5	2.6
316	5.7	299	7.7	353	5.1	193.3	3.9
430	6.8	418	8.6	443	5.9	286.6	5.1
						417	6.4

Nickel-Steel 26.64 per cent. *Nickel-Steel 24.40 per cent.*

$t = -39^{\circ}.5 \text{ C.}$		$t = -78^{\circ}.4 \text{ C.}$		$t = -69^{\circ}.2 \text{ C.}$		$t = -97^{\circ}.7 \text{ C.}$	
H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$
45.7	0.3	26.6	0.5	21.4	0.1	28.8	0.5
127.9	1.0	67.8	2.0	47.8	0.2	67.0	1.9
268.0	2.0	166.7	4.8	152.9	2.2	177.5	3.8
442	3.8	266.5	7.0	240	3.5	281	6.6
		390	9.5	336	4.8	389	7.9
				411	5.3		

Nickel-Steel 24·04 per cent.

$t = -2^{\circ} \cdot 0 \text{ C.}$		$t = -64^{\circ} \cdot 0 \text{ C.}$		$t = -96^{\circ} \cdot 0 \text{ C.}$	
H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$	H.	$\frac{\delta l}{l} \times 10^6.$
40·9	0·1	43·5	0·5	20·5	0·1
124·4	0·2	114·0	2·0	44·2	1·0
266	0·6	217·3	3·7	125·4	3·4
463	1·1	402	6·6	216·8	5·6
				318	7·7
				412	9·3

From these numbers, we find a parallelism between the change of magnetization and that of the length-change. In weak fields, the change of length gradually increases as the temperature falls till it reaches a maximum, and then decreases. As the field becomes stronger the maximum elongation is displaced in lower temperatures, and at last vanishes. These changes are common to nickel-steels of percentages higher than 28·32 per cent.; for percentages lower than 26·64 per cent. the elongation for a constant field at first increases gradually and then rapidly, soon approaching an asymptotic value as the temperature falls.

[To be continued.]

LXIII. *On the Constitution of Natural Radiation.*

By J. LARMOR, F.R.S. *

THE recent paper by Lord Rayleigh on "The Origin of the Prismatic Colours" † recurs to fundamental and delicate points in the philosophy of Optics, first effectively expounded by himself in 1881 and the following years ‡, on which I desire to offer some observations; especially as the mode of exposition of the dispersive action of a prism which was adopted by me several years ago § has been the subject of criticism in the papers by Schuster and Ames, to which Lord Rayleigh refers at the beginning of his paper.

The first part of the following remarks would doubtless bear condensation, as the full force of Lord Rayleigh's comparison of the dispersion problem with that of a travelling maintained source had not been grasped when they were written; but in a subject which so largely turns on the mode of expression, condensation might involve obscurity. There

* Communicated by the Author.

† Phil. Mag. Oct. 1905, p. 401.

‡ Cf. especially 'Wave Theory' § 7, Ency. Brit. 1888; Scientific Papers, vol. iii.

§ 'Æther and Matter,' 1900, chap. xv.

does not, however, seem to be in what follows anything that stands in definite contradiction with Lord Rayleigh's published views.

That there is room for still further precision of terminology in this subject is indeed suggested by the beginning of Lord Rayleigh's paper. It would seem that a definite choice can be made between the two modes of exposition, both of which he considered to be allowable. According to the first, "The assertion that Newton's experiments prove the colours to be already existent in white light is usually made in too unqualified a form." On a first impression this remark might be imagined to strike at the roots of all the various instrumental methods that have been elaborated for analysing complex radiations; for if the analysis brings out features that are not already existent in the radiation, two different methods of analysis (*e. g.*, by a grating and by a prism) can hardly be expected to give concordant results.

The alternative mode of exposition is to say that each complex type of radiation is constituted definitely of those colours (simple trains of various wave lengths), into which the Fourier mathematical analysis would divide its vibration-curve: and that various analysing instruments (gratings, prisms, &c.) are capable of revealing this constitution with different amounts of precision, the outstanding differences between these analyses being treated as due to imperfections of the instruments as regards the purpose in question. This (the usual) point of view is claimed as a valid alternative in Lord Rayleigh's third paragraph: to hold, as is done *infra*, that in some cases the resulting analysis is so imperfect as to be valueless, need not disturb the general validity of this point of view.

So far, the matter is one as to the most suitable mode of theoretical description or formulation. But we presently reach questions on which opinions may perhaps differ as to physical fact. In the spectral analysis of ordinary continuous radiation, the prism and the grating give consistent results, when well understood corrections and adaptations are applied before making the comparison. If these instruments are applied to radiation consisting of a system of sharp, entirely uncoordinated, discrete pulses, such as the Röntgen rays are supposed to be, will this general agreement continue? It is clear that the grating (if of ideal perfectly reflecting quality) will draw out each pulse into a spectrum, and thus will analyse the radiation. It seems open to question whether a prism will not merely gradually dissipate it by scattering, however wide the pulses may be, even if they are of breadth comparable with the wave-lengths of visible light. If this

be so, the prism is very badly suited for the analysis of this type of radiation, and no amount of adaptation of the result will bring the prism into conformity with the ideal grating.

The Fourier mathematical process*, as also the ideal grating which reflects back the disturbance in *échelon* so to say, operates by simply selecting and piecing together elements existing in the original radiant disturbance, so as to isolate periodic wave-trains that on superposition would reproduce the form of the original vibration-curve.

On the other hand, what the prism may do to a given isolated pulse would seem to depend on its own constitution. The customary mode of investigation would be to replace the pulse by the equivalent infinite system of component Fourier wave-trains, to find the effect produced on each of them by substituting its expression in the differential dynamical equations of the dispersive medium, and to add the results thus found. Though the original Fourier expansion of the pulse is always analytically legitimate and definite, it is not always allowable, without scrutiny as to convergency, thus to operate on its separate terms and add. Indeed, the particular component waves whose period is a free period of the medium would increase infinitely in importance in the result: thus it must be ascertained whether this infinity of intensity is more than compensated by infinite smallness of the element of period over which it ranges, before the procedure which includes it can be accepted as mathematically legitimate. The insertion of a very small frictional term in the dynamical equation will, however, secure that the component vibration remains finite though great at this critical period, and the analysis then becomes entirely valid. But the problem is only shifted; it has now to be ascertained whether the limit which this solution approaches as the friction is reduced indefinitely is the same as the solution previously arrived at in the absolute absence of friction,—whether in fact there exists a definite limit. That there is in many cases no definite limit is merely another way of expressing the theory of anomalous or selective dispersion, in which the final steady result depends essentially on the magnitude of the small viscous term that must be introduced in order to evade infinities, while the mode of the gradual establishment of that result is likewise undetermined. The question thus arises, whether the proportion of the energy of an incident isolated pulse that goes into this selective vibration is capable of determination by operating analytically upon its Fourier analysis in this way,—whether, in fact, a different line of

* Cf. 'Æther and Matter,' § 162.

attack would not be required in order to determine it. At any rate, such questions of mathematical validity can arise only in regard to the presence of selective or anomalous dispersion*.

The argument of Prof. Schuster (*Phil. Mag.* Jan. 1904, p. 6 †) arrives at the conclusion that a single pulse is split up regularly into a spectrum by a prism. It appears to start from an implied hypothesis that even an abrupt pulse travels unchanged across the dispersive medium, with the velocity appropriate to a group of waves. If for a pulse is substituted a train of waves with wave-lengths variable within the narrow limits λ and $\lambda + \delta\lambda$, so that the train is very nearly simple harmonic, this statement will be sensibly exact except near the beginning and end of the train: and Prof. Schuster's representation of the emergent radiation, as consisting of groups of waves, most concentrated in the neighbourhood of surfaces which are oblique to the wave-fronts, then affords an instructive view of the process of dispersion, whether prismatic or diffractive. But if this argument is to be pressed so as to include a single sharp pulse, what value of λ are we to take as applicable to it? The theorem of definite group-velocity is demonstrated only for the compound disturbance arising from the superposition of simpler trains of some common type but with slightly differing parameters,—the trains being unlimited and of simple harmonic type in the usual Stokes-Rayleigh theory. A single pulse will thus not have any definite group-velocity with which it can travel; or, what comes to the same, the different parts of it will travel in the dispersive medium with widely different velocities, so that it will spread out and be dissipated. An argument which assigns a definite velocity to a complex disturbance can thus be applicable only to very special types of disturbance ‡: for them it must of necessity lead to the same

* I fear that I have on previous occasions orally assigned to them a wider importance. It is only the fate of the constituent wave-trains that are near the free period that is undetermined.

† The paper, as its title indicates, is concerned mainly with a brilliant application of groups of undulations to the instantaneous explanation of Fox Talbot's interference bands. In connexion with § 6 it may be remarked that the dynamical relations require that a limited disturbance, travelling in a transparent medium, must consist of compensating positive and negative parts.

‡ Formation of the differential equation for the forms of disturbances that are propagated without change of type shows that, when simple wave-trains possess this property, there are in general no other solutions; the existence of a wave-group in fact implies the existence of the wave-train through which it travels.

law of prismatic dispersive power as holds by the same argument for a disturbance consisting of uniform trains of simple waves, if the average wave-length of the latter corresponds to this group-velocity.

As these considerations relating to the mode of propagation of pulses apply to both Prof. Schuster's and Prof. Ames's arguments, it will not be superfluous to fortify them by the following quotation from Lord Kelvin*, written in connexion with the features exhibited near the beginning and end of a regular gravitational train of surface-waves travelling on deep water:—"Our present solution shows how rapidly the initial sinusoidality of the head and front of a one-sided infinite procession, travelling right-wards, is disturbed in virtue of the hydrokinetic circumstances of a procession invading still water. Our solution, and the item towards it represented in figs. 6 and 7, and in fig. 2 of § 6 above, show how rapidly fresh crests are formed. The whole investigation shows how very far from finding any definite 'group-velocity' we are, in any initially given group of two, three, four, or any number, however great, of waves. I hope . . . to return to this subject in connexion with the energy principle set forth by Osborne Reynolds, and the interferential theory of Stokes and Rayleigh giving an absolutely definite group-velocity in their case of an infinite number of mutually supporting groups."

It would appear then that there is no certain ground, on the basis of the ideas pertaining to group-velocity, for concluding that a prism is competent to disperse any isolated æthereal pulse, or any series of pulses with absolutely irregular statistics, into a series of simple wave-trains, in a regular manner †, as an ideal grating could do, the number of undulations in each train being in that case the number of rulings in the grating or a sub-multiple thereof.

But Lord Rayleigh in his recent paper has thrown fresh light on the subject of the general action of dispersive media, by examining the disturbance that follows an impressed travelling aperiodic pulse, *maintained at constant intensity*, and showing that such a pulse imitates in some respects closely the behaviour of a wave-train ‡. He in fact points out the analogy with the surface-waves produced by a boat travelling with uniform velocity on a lake, which, as everybody has

* Kelvin, "On the Front and Rear of a Free Procession of Waves in Deep Water." *Phil. Mag.* vol. viii. 1904, p. 468.

† What applies to a prism would probably also apply to colour-perception by the eye.

‡ A more direct investigation than that quoted from Lord Kelvin's note of 1877 is given in Prof. Lamb's 'Hydrodynamics,' 1895, § 227.

observed, are dispersed into simple wave-trains each travelling in its own appropriate direction. It would seem indeed that this illustration bears more closely on the action of a travelling *source* impressed on the medium than on the fate of an unsupported pulse travelling across it spontaneously. In default of a constant supply of power to the boat, to be spent in making new waves, it would soon lose its velocity unless it had a store of kinetic energy great for its size. Thus this close analogy with ordinary dispersion, which is afforded by the dispersed wave-trains excited by a pulse, impressed and maintained from outside, appears to leave where it was the question of the fate of an isolated unsupported pulse, propagated into the dispersive medium and then left to itself. The steadiness which in the ordinary dispersion-theory arises from the succession of fresh waves of the train, is obtained in the illustration above by the maintenance of the energy of the pressural source, with results in close analogy in the two cases.

It thus still seems difficult to evade the force of the argument of Sir George Stokes* :—"When you let a ray of light fall on a refracting medium such as glass, motions begin to take place in the molecules forming the medium. The motion is at first more or less irregular; but the vibrations ultimately settle down into a system of such a kind that the regular joint vibrations of the molecules and of the ether are such as correspond to a definite periodic time, namely that of the light before incidence on the medium. That particular kind of vibration among the molecules is kept up, while the others die away, so that after a prolonged time—the time occupied by, we will say, ten thousand vibrations, which is only about the forty thousand millionth part of a second—the motion of the molecules of the glass has gradually got up until you have the molecules of the glass and the ether vibrating harmoniously together. But in the case of the Röntgen rays, if the nature of them be what I have explained, you have a constant succession of pulses independent of one another. Consequently there is no chance to get up harmony between the vibrations of the ether and the vibrations of the body."

The distinction may perhaps be put more definitely. White light from an incandescent solid is made up of a vast number of pulses arising from the molecular shocks incessantly occurring in the hampered spaces to which the molecules are confined. On the other hand, the Röntgen rays are made up

* Wilde Lecture "On the Nature of the Röntgen Rays," 1897. Math. and Phys. Papers, v. p. 271.

of the independent sporadic shocks transmitted through the æther from the impacts of the separate and independent cathode particles. Both kinds of disturbance are resolvable by Fourier's principle into trains of simple waves. But if we consider the constituent train having wave-length variable between λ and $\lambda + \delta\lambda$, *i. e.* varying irregularly from part to part of the train within these limits, a difference exists between the two cases. In the case of the white light the vibration-curve of this approximately simple train is in appearance steady: it is a curve of practically constant amplitude, but of wave-length slightly erratic within the limits $\delta\lambda$ and therefore of phase at each point entirely erratic. In the Fourier analysis of the Röntgen radiation the amplitude is not regular, but on the contrary may be as erratic as the phase. The origin of this difference is that the body radiating the white light is presumably so far in a steady state that each element of it has a definite temperature at each instant, which implies a statistical uniformity in the vibratory disturbance that is emitted. Or, approaching the subject from the side of the thermodynamics of radiation, each elementary constituent wave-train, say that corresponding to the interval $\delta\lambda$ above, has its own temperature which it carries permanently along with it, the same as the temperature of its source supposed a perfect radiator. This temperature is at each point of it a function of the energy-density, and therefore of the amplitude of the radiation. If the amplitude were different along two reaches of the train, the reach of higher amplitude could be in equilibrium of spontaneous exchanges of energy with a perfect radiator of higher temperature than its own source, and ideal automatic arrangements involving intensification of the energy would be possible, in opposition to Lord Kelvin's fundamental principle of degradation. In the internal equilibrium to which a material system nearly instantaneously settles down, in acquiring a definite temperature for each element of its mass, such differences of amplitude must thus have disappeared: the state of uniform amplitude is, in fact, the most probable one*.

In the distinction which is here suggested, the average degree of suddenness of the Röntgen pulses is not involved. That would still be capable of estimation by experiments on diffraction of rays travelling in free æther, in the manner of Haga and Wind. But no physical *rationale* of prismatic dispersion† except the influence of the vibrations excited in the material system seems now to be entertained; and this

* It is hoped to pursue this idea in another connexion.

† Propagation in limited systems such as bars is not to the point.

appears to impose a limit to the types of disturbances that are subject to regular dispersion.

Even in the case of wave-trains excited on the surface of water by a travelling source, where, as Lord Rayleigh remarks, there is no structural periodicity *, the presence of the wave-trains travelling in any direction does, at any rate, depend on the persistence of free periodic trains of waves. If we imagined the water replaced by a medium in which no free wave-trains could travel with velocities within certain limits, then we would expect a gap in the wave pattern formed by the travelling source, corresponding to those limits. The passing remark of Sir George Stokes, which likens the synchronous optical vibration of a transparent solid body to the sonorous vibration of the sounding-board of a pianoforte, thus assigning it to regions of the material medium in bulk rather than to its individual molecules †, would bring the optical effect somewhat nearer to the water-wave phenomenon.

But the feature in the case of the water-waves to which Lord Rayleigh doubtless intends to draw special attention, is the absence of gradual initiation and delay of effects ; as soon as the source begins to move with uniform velocity, the wave-pattern begins to travel out from it, and as soon as motion of the source stops, so does the formation of the wave-pattern. By a legitimate application of the principle of group-velocity, the number of undulations formed is shown to be connected with the distance over which the source has moved ; just as the number of waves formed from a single freely travelling pulse by a grating is determined by the number of its lines over which the pulse has travelled.

At first sight, as above stated, it is difficult to detect sufficient similarity to the optical case. But if we consider (with Lord Rayleigh, p. 404) a thin plane pulse incident from free æther obliquely on an infinitely extended plane face of a refracting medium, the intersection of the pulse with the face will be just such a maintained disturbance, travelling

* Prof. Schuster (Phil. Mag. Jan. 1904) puts the point as follows :—
“As we may imagine continuous media of such elastic properties as to give dispersion, the true explanation must be independent of the sympathetic vibrations,” on which I had relied in ‘Æther and Matter,’ p. 248. The force of this is obvious: yet, when friction is ruled out, what can there be, as a matter of fact, except conspiring periodicities in time (free periods) or space, to modify simple elastic waves, which travel without change, into the dispersive type?

† In the case of a vapour, the molecules, being isolated, must however operate independently.

along the face, as we require. If the medium has the dispersive quality fully developed, for all disturbances however sharp, *i. e.* if the differential equation determining dispersive vibration has no limits to its full application to such disturbances, the resolution into trains of waves must be granted as a necessary consequence of the analysis for a steady travelling source.

This *rationale* of the dispersive refraction, at a plane surface, of an obliquely incident thin plane pulse, is one of those obvious things that when once grasped form a permanent addition to our stock of physical imagery*. One can picture its application to water-waves. A tract of water may be imagined, of small uniform depth h , in which therefore all waves travel with the same velocity $\sqrt{gh}$, separated from a region of deep water, in which the velocity of a train depends on its wave-length, by a straight boundary. A disturbance consisting of a thin plane ridge can advance obliquely towards the deep water without change of form; the successive parts of the ridge reach the boundary in the manner of a maintained local disturbance running along the boundary with uniform speed, of which a definite fraction is transmitted across into the deep water, the rest being reflected back. The mode of this transmission is, by symmetry, the same as if that part of the disturbance were doubled and the deep water were unlimited on both sides: regular wave-trains are shed off dispersively in the different directions, as in the case of the boat described above, with wave-lengths such that their velocity can just keep up with the travelling source, while the distribution of intensities between the various directions depends on the character of the moving source, *i. e.* of the incident travelling ridge of water. The waves must, in fact, form a steady pattern travelling with the source: thus the velocity of free propagation of the component train travelling in any direction must be the component in that direction of the velocity of the source. But as the travelling source has finite size, this component train, though nearly homogeneous in wave-length, is not quite so; being nearly homogeneous, the dispersive quality of the medium will make its waves travel in groups, which progress in the known manner with only half the velocity of their component waves. Thus after the train is well formed, the groups of disturbance will recede from the

* Lord Rayleigh considers this explanation of the *refraction* of a pulse into a dispersive medium to be less simple than his first case of the propagation of a plane pulse in such a medium; but owing to the difficulty described above, regarding the maintenance of such a pulse, I have failed to appreciate the argument in that case.

source with a velocity equal to the difference of these two velocities, and Lord Rayleigh's determination of the length of disturbance emitted in a given time ensues,—subject, however, to the reservation quoted above from Lord Kelvin as regards the extreme head of the train. The account of the process, which is indicated by Lord Rayleigh, seems in its essentials to be fully verified.

Yet the quotation above made from Sir George Stokes, in which the imagery is optical instead of hydrodynamical, appears to show a different aspect of the picture which we are bound to follow out ; though Lord Rayleigh has guarded himself against it by his reservation, “so long at least as we are content to take for granted the character of the dispersive medium—the relation of velocity to wave-length—without inquiring further as to its constitution.” The postulate thus indicated is that the partial differential equation of propagation is to hold true without limitation. This implies that the dispersive medium must be homogeneous in space ; if it had minute alternating structure, then this differential equation could not of course be applied without modification to waves of length comparable with the dimensions of that structure,—a circumstance on which Cauchy reared his original attempt at an explanation of optical dispersion. But it requires also that the medium should, so to speak, be homogeneous in *time*. An optical dispersive medium is made up of elements which have periods of free vibrations of their own, that are more or less durable ; the differential equation will not hold for disturbances whose scale of duration is so small as to be of the same order as the time of the natural subsidence of free disturbance among the elements of the medium. In connexion (originally) with the dynamical theory of viscosity in gases, Maxwell introduced the term *time of relaxation* to express the time, roughly assignable, that it would take for a local derangement of the molecules of the medium to smooth itself out. In optics it is the time needed for the free irregular vibrations of an element of the medium, produced by a local shock or other disturbance, to die out by dissipation into surrounding elements. The theory of regular dispersion of a disturbance into wave-trains caused by refraction, re-stated above for the hydrodynamic case of waves on water, cannot be applied in the optical case unless the scale of duration of the disturbance is long compared with the *time of optical relaxation* of the dispersive medium. In Sir George Stokes's illustration, taken at a venture, the time of relaxation would be ten thousand times the period of a light-wave ; if so, regular refraction and dispersion would

hardly be established for sequences of less than ten thousand similar waves. Perhaps the only means of even roughly guessing at the time of optical relaxation is by the time-lag in such phenomena as fluorescence, which are connected in part with free internal vibrations excited in the elements of the medium. Stated in the present form, the criterion that a Röntgen æther-pulse should be regularly refracted and dispersed into wave-trains, according to a process of which Lord Rayleigh's *rationale* has been paraphrased above, is that its duration should be long compared with the time of optical relaxation of the dispersing medium. In the hydrodynamic illustration the restriction does not arise, for the time of molecular relaxation is far beneath the period of any observable surface-waves.

To sum up, it now seems clear that Lord Rayleigh's application of the phenomena of a *maintained moving source* gives an adequate picture of the *modus operandi* of the dispersion of an incident aperiodic disturbance into regular wave-trains by refraction, for all types of disturbance that are slow compared with the period of natural molecular relaxation of the refracting medium,—provided, however, anomalous dispersion, which cannot be included unless a *quasi-frictional* term is *assumed* in the analysis, plays a part which is unimportant. But it is still held to be unlikely that æthereal pulses of the type of the Röntgen rays come as a rule within this limit. If this be so, white light, such as can be regularly dispersed by a prism, cannot consist of wholly irregular æthereal disturbance; each Fourier component, comprised within say the infinitesimal range of wave-length between λ and $\lambda + \delta\lambda$, must have sequences of regularity in its amplitude, of duration comparable with the time of optical relaxation of the dispersing medium.

Cambridge, October 10, 1905.

LXIV. *On the Emission of Negative Corpuscles by the Alkali Metals.* By J. J. THOMSON, M.A., F.R.S.*

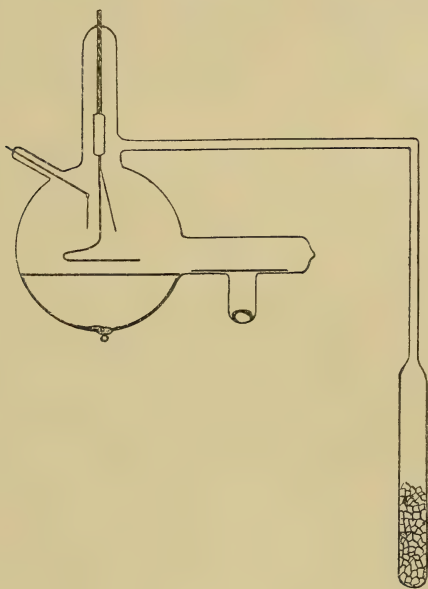
IT is well known that the alkali metals when exposed to light give out negative corpuscles, even when the light is of very feeble intensity. Thus Elster and Geitel found that the light emitted by a piece of glass rod heated to a dull red heat was sufficient to make rubidium emit corpuscles. It has not, however, as far as I am aware, been noticed that

* Communicated by the Author.

with these metals there is a small emission of corpuscles, even when all external light is excluded. The following experiments show, however, that rubidium and the liquid alloy of sodium and potassium give out corpuscles in the dark.

The experiments showing this were made as follows:—A gold-leaf electroscope with quartz insulation was enclosed in a glass vessel, which was exhausted to an exceedingly low

Fig. 1.



vacuum by means of charcoal cooled to a very low temperature by liquid air in the way discovered by Sir James Dewar. The rubidium or Na-K alloy was placed below the gold leaves of the electroscope, care being taken to have the surface of the metal as clean as possible; the metal was earthed by means of a wire fused through the glass. The vessel was placed inside a box made light-tight by means of felt, the tube containing the charcoal which protruded from the box was painted over with lamp-black. To measure the divergence of the leaves of the electroscope, these were momentarily illuminated by a faint light transmitted through a red glass window, the position of the leaves was deter-

mined by a reading microscope passing through the side of the box ; after a reading had been taken, shutters were put before the window and the eyepiece of the microscope covered with a cap. The readings only took a second or two, but even in that time the red light produced an appreciable leak in the electroscope ; this part of the leak can, however, easily be separated from that taking place in the dark as the latter is proportional to the interval between two readings, while the former is independent of this interval.

To test the efficiency of the means taken to exclude the light, some of the experiments on the rate of leak were made in a photographic dark room, others with the case exposed to the light of the laboratory ; the leak was the same in the two cases. Another proof of the exclusion of light is that a sensitive photographic plate placed in the case for 48 hours was not fogged.

When the leaves of the electroscope were charged with *positive* electricity there was, even in the dark always, a small leak of electricity from the leaves, while there was no perceptible leak when the leaves were *negatively* charged. The positive leak was entirely stopped by a transverse magnetic field : this proves that it is due to negative corpuscles emitted by the rubidium or the alloy of Na and K.

On some occasions the positive leak was abnormally large. This was traced to the presence in the tube containing the electroscope of minute quantities of hydrogen ; it was found that this gas had an extraordinary influence on the emission of corpuscles from electropositive metals. To investigate this more fully, an arrangement was added to the glass vessel containing the electroscope, by which bubbles of hydrogen could be admitted from time to time into the vessel. The admission of a very small quantity of hydrogen produced temporarily a very large increase in the rate at which electricity escaped from the positively charged leaves of the electroscope, the rate of leak after the admission of the hydrogen being often ten times its previous value. The increase in the leak rapidly died away, and after about 20 minutes the leak resumed its original value ; the admission of a fresh supply of hydrogen, however, sent it up again. The admission of small quantities of air or carbonic acid did not produce any appreciable increase in the leak. It would appear that while these electropositive metals are absorbing hydrogen, the rate of emission of negatively electrified particles is greatly increased.

The influence of hydrogen on the emission of corpuscles

from a hot platinum wire has been observed by Dr. H. A. Wilson ; the increase in this case is very much greater than that with cold alkali metals.

In a previous paper (*Proc. Camb. Phil. Soc.* xiii. p. 49) I showed that the radioactive substances radium and polonium emit when cold slowly-moving negatively electrified corpuscles. The experiments just described show that this property is also possessed to an appreciable extent by substances not usually regarded as radioactive. With more delicate apparatus than that used in these experiments it is probable that this property might be detected in all substances :—I tried in my apparatus, in addition to the alkali metals, lead, silver, and mercury, but could get no indication of the emission of corpuscles by these metals.

The alkali metals give out corpuscles when in the gaseous as well as in the solid state ; this was proved in the following way. If the gaseous atoms of sodium give out negative corpuscles, the atoms themselves will be positively charged and so will be attracted by negatively electrified bodies : this was found to be the case. Sodium was heated in a highly exhausted flask, the cooler part of which contained two glass tubes down which ran wires. The wire in one tube was connected to the positive pole of a battery of small storage-cells giving a potential-difference of 600 volts, the wire down the other tube was connected with the negative pole of the same battery. When the bottom of the flask was heated, the sodium evaporated and condensed on the two tubes. When the wires down these tubes were disconnected from the battery, the deposit of sodium was pretty equally distributed between the tubes. When, however, the wires were connected with the battery, there was very little deposit on the tube connected with the positive pole, while the deposit on that connected with the negative pole was very dense. If the connexions with the battery were reversed, the sodium began to deposit on the tube which had previously been clean, while hardly any increase took place in the deposit on the other tube ; showing that the effect was due to the electrical charges on the tubes, and not to any want of symmetry. In these experiments the tubes were not absolutely dark. This deposit of sodium on a negatively electrified surface may be compared with the flow of radium emanation to a negatively electrified wire.

The result at which we have arrived from the preceding experiments, that some substances emit many corpuscles, while others at the same temperature only emit few, if any,

has important consequences when considered in relation with the Second Law of Thermodynamics. For, consider an enclosure at a constant temperature containing two substances in electrical connexion, one A giving at this temperature a copious supply of corpuscles, the other B few, if any : we see that we could utilize the stream of particles from A so as to do mechanical work. But since everything is at the same temperature, it follows from the Second Law of Thermodynamics that the energy required for this work cannot be derived from a lowering of the temperature of any part of this enclosure ; it cannot come from thermal sources, but must come from some change in the state of the working substance, presumably from some diminution in the internal energy of the atoms of this substance. Investigations made with the object of seeing whether prolonged emission of corpuscles, such as might be produced by long-continued incandescence, produces any appreciable effect on the properties of the subject, might be expected to give interesting results. There are undoubtedly changes produced in a substance such as a piece of platinum wire by long-continued incandescence, but we do not know as yet whether these changes are such as indicate a change in the platinum atom, or whether they are merely physical, such, for example, as would result from the expulsion of gases absorbed by the wire. Again, many metals after the emission of corpuscles by exposure to ultra-violet light show "fatigue," *i. e.*, the rate of emission of corpuscles after long exposure becomes less than it was initially : this is usually ascribed to the formation or removal of films of gas or to a roughening of the surface ; it is possible, however, that it may partly be due to some change in the metal itself. Investigations on these points would be of especial interest because, if the energy of the corpuscles does come from changes in the atomic energy, we have here a case in which this transmutation of energy can be started and influenced by external conditions, such as incandescence or the incidence of ultra-violet light or Röntgen rays.

On this view, the energy of the corpuscle emitted is not derived directly from the work done on the corpuscles by the electric field which exists in the Röntgen rays or in the light. The rays act as detonators, causing some of the atoms on which they fall to explode, and the energy of the corpuscle is derived from the energy liberated by this explosion.

In the case of radium and other radioactive substances we have probably also the transformation of internal atomic

energy into the kinetic energy of corpuscles and α particles, but in this case, as far as is known, the transformation is quite uninfluenced by external physical conditions, and is thus beyond our control. If, however, the view we have been considering above is correct, the tapping of the internal atomic energy by corpuscular streams is to some extent under our control and can be brought about by elevation of temperature or by ultra-violet light.

Since the emission of corpuscles goes on to some extent at all temperatures, and since inside a body the energy of these corpuscles would ultimately be transformed into heat energy, there is probably a continual transformation of internal atomic energy into heat: this would cause the interior of a mass of metal to be hotter than the surface, the increase in the inside temperature depending on the amount of energy transformed, on the size of the body, and on its thermal conductivity.

If the body is a sphere of radius a , of uniform composition, we can easily show that θ , the difference between the temperature of the surface and that at the centre, is given by the equation

$$\theta = \frac{qa^2}{6k},$$

where q is the amount of energy transformed into heat per cubic centimetre per second, k the thermal conductivity of the substance.

For bodies comparable in size with the earth, a very small amount of transformation of internal atomic energy into heat would produce very large differences of temperature between the centre and the surface. Thus, if the conductivity of the sphere were $\cdot 01$, which is about three times that of granite at the temperature of the earth's surface, there would be a difference of 3000°C. between the centre and surface of a sphere the size of the earth if $q = 45 \times 10^{-17}$, i. e., if the atomic energy transformed into heat per c.c. in 100 million years were less than that required to raise the temperature of $1\cdot 5$ gramme of water 1°C. If the corpuscles were emitted with a kinetic energy corresponding to that which would be acquired by the fall of their electric charge through two volts, the emission by an atom of a corpuscle once in a thousand million years on an average would be far more than sufficient to produce the required transformation of energy. The temperature difference between the centre and the surface being proportional

to the radius of the sphere, leads us to expect that with bodies of the size we could manipulate in the laboratory the differences of temperature would be exceedingly small unless the emission of corpuscles was very copious. It would, however, be interesting to test whether the inside of a block of lime which, as Wehnelt has shown, at high temperatures emits large quantities of corpuscles, is at such temperatures appreciably hotter than the outside.

If there is a continual transformation of the internal energy of the atoms into other forms of energy when the atom is emitting corpuscles, we should expect that the internal energy of an atom would vary with the treatment it had received; that it would have been more diminished if the atom had been maintained for long periods in a state of incandescence than if it had been kept cool; we should thus expect the internal energy of an atom of an element in the sun to differ from that of the same atom on the earth: if this is so, then this variation in the internal atomic energy must be without effect on some of the properties of the atom. Thus, for example, spectrum analysis shows that the periods of vibration of an atom in the sun do not differ appreciably from those of the same element on the earth; we have indeed at present no evidence of the existence of any difference in the properties of atoms of the same element. We can, however, easily conceive an atom constructed in such a way that before the internal energy had diminished sufficiently to appreciably alter many of its properties, the atom would become unstable and explode, breaking up into atoms of elements of a different kind. Suppose, for example, that the atom consists of a number of corpuscles arranged in layers on the surfaces of concentric shells, and that the loss of internal energy by the atom is mainly due to the loss of kinetic energy by those corpuscles in the outer layer, this will hardly affect the times of vibrations of the corpuscles inside, while the outer layers may lose such a large amount of energy that their configuration becomes unstable, and the corpuscles in the outer layers rearrange themselves: in doing this, such a large amount of kinetic energy may be liberated that the atom explodes and breaks up into atoms of different kinds. Thus, in a case of this kind we should have the atom losing internal energy and yet as long as it remained intact the great majority of its periods of vibration would be unaltered, and the atom would explode before the change in its internal energy was sufficient to appreciably affect the great majority of its properties.

LXV. *A Repetition of Fizeau's Experiment on the Change produced by the Earth's Motion on the Rotation of a Refracted Ray.* By D. B. BRACE*.

THE following experiment was undertaken for the purpose of re-examining the results which Fizeau † announced he had obtained, nearly fifty years ago, in observing a change in the azimuth of a plane-polarized ray when refracted through a plate of glass, respectively along and against the earth's motion through space.

The results of my observations, made under similar conditions, show that the effects which Fizeau obtained must have been due to other causes than those whose effects he supposed he was examining. This conforms to that which we should expect from the results of other experimenters, examining the problem by other modes of experimentation, and also to the prevailing opinion that the results of Fizeau's observations were somewhat doubtful. In fact it appears that he has so expressed himself in this matter ‡. Notwithstanding these facts, reference is still made to the positive results of this experiment, which are the only ones ever obtained by experimental means on the problem of the "æther drift."

It would have been highly desirable if Fizeau himself could have repeated his experiments long ago. Perhaps, however, the evidence from other sources did not warrant this; but the undoubted care and skill devoted to this experiment have left an impression that, perhaps, after all the test involved surface conditions which did not enter in the other experiments and which might give the positive results obtained.

Experience in several similar problems in polarization recently has led me, incidentally, to believe that it would be entirely possible to repeat this experiment with sufficient sensibility to give conclusive results. Two principal factors have made this possible:—first, a sensitive-strip plane polarizer §; and, second, the use of a "crossed" system of thin refracting plates. This system reduced the double refraction to a minimum, and also compensated for the colour dispersion that Fizeau had to contend with, thus allowing me to use white light with, consequently, a higher sensibility in the half-shade system.

* Communicated by the Author.

† *Ann. de Chim. et de Phys.* (3) lviii. p. 129.

‡ Lorentz, *Versuch einer Theorie*, Leiden, 1895, p. 2.

§ *Phil. Mag.* Jan. 1903, p. 161.

Fizeau used a combination of elements to compensate for the dispersion and the unavoidable double refraction and, also, to magnify the expected rotation of the ray. He further readjusted the beam of sunlight, which he used, on reversing the system, and this seems to have given him variations in his readings. These possible sources of spurious effects have been eliminated in the arrangement which I used.

His experiment is based on Brewster's law for a refracted ray, namely,

$$\tan \beta = \frac{\tan \alpha}{\cos (i-r)},$$

where i and r are the angles of incidence and refraction and α and β the corresponding azimuths, respectively, of the plane-polarized ray. Any change in the index would indicate itself by a variation in the angle r and, hence, in the azimuth β . Fizeau usually used, for producing the effect, two so-called rotary piles, each containing four slightly prismatic plates of glass 1 to 2 millimetres thick, and mounted slightly inclined to one another. In his experiment the incidence of the ray upon these piles was 70° , and the azimuth of the polarized beam, usually 20° , sometimes 30° . He also generally used two piles, at a smaller inclination, for compensating the elliptical polarization due to the double refraction in the rotary plates. For compensating the rotary dispersion, he inserted during a part of his observations a natural rotary substance, such as quartz, or some of the essential oils. Finally, to increase the effect, he inserted several "amplifying" piles which magnified the effect some 30 times. His polariscope consisted either of crossed nicols, an interference system, or the sensitive-tint single or biquartz systems. Such a complicated arrangement necessarily caused a great loss of light and thus reduced his sensibility.

In my preliminary experiments I adopted a similar arrangement to that of Fizeau. In order to avoid as far as possible the disturbing factor of double refraction, ten plates were cut from well-annealed optical glass $n_D = 1.5178$. These plates were ground wedge-shaped so that the internally reflected images would not enter the field of view. They were 20 mm. long and 15 mm. wide, and their faces made an angle with one another of approximately $50'$ in the direction of their greatest lengths, their thickness diminishing from 0.6 mm. to 0.3 mm. These plates were set on rings, upon which they could turn, whose planes made an angle of 70° with the vertical plane, in which plane a second ring, carrying the first ring, could be made to rotate. Thus, with an

incidence of 70° , the azimuth of the plane of polarization of a ray could be varied at will. These were then mounted, one at a time, between the polarizer and the analyser. Starting with an azimuth of 45° , the analyser was adjusted for minimum intensity, being shifted to receive the light axially, since, on inserting the wedge, the direction of the ray was changed. A second plate and its mounting was then inserted and adjusted so as to receive the ray from the first one in an azimuth of 45° , the wedge-shaped plate being turned in its own plane upon its ring-carrier until the multiply reflected images were thrown out of the field of view. This necessitated a somewhat spirally-shaped arrangement of the system when the analyser and all the plates were finally mounted. A thin strip mica compensator* was placed after the polarizing half-shade system, to compensate for any slight amount of elliptic polarization which might be produced by the plates themselves. This was quite effectual and, with three or four plates, moderately close settings could be made with the plane-polarizing half-shade system. However, with white light, the colour dispersion due to the rotation prevented anything like the sensibility required by the experiment. I attempted to compensate for this by means of quartz, as did Fizeau, but still found that, with white light, the sensibility was far too small, I then tried sunlight, using one of Wülfing's spectral "sifters" to obtain homogeneous light. But here my intensity was too low. A powerful mercury lamp gave similar results. I then saw that white light and complete compensation of the dispersion, due to the rotation of the plates (this rotation was approximately through one quadrant), would be necessary to attain the required sensibility. Only moderate pains had been taken in grinding the plates, and I found that, with all ten plates in, the astigmatism was so large that the definition of the "half-shade" was greatly reduced. I therefore decided on a different arrangement which, though apparently unlike that of Fizeau, involved the principle of his experiment and, hence, would be valid in a repetition of the test. This consisted in combining pairs of plates into the "crossed" system mentioned above. Thus (fig. 1) if in the equation

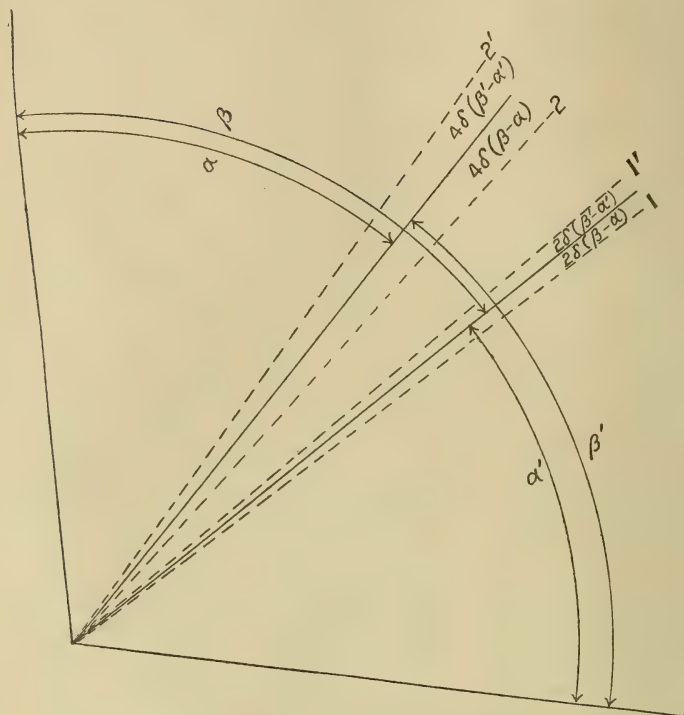
$$\tan \beta = \frac{\tan \alpha}{\cos (i-r)},$$

$(\beta - \alpha)$ is the rotation experienced by a ray after refraction at a single surface, $\delta(\beta - \alpha)$ would be the increment, say, due

* Phys. Review, Feb. 1904, p. 70.

to the effect of the earth's motion upon the velocity of propagation in the glass. The total increment for a single plate would be $2\delta(\beta - \alpha)$ approximately. If now the ray be reflected back through the second plate, in the same azimuth

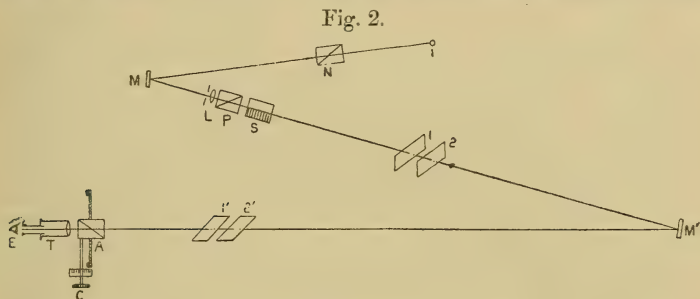
Fig. 1.



with respect to the incident ray, it would experience similarly a rotation of $2(\beta' - \alpha')$; and, since its direction is reversed with respect to that of the motion of the earth, it would experience a *decrement* of $2\delta(\beta' - \alpha')$. Hence the azimuth of the ray will appear to an observer to have changed by an amount equal to $4\delta(\beta - \alpha)$, approximately, for the pair of plates, if $\alpha' = \alpha$ and $\beta' = \beta$. For a second pair of plates, in series with the first, the change would be $8\delta(\beta - \alpha)$, and so on. The full lines in the figure show the azimuths of the ray, without the effect of the earth's motion, and the numbered dotted lines, those with the supposed changes in the planes of polarization. Thus, line 1 is the new azimuth after passing the first plate, and line 2 the new azimuth after returning through the second plate of the pair. Similarly, if the

system is reversed in the direction of "drift," line 1' would be the azimuth after the first plate and line 2' after returning through the second plate. Thus, the total change in the azimuth is equal to the angle between 2 and 2', and so on for any number of pairs. It is evident, from the figure, that, neglecting the effect of "drift," rays of all colours will emerge from the system with their absolute directions of vibration the same as when incident on the first plate. Hence, if we start with our nicols crossed, the insertion of these pairs of plates will not affect the final direction of vibration of the rays, and we shall have perfect compensation of the rotation, as well as of the rotary dispersion. Elliptic polarization may of course occur to a greater or less extent, due to imperfect annealing.

The arrangement, as finally adopted to utilize these various ideas, is shown in fig. 2. I is the radiant (a Nernst filament



of 110 volts), N is a nicol for polarizing the light before entering the half-shade system, in order to reduce the diffused light arising from internal reflexion within the polarizer P proper of the half-shade system PS, M and M' mirrors silvered on the front surface, L a lens forming a conjugate image of the radiant I on the analyser A, S the sensitive-strip, 1, 2 and 1', 2' the two pairs of wedges, T a small telescope of 2 or 3 diameters, and C the reading circle carrying the analyser and provided with a micrometer-screw. The sensitive-strip S, which I have already described*, consisted of a plate of Iceland spar 0.15 mm. thick and so mounted within a cell containing carbon disulphide that its optic axis was perpendicular to the ray and covering one half of the field of view. The micrometer tangent-screw was provided with a drum so that it could be read directly. The entire system was mounted on a wooden support, so that the axes of the ray were in a horizontal plane, and this was

* *Phil. Mag.* Jan. 1903, p. 161.

placed upon the same pivoted trough which I used before in my experiments on the double refraction of water* and on rotary polarization†. A seat was also fastened upon the trough so that I could keep my eye uninterruptedly in a fixed position E, before the telescope T, when the system was rotated. The distances IM and MM'A were 40 cm. and 180 cm. respectively, and the angle MM'A was 10° approximately. The plates 1, 1', 2, 2', were selected from the ten plates mentioned above, so as to give as far as possible the best definition. The analyser was first set for a match, and then plates 1 and 1' inserted, and the mirror M' and analyser A adjusted so as to bring the image of the radiant upon the centre of the latter. Plate 1' was rotated in its own plane until the multiple images disappeared, and then its carrier was turned in its vertical plane sufficiently to give a match, the azimuth of the plates thus becoming the same with respect to the direction of propagation of the ray at incidence. Plates 2 and 2' were then inserted and similar adjustments made. After various minor adjustments, I found that either half of the field could be made practically black, on rotating the analyser, showing that the ray had suffered but a slight amount of depolarization by the reflexion from the mirror M' or by the transmission through the plates. The vanishing line, however, was somewhat indistinct, but, by diaphragming the aperture of the system down to about 6 mm. at L, I had a small portion of the field which was fairly uniform with a moderately sharp vanishing line. With carefully figured plates and a more intense source I might have increased my sensibility considerably, but I found this large enough for the purpose as it was.

The magnitude of the probable effect Fizeau calculated by means of Fresnel's "coefficient of convection" for the "æther drift," namely, $(\mu^2 - 1)/\mu^2$. The change in μ due to the velocity v of the glass plate, the velocity of propagation of the ray being V , becomes

$$\delta\mu = \frac{v}{V}(\mu^2 - 1).$$

In order to find the corresponding change in the azimuth of the ray due to such an effect, he compared the rotations of two piles of plates of crown and flint glasses, respectively, with the difference in their indices. Thus he found

$$\frac{\delta(\beta - \alpha)}{\delta\mu} = 4.686 \frac{\beta - \alpha}{\mu}$$

* Phil. Mag. April 1904, p. 318.

† Phil. Mag. Sept. 1905, p. 383.

for the difference $1.6224 - 1.5134$ of the indices of the two kinds of glass. But $\frac{\delta \mu}{\mu} = \frac{1}{11740}$ from the previous equation, on substituting the values of v and V , where

$$\frac{v}{V} = 10^{-4} \text{ and } \mu = 1.5134.$$

Hence he finds
$$\frac{\delta(\beta - \alpha)}{(\beta - \alpha)} = \frac{1}{2500}.$$

For a complete reversal of the ray we should have twice the increment of the rotation, or $\frac{1}{1250}$. Correcting this for the obliquity of the ray to the drift, on account of the refraction of the ray within the glass, we have a change in the rotation of $\frac{1}{1500}$ or, since $\beta - \alpha = 6^\circ 40'$ for each plate with an incidence of 70° and an azimuth of 20° in his experiment, we have, finally, $\frac{1}{1500} \times 6^\circ 40' = 16''$ as the probable effect on a single plate of the motion of the earth.

In my experiment the incidence, 70° , and the index, 1.517 , were, approximately, the same as in Fizeau's experiment. However, instead of an azimuth of 20° , I used an azimuth of 45° , each plate being adjusted with respect to the preceding one to attain this end. Furthermore, on account of the inclination of the plates and the angle $MM'A$, the deviation of the path of the ray within the plates was less than in Fizeau's experiment, so that his factor of $\frac{1}{1500}$ reduced to $\frac{1}{1430}$ in my arrangement. Direct measurements of the angles of rotation, for azimuths 45° and 20° , gave $8^\circ.6$ and $5^\circ.1$, respectively, for each plate, using white light. Fizeau obtained with a single one of his plates a rotation of $6^\circ 40'$. This made his calculated probable effect $16''$ for each plate. The corresponding effect in my experiment was $21''.7$, for each plate, which is $\frac{5}{3}$ greater than that for an azimuth of 20° . With the four plates the calculated probable effect should then be $+0^\circ.024$.

The sensibility of my optical system was such that I could certainly have detected a change of less than $0^\circ.02$ under favourable conditions with an unfatigued eye. On reversing the system I was unable to detect any change whatever. In order to give greater weight to my observations, I took a series of settings, on several different days, whose means showed that such an effect would have been detected several times over. A set of observations consisted of five distinct

settings with the light going westward into the observing telescope, and then, on reversing, five settings with the light entering the eye from the west, or *vice versa*. I then rested my eye preparatory to a second set, and so on. The settings for a match were necessarily made by myself, my assistant reading the drum of the micrometer-screw and recording the same, and then reversing the trough steadily so as not to displace my eye, which was kept focussed upon a definite point of the vanishing line. Some 250 settings were made at the noon hour, and 100 at 5.30 P.M. sun-time; part being taken on reversal from West to East, and part from East to West. 4.2 turns of the micrometer-screw corresponded to 5° of arc. The drum of the micrometer-screw was divided into 50 parts.

I give below a single set of readings taken at random, and also the differences W—E in the means of each set of five readings.

Micrometer Settings
in divisions of the drum.

W.	E.
13.0	13.8
13.4	13.6
13.3	13.2
13.4	13.4
13.2	13.4
13.26	13.48

Differences in
Micrometer Settings.

W—E.

— .20
— .22
+ .16
+ .27
+ .64
+ .54
+ .00
— .22
+ .56
+ .46

— .06
— .38
+ .00
— .34
+ .12

Aug. 3, 1905.

11 h. 53 m. to 12 h. 18 m.

Reversal West to East.

Aug. 3, 1905.

5 h. 13 m.—5 h. 23 m.

West to East.

Differences in
Micrometer Settings.

+ .32	}	Aug. 4, 1905. 11 h. 53 m.—12 h. 4 m. West to East.
+ .46		
+ .12		
+ .40		
— .04		
— .50	}	Aug. 4, 1905. 5 h. 21 m.—5 h. 31 m. East to West.
— .30		
— .18		
+ .52		
— .02		
+ .24	}	Aug. 5, 1905. 11 h. 58 m.—12 h. 8 m. East to West.
+ .38		
+ .28		
— .04		
+ .28		
+ .56	}	Aug. 7, 1905. 11 h. 58 m.—12 h. 8 m. East to West.
— .52		
— .70		
+ .18		
— .32		

We have as the mean value of the noon setting, $W-E=+.14$ of a division of the micrometer drum; and $W-E=-.11$ of a division of the micrometer drum for the afternoon settings. Reducing, we have

$$+.14 \times .02 \times 5^\circ/4.2 = +0^\circ.0033$$

and $-.11 \times .02 \times 5^\circ/4.2 = -0^\circ.0026$

The mean value of the noon deviation $+0^\circ.0033$ is thus over seven times smaller than the calculated value $+0^\circ.024$. The fact that this mean value is of the same sign as that which Fizeau calculated, I do not consider of any significance, as, with a sufficient number of observations, this mean would probably have vanished. Nor do I attach any significance to the fact that the afternoon results show a negative mean. These residuals are quite within the limits of experimental error.

We may therefore conclude that the azimuth of a polarized ray, reflected along the line of drift, is not affected by the motion of the earth through space, at least, to the first order of small quantities.

Physical Laboratory, University of Nebraska,
Lincoln, August 9, 1905.

LXVI. *On the α Particles of Radium.**To the Editors of the Philosophical Magazine.*

GENTLEMEN,—

The University of Adelaide,
20th September, 1905.

PROFESSOR RUTHERFORD has recently announced his discovery that the α particle projected from RaC ceases ionizing when its velocity falls below 60 per cent. of its velocity of projection; when therefore it still possesses a velocity of 1.6×10^{10} cms. per second.

In a recent paper by Mr. Kleeman and myself (Phil. Mag. Sept. 1905) we described a number of experimental results bearing on the relation between the velocity of the particle and its ionizing power and on the loss of range in passing through various atoms and molecules. In our discussion of the results we assumed that the velocity of the α particle fell to a very small value when ionization ceased. Professor Rutherford's important discovery therefore necessitates some modification of our arguments; and I ask your permission to point out briefly what changes must be made.

In the first place, we showed that the loss of range of the α particle in passing through an atom was in all the substances examined proportional to the square root of the weight of the atom. This conclusion requires no modification. But in discussing the theoretical significance of this fact, we had to make some assumption in regard to the amount of energy spent in different gases by the α particle; and we supposed that this amount was independent of the nature of the gas traversed. We argued that, if the velocity of the α particle was very small when it ceased ionizing, the actual value of the critical velocity could be of no importance; and the same amount of its energy, viz. the whole of it, was spent by an α particle before it ceased ionizing, no matter what the gas traversed might be. It now appears that only a portion of the energy is so spent. But the point which we wished to make is clearer than before. It is plain that the α particle stops ionizing at exactly the same speed in all gases (so far as examined), and that our supposition was correct, though our argument was not. For we have traced the ionization curve in the case of a mixture of gases, and of gases of complex molecular structure. Now if the α particle were able to ionize in, say, hydrogen at a lower speed than in oxygen or carbon, then in a mixture we should get a complex curve,

consisting of simple curves overlapping. The entire absence of any effect of this kind shows that the critical speed is invariable. This seems a very important result; and strengthens the argument that the act of ionization is exactly the same in all gases.

It might possibly be argued that the ionization curve of a mixture is simple because the α particle is absolutely stopped at a certain velocity by one of the gases of the mixture. Apart from other difficulties, this explanation is clearly impossible, because, if it were true, the range in the mixture would depend on that gas only.

I may add that we have now examined the range of the α particle in a number of substances besides those given in our first list, viz.: lead, iron, nickel, ethyl iodide, chloroform, oxygen, carbon dioxide, carbon bisulphide, pentane, and benzene; with results at least as good as those previously obtained.

As regards our attempt to discover the relation between velocity and ionization we are not so fortunate; and our original calculation fails because we assumed the α particle of Ra C to have lost its speed at 7 cms. from the start. It is easy, however, to make the proper alteration; but it is possible that the extra complication reduces the chance of obtaining a correct interpretation of the result.

Let s_0 be the distance traversed by the particle from the point where it ceases ionizing to the point where its velocity becomes negligible. Then in the case of the α particle of Ra C, the energy at a distance s_0 from this latter point is 40 per cent. of the energy at a distance $7 + s_0$. If distance from this point be denoted generally by s , and if ionization varies inversely as the n th power of the velocity, then the energy $\propto s^{2/(n+2)}$ (see previous paper),

$$\text{and } \therefore \frac{5}{2} = \frac{(s_0 + 7)^{\frac{2}{n+2}}}{s_0^{\frac{2}{n+2}}};$$

$$\text{so that } s_0 = \frac{7}{(2.5)^{\frac{n+2}{2}} - 1}.$$

Having thus found s_0 , we see that the ionization is proportional to

$$(x + s_0)^{2/(n+2)} - (x + s_0 - d)^{2/(n+2)},$$

when α particles from the highest stratum of the radium layer

pass the ionization chamber by a distance x measured in air, and those from the lowest by a distance $x-d$, d being the effective thickness of the layer measured in air. When x is less than d , as in the top part of the ionization curve, the ionization is proportional to

$$(x + s_0)^{2/(n+2)} - s_0^{2/(n+2)}.$$

I find that when n is put equal to 1, d being estimated at $\cdot 5$ cm., $s_0 = 2\cdot 37$ cm., when $n=2$, $s_0 = 1\cdot 33$ cm., and when $n=3$, $s_0 = \cdot 8$ cm.: and I have calculated a few values on the ionization curves given by the above formulæ for each of these values of n . I have placed alongside some corresponding actual observations, making all the curves nearly agree at a distance 6·4 cm. from the Ra.

Distance from Radium.	Calculated Values of Ionization.			Observed Values.
	$n=1.$	$n=2.$	$n=3.$	
7·0	0	0	0	0
6·8	132	120	116	115
6·4	282	277	283	273
6·0	266	248	237	250
5·5	252	223	200	218
5·0	242	205	176	200
4·5	233	191	157	187
4·0	224	177	144	177
3·5	217	168	133	172

So far, therefore, as the evidence goes, it is now in favour of the value $n=2$. It is interesting to observe that when one particle flies at great speed past another which is relatively at rest, the energy given by the ionizing to the stationary particle, in consequence of their mutual attractions or repulsions, is inversely proportional to the square of the velocity of the moving particle. (Report of the Australasian Association for the Advancement of Science, Dunedin, 1904, p. 64.)

I am, Gentlemen,

Yours faithfully,

W. H. BRAGG.

LXVII. *On Surfaces of Discontinuity in a Rotationally Elastic Medium.* By T. H. HAVELOCK, M.A., D.Sc., Fellow of St. John's College, Cambridge*.

§ 1. *Introduction.*

THE kinematics of wave propagation have been studied in great detail recently by Duhem† and Hadamard‡. Their method consists in following the motion of surfaces of discontinuity in the fluid and in developing in connexion therewith the idea of compatibility or persistence, due originally to Hugoniot.

In §§ 2–5 of the following paper a short account of this method is given, and the rest of the paper consists of an application to the electric æther when this is considered as a rotationally elastic medium; the production and mode of propagation of discontinuities of various orders are discussed, together with the relations of various vectors at the surface of discontinuity.

§ 2. *Surfaces of Discontinuity in general.*

The discontinuities which we shall consider are not perfectly general, but are limited by the condition that they are situated on isolated surfaces. Let Φ be any function of x, y, z, t and the derivatives of the coordinates with respect to the time, and let S be a surface on which Φ is discontinuous at any time; let S be given by

$$f(x, y, z, t) = 0.$$

The surface S divides the space into two regions 1 and 2. At each point of the region 1, Φ is continuous and takes a definite value Φ_1 ; further, at each point (x_0, y_0, z_0) on the surface S , Φ takes the definite value Φ_1^0 and is said to be continuous provided the path by which (x, y, z) approaches the point (x_0, y_0, z_0) lies entirely within the region 1.

Similarly for the region 2 there is a limiting value Φ_2^0 which the function Φ takes at the point (x_0, y_0, z_0) when approached by a path lying entirely in the region 2.

The difference $\Phi_2^0 - \Phi_1^0$ is the measure of the discontinuity of Φ on the surface S and is written $[\Phi]$.

Next suppose that Φ itself is continuous on S , but that its first derivatives are discontinuous; that is,

$$[\Phi] = 0; \quad \left[\frac{\partial \Phi}{\partial x} \right], \quad \left[\frac{\partial \Phi}{\partial y} \right], \quad \left[\frac{\partial \Phi}{\partial z} \right] \neq 0.$$

* Communicated by the Author.

† Duhem, *Recherches sur l'Hydrodynamique*, Paris, 1904.

‡ Hadamard, *Leçons sur la propagation des Ondes*, Paris, 1903.

These latter quantities are not absolutely independent. For if we vary Φ along a path on the surface S , first in region 1 and then in region 2, we have

$$\left[\frac{\partial\Phi}{\partial x}\right]\delta x + \left[\frac{\partial\Phi}{\partial y}\right]\delta y + \left[\frac{\partial\Phi}{\partial z}\right]\delta z = 0,$$

where the variations δx , δy , δz are connected by the relation

$$\frac{\partial f}{\partial x}\delta x + \frac{\partial f}{\partial y}\delta y + \frac{\partial f}{\partial z}\delta z = 0.$$

Hence we have the relations

$$\left[\frac{\partial\Phi}{\partial x}\right]\left/\frac{\partial f}{\partial x}\right. = \left[\frac{\partial\Phi}{\partial y}\right]\left/\frac{\partial f}{\partial y}\right. = \left[\frac{\partial\Phi}{\partial z}\right]\left/\frac{\partial f}{\partial z}\right..$$

Again, if we suppose Φ and its first derivatives continuous, but the second derivatives discontinuous on S , we find in a similar manner the following relations among the second-order discontinuities— λ being an arbitrary parameter :

$$\begin{aligned} \left[\frac{\partial^2\Phi}{\partial x^2}\right] &= \lambda\left(\frac{\partial f}{\partial x}\right)^2; & \left[\frac{\partial^2\Phi}{\partial y^2}\right] &= \lambda\left(\frac{\partial f}{\partial y}\right)^2; & \left[\frac{\partial^2\Phi}{\partial z^2}\right] &= \lambda\left(\frac{\partial f}{\partial z}\right)^2; \\ \left[\frac{\partial^2\Phi}{\partial y\partial z}\right] &= \lambda\frac{\partial f}{\partial y}\frac{\partial f}{\partial z}; & \left[\frac{\partial^2\Phi}{\partial z\partial x}\right] &= \lambda\frac{\partial f}{\partial z}\frac{\partial f}{\partial x}; & \left[\frac{\partial^2\Phi}{\partial x\partial y}\right] &= \lambda\frac{\partial f}{\partial x}\frac{\partial f}{\partial y}. \end{aligned}$$

And, in general, if Φ and its derivatives are continuous up to order $n-1$, the relations between the discontinuities of order n can be written in the form

$$\begin{aligned} \left(\left[\frac{\partial}{\partial x}\right]\delta x + \left[\frac{\partial}{\partial y}\right]\delta y + \left[\frac{\partial}{\partial z}\right]\delta z\right)^n \Phi \\ = \lambda\left(\frac{\partial f}{\partial x}\delta x + \frac{\partial f}{\partial y}\delta y + \frac{\partial f}{\partial z}\delta z\right)^n. \quad (1) \end{aligned}$$

§ 3. *The Order of a Discontinuity.*

We are to consider x , y , z as the actual coordinates of a particle of an elastic medium ; thus if a , b , c be the initial or natural coordinates of any particle, we have

$$(x, y, z) = (a + \xi, b + \eta, c + \zeta),$$

the displacement (ξ, η, ζ) being considered small.

We shall consider discontinuities in (ξ, η, ζ) and their various derivatives with respect to the time t and the co-ordinates (x, y, z) . An absolute discontinuity is one which occurs in (ξ, η, ζ) itself ; and if n be the order of a derivative

such as

$$\frac{\partial^n \xi}{\partial x^p \partial y^q \partial z^r \partial t^s}, \text{ where } p+q+r+s=n,$$

then the order of a discontinuity is the lowest of the orders of the derivatives in which discontinuity occurs.

§ 4. *Relations of Identity.*

Consider a discontinuity of the first order in (ξ, η, ζ) . Then from the general results in § 2 we have

$$\left. \begin{aligned} \left[\frac{\partial \xi}{\partial x} \right] &= \lambda \frac{\partial f}{\partial x}; & \left[\frac{\partial \xi}{\partial y} \right] &= \lambda \frac{\partial f}{\partial y}; & \left[\frac{\partial \xi}{\partial z} \right] &= \lambda \frac{\partial f}{\partial z}; \\ \left[\frac{\partial \eta}{\partial x} \right] &= \mu \frac{\partial f}{\partial x}; & \left[\frac{\partial \eta}{\partial y} \right] &= \mu \frac{\partial f}{\partial y}; & \left[\frac{\partial \eta}{\partial z} \right] &= \mu \frac{\partial f}{\partial z}; \\ \left[\frac{\partial \zeta}{\partial x} \right] &= \nu \frac{\partial f}{\partial x}; & \left[\frac{\partial \zeta}{\partial y} \right] &= \nu \frac{\partial f}{\partial y}; & \left[\frac{\partial \zeta}{\partial z} \right] &= \nu \frac{\partial f}{\partial z}; \end{aligned} \right\} \quad . \quad . \quad (2)$$

where λ, μ , and ν are arbitrary parameters. Thus a single vector (λ, μ, ν) , given over the surface S , is sufficient to define these nine discontinuities of the first order.

Similarly for a discontinuity of the second order; and in general for one of the n th order the relations between the derivatives not involving the time can be expressed by means of a vector (λ, μ, ν) in the form

$$\begin{aligned} & \left(\left[\frac{\partial}{\partial x} \right] \delta x + \left[\frac{\partial}{\partial y} \right] \delta y + \left[\frac{\partial}{\partial z} \right] \delta z \right)^n (\xi, \eta, \zeta) \\ &= (\lambda, \mu, \nu) \left(\frac{\partial f}{\partial x} \delta x + \frac{\partial f}{\partial y} \delta y + \frac{\partial f}{\partial z} \delta z \right)^n \quad . \quad . \quad (3) \end{aligned}$$

§ 5. *Conditions of Compatibility.*

The notion of compatibility was first introduced by Hugoniot into the study of fluid motions in two regions separated by a surface of discontinuity, and the same idea is used by Duhem under the name of "persistence."

The relations of identity found in the previous section are those which hold at a given surface of discontinuity S at a given time t_0 . The motions in the two regions separated by S are said to be compatible if the surface S is unique not only at the instant t_0 but also at times immediately before and after that instant; the moving surface S is then a wave-front and is said to constitute a persistent wave. There are certain kinematical relations which are satisfied whatever be the dynamical equations of the particular medium.

The wave-front S is defined as the position at a given time of a surface given in three-dimensional space by

$$f(x, y, z, t) = 0. \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Now consider x, y, z, t as space coordinates in a four-dimensional space; and let Σ be the multiplicity given by equation (4). Then the various positions of S are the sections of Σ by planes $t = \text{constant}$.

Now the relations of identity were obtained by considering variations on the surface S , and in an exactly similar manner it is clear that the conditions of compatibility are to be obtained by the same process applied to Σ . And in general, for a discontinuity of order n in a function Φ , we obtain instead of (1) the relation

$$\begin{aligned} \left(\left[\frac{\partial}{\partial x} \right] \delta x + \left[\frac{\partial}{\partial y} \right] \delta y + \left[\frac{\partial}{\partial z} \right] \delta z + \left[\frac{\partial}{\partial t} \right] \delta t \right)^n \Phi \\ = \lambda \left(\frac{\partial f}{\partial x} \delta x + \frac{\partial f}{\partial y} \delta y + \frac{\partial f}{\partial z} \delta z + \frac{\partial f}{\partial t} \delta t \right)^n. \quad . \quad . \quad (5) \end{aligned}$$

Thus for a discontinuity of order n in the displacement (ξ, η, ζ) a single vector (λ, μ, ν) arbitrarily given over Σ is sufficient to define the variations in all the derivatives of order n , provided the wave-front is persistent. The relations can be written in the form

$$\begin{aligned} \left(\left[\frac{\partial}{\partial x} \right] \delta x + \left[\frac{\partial}{\partial y} \right] \delta y + \left[\frac{\partial}{\partial z} \right] \delta z + \left[\frac{\partial}{\partial t} \right] \delta t \right) (\xi, \eta, \zeta) \\ = (\lambda, \mu, \nu) \left(\frac{\partial f}{\partial x} \delta x + \frac{\partial f}{\partial y} \delta y + \frac{\partial f}{\partial z} \delta z + \frac{\partial f}{\partial t} \delta t \right)^n, \quad . \quad (6) \end{aligned}$$

which is to hold for all values of $\delta x, \delta y, \delta z, \delta t$.

§ 6. *The Æther as a Rotationally Elastic Medium.*

Having obtained the kinematical relations, we must consider now the dynamical equations of the particular medium. Let (X, Y, Z) be the electric force measured electrostatically, and (α, β, γ) the magnetic force in electromagnetic units; then we have the usual circuital relations in the form

$$\left. \begin{aligned} \frac{1}{c} \frac{\partial}{\partial t} (X, Y, Z) &= \text{Curl } (\alpha, \beta, \gamma) \\ - \frac{1}{c} \frac{\partial}{\partial t} (\alpha, \beta, \gamma) &= \text{Curl } (X, Y, Z) \end{aligned} \right\} . \quad . \quad . \quad . \quad . \quad (7)$$

Now we may introduce a new vector (ξ, η, ζ) defined by

$$(\alpha, \beta, \gamma) = \frac{\partial}{\partial t}(\xi, \eta, \zeta); \quad \frac{1}{c}(X, Y, Z) = \text{Curl}(\xi, \eta, \zeta). \quad (8)$$

In a rotationally elastic æther, (ξ, η, ζ) is interpreted as linear displacement; the constants of inertia and elasticity of the medium must be supposed very large so that all the motions and displacements are small and can be analysed into independent differential strains and rotations*. Otherwise, we may begin with such a medium and take the energy functions to be

$$T = \frac{1}{2}A \int (\dot{\xi}^2 + \dot{\eta}^2 + \dot{\zeta}^2) d\tau;$$

$$W = \frac{1}{2}B \int (f^2 + g^2 + h^2) d\tau;$$

where

$$(f, g, h) = \text{Curl}(\xi, \eta, \zeta).$$

The principle of least action gives the equations of motion; and comparing the potential energy function W with electrical energy we obtain the correspondence given in (8).

Regarding then the æther as an incompressible, rotationally elastic medium, its dynamical equations are

$$\nabla^2(\xi, \eta, \zeta) = \frac{1}{c^2} \frac{\partial^2}{\partial t^2}(\xi, \eta, \zeta), \quad \dots \dots \dots (9)$$

combined with

$$\frac{\partial \xi}{\partial x} + \frac{\partial \eta}{\partial y} + \frac{\partial \zeta}{\partial z} = 0. \quad \dots \dots \dots (10)$$

Now in order to determine completely the motion in the medium, we must know in addition the conditions at the limiting surface of the medium. For instance, suppose we know the motion of the boundary at each instant, and thus the acceleration of each point; then this set of values for the acceleration may differ at any moment from those obtained from the internal equations of motion of the medium. Consequently a discontinuity of the second order is set up at the boundary and is propagated into the medium. Again, if the velocity or pressure at the boundary be suddenly altered, a discontinuity of the first order will be set up.

In electrical terms, we may be given a boundary in the æther at which the electric or magnetic force is assigned; then there may be set up a discontinuity of the first order which is propagated into the æther as a wave-front at which the electric and magnetic forces are not continuous. Or, again, if the electric current is assigned at the boundary,

* Larmor, 'Æther and Matter,' p. 332.

there may be the production of a discontinuity of the second order.

Supposing these discontinuities to be produced in some such manner, we proceed to discuss the method of their propagation.

§ 7. *Discontinuity of the Second Order.*

In this case we have on S

$$\left[\frac{\partial^2 \xi}{\partial x^2}\right] = \lambda \left(\frac{\partial f}{\partial x}\right)^2; \dots \left[\frac{\partial^2 \xi}{\partial t^2}\right] = \lambda \left(\frac{\partial f}{\partial t}\right)^2; \text{ \&c.}$$

Hence, supplying these values in the second-order equations given in (9), we have

$$\left(\frac{\partial f}{\partial x}\right)^2 + \left(\frac{\partial f}{\partial y}\right)^2 + \left(\frac{\partial f}{\partial z}\right)^2 = \frac{1}{c^2} \left(\frac{\partial f}{\partial t}\right)^2. \quad \dots (11)$$

Thus the discontinuity is propagated in the medium as a wave-front moving with a constant normal velocity c .

Using the notation

$$(\lambda, \mu, \nu) = \mathbf{R}; \quad \left(\frac{\partial f}{\partial x}, \frac{\partial f}{\partial y}, \frac{\partial f}{\partial z}\right) = \mathbf{N},$$

$\mathbf{R} \cdot \mathbf{N}$ = scalar product,

and $\mathbf{R} \times \mathbf{N}$ = vector product,

we have from (10) and (8)

$$\left. \begin{aligned} \mathbf{R} \cdot \mathbf{N} &= 0 \\ [u, v, w] &= \frac{\partial f}{\partial t} \mathbf{R} \times \mathbf{N} \\ [\alpha, \beta, \gamma] &= \left(\frac{\partial f}{\partial t}\right)^2 \mathbf{R} \end{aligned} \right\} \dots \dots \dots (12)$$

Hence the vector defining the discontinuity lies everywhere in the wave-front. And the discontinuities in the electric and magnetic currents are also tangential to the wave-front and at right angles to each other, while the electric and magnetic forces are supposed continuous.

§ 8. *Discontinuity of the First Order.*

In this case we have

$$\left[\frac{\partial \xi}{\partial x}\right] = \lambda \frac{\partial f}{\partial x}; \quad \left[\frac{\partial \xi}{\partial y}\right] = \lambda \frac{\partial f}{\partial y}; \quad \left[\frac{\partial \xi}{\partial z}\right] = \lambda \frac{\partial f}{\partial z}; \quad \dots (13)$$

$$\left[\frac{\partial \xi}{\partial t}\right] = \lambda \frac{\partial f}{\partial t}; \quad \dots \dots \dots (14)$$

and similar relations in (η, μ) and (ζ, ν) .

Now in order to use the dynamical equations, we must find the influence of a first-order discontinuity upon the derivatives of the second order*. We shall use the following notation:—

$$f_1(\delta x, \delta y, \delta z) = \frac{\partial f}{\partial x} \delta x + \frac{\partial f}{\partial y} \delta y + \frac{\partial f}{\partial z} \delta z,$$

$$f_2 = \left(\frac{\partial}{\partial x} \delta x + \frac{\partial}{\partial y} \delta y + \frac{\partial}{\partial z} \delta z \right)^2 f = \frac{\partial^2 f}{\partial x^2} \delta x^2 + \&c.,$$

$$f_1' = \frac{\partial^2 f}{\partial x \partial t} \delta x + \frac{\partial^2 f}{\partial y \partial t} \delta y + \frac{\partial^2 f}{\partial z \partial t} \delta z,$$

$$\xi_2 = \left(\left[\frac{\partial}{\partial x} \right] \delta x + \left[\frac{\partial}{\partial y} \right] \delta y + \left[\frac{\partial}{\partial z} \right] \delta z \right)^2 \xi,$$

$$\xi_1' = \left[\frac{\partial^2 \xi}{\partial x \partial t} \right] \delta x + \left[\frac{\partial^2 \xi}{\partial y \partial t} \right] \delta y + \left[\frac{\partial^2 \xi}{\partial z \partial t} \right] \delta z.$$

To form the relations of identity as before, we proceed to the second variation on S of the equation

$$[\xi] = \text{constant};$$

then we obtain

$$\xi_2 + \left[\frac{\partial \xi}{\partial x} \right] \delta^2 x + \left[\frac{\partial \xi}{\partial y} \right] \delta^2 y + \left[\frac{\partial \xi}{\partial z} \right] \delta^2 z = 0, \quad (15)$$

together with

$$\left. \begin{aligned} f_1 &= 0 \\ f_2 + \frac{\partial f}{\partial x} \delta^2 x &= \frac{\partial f}{\partial y} \delta^2 y + \frac{\partial f}{\partial z} \delta^2 z = 0 \end{aligned} \right\} \quad (16)$$

Consequently we have from (13),

$$\xi_2 - \lambda f_2 = 0,$$

when

$$f_1 = 0.$$

Hence for all values of δx , δy , δz we have a relation of the form

$$\xi_2 = \lambda f_2 + 2f_1(A\delta x + B\delta y + C\delta z). \quad (17)$$

Now vary on S the equation

$$\left[\frac{\partial \xi}{\partial x} \right] = \lambda \frac{\partial f}{\partial x}.$$

Then, since

$$\delta \left(\frac{\partial f}{\partial x} \right) = \frac{\partial^2 f}{\partial x^2} \delta x + \frac{\partial^2 f}{\partial x \partial y} \delta y + \frac{\partial^2 f}{\partial x \partial z} \delta z = \frac{1}{2} \frac{\partial f_2}{\partial (\delta x)},$$

* Cf. Hadamard, *loc. cit.* p. 121.

and similarly

$$\delta \left[\frac{\partial \xi}{\partial x} \right] = \frac{1}{2} \frac{\partial \xi_2}{\partial (\delta x)},$$

we obtain

$$\frac{1}{2} \frac{\partial \xi_2}{\partial (\delta x)} = \frac{\partial f}{\partial x} \delta \lambda + \frac{1}{2} \lambda \frac{\partial f_2}{\partial (\delta x)}. \quad \dots \quad (18)$$

But from (17)

$$\frac{1}{2} \frac{\partial \xi_2}{\partial (\delta x)} = \frac{1}{2} \lambda \frac{\partial f_2}{\partial (\delta x)} + \frac{\partial f}{\partial x} (A \delta x + B \delta y + C \delta z).$$

Hence on S we have

$$\delta \lambda = A \delta x + B \delta y + C \delta z.$$

To obtain the conditions of compatibility in the second order, we take variations as before on the multiplicity Σ instead of on S; the time variation δt occurs in a similar manner to the other variations, and introducing a fourth undetermined multiplier D, we have on Σ

$$\delta \lambda = A \delta x + B \delta y + C \delta z + D \delta t.$$

And the discontinuities in the second-order derivatives can be expressed finally in the form

$$\begin{aligned} & \left(\left[\frac{\partial}{\partial x} \right] \delta x + \left[\frac{\partial}{\partial y} \right] \delta y + \left[\frac{\partial}{\partial z} \right] \delta z + \left[\frac{\partial}{\partial t} \right] \delta t \right)^2 \xi \\ & \quad = \lambda \left(\frac{\partial}{\partial x} \delta x + \frac{\partial}{\partial y} \delta y + \frac{\partial}{\partial z} \delta z + \frac{\partial}{\partial t} \delta t \right)^2 f \\ & + 2 \left(\frac{\partial f}{\partial x} \delta x + \frac{\partial f}{\partial y} \delta y + \frac{\partial f}{\partial z} \delta z + \frac{\partial f}{\partial t} \delta t \right) (A \delta x + B \delta y + C \delta z + D \delta t); \quad (19) \end{aligned}$$

together with similar expressions in

$$(\eta, \mu, A', B', C', D') \text{ and } (\zeta, \nu, A'', B'', C'', D'').$$

Thus for a first-order discontinuity defined by a vector (λ, μ, ν) in equations (13) and (14), the variations in the second-order derivatives are expressible in the form

$$\begin{aligned} \left[\frac{\partial^2 \xi}{\partial x^2} \right] &= \lambda \frac{\partial^2 f}{\partial x^2} + 2A \frac{\partial f}{\partial x}; & \left[\frac{\partial^2 \eta}{\partial x \partial y} \right] &= \mu \frac{\partial^2 f}{\partial x \partial y} + B' \frac{\partial f}{\partial x} + A' \frac{\partial f}{\partial y}; \\ \left[\frac{\partial^2 \zeta}{\partial t^2} \right] &= \nu \frac{\partial^2 f}{\partial t^2} + 2D'' \frac{\partial f}{\partial t}; \quad (20) \end{aligned}$$

and similar expressions.

Now for the particular medium in question we have the equations:—

$$\left[\frac{\partial \xi}{\partial x} \right] + \left[\frac{\partial \eta}{\partial y} \right] + \left[\frac{\partial \zeta}{\partial z} \right] = 0,$$

$$\left[\frac{\partial^2 \xi}{\partial x^2} \right] + \left[\frac{\partial^2 \eta}{\partial x \partial y} \right] + \left[\frac{\partial^2 \zeta}{\partial x \partial z} \right] = 0,$$

and three similar equations.

Also there are three dynamical equations of the type

$$\left[\frac{\partial^2 \xi}{\partial x^2} \right] + \left[\frac{\partial^2 \xi}{\partial y^2} \right] + \left[\frac{\partial^2 \xi}{\partial z^2} \right] = \frac{1}{c^2} \left[\frac{\partial^2 \xi}{\partial t^2} \right].$$

Supply in these the values from (20) and use for shortness the vector notation:—

$\mathbf{A} = (A, A', A'')$ and similarly for $\mathbf{B}, \mathbf{C}, \mathbf{D}$;

$\mathbf{R} = (\lambda, \mu, \nu)$; $\mathbf{N} = \left(\frac{\partial f}{\partial x}, \frac{\partial f}{\partial y}, \frac{\partial f}{\partial z} \right)$;

also $\nabla(\mathbf{R} \cdot \mathbf{N})_{\mathbf{R}} \equiv \Sigma \left(\mathbf{R} \cdot \frac{\partial \mathbf{N}}{\partial x} \right) \mathbf{i}.$

Then we obtain the following equations:—

$$\mathbf{R} \cdot \mathbf{N} = 0,$$

$$\nabla(\mathbf{R} \cdot \mathbf{N})_{\mathbf{R}} + (\mathbf{A}, \mathbf{B}, \mathbf{C}) \cdot \mathbf{N} = -(\mathbf{A} + \mathbf{B}' + \mathbf{C}'') \mathbf{N},$$

$$\mathbf{R} \cdot \frac{\partial \mathbf{N}}{\partial t} + \mathbf{D} \cdot \mathbf{N} = -(\mathbf{A} + \mathbf{B}' + \mathbf{C}'') \frac{\partial f}{\partial t}$$

$$\left(\nabla^2 f - \frac{1}{c^2} \frac{\partial^2 f}{\partial t^2} \right) \mathbf{R} = -2 \left(\frac{\partial f}{\partial x} \mathbf{A} + \frac{\partial f}{\partial y} \mathbf{B} + \frac{\partial f}{\partial z} \mathbf{C} - \frac{1}{c^2} \frac{\partial f}{\partial t} \mathbf{D} \right).$$

From these equations, representing eight scalar equations, we can deduce the following scalar equation:

$$\mathbf{N} \cdot \nabla(\mathbf{R} \cdot \mathbf{N})_{\mathbf{R}} - \frac{1}{c^2} \frac{\partial f}{\partial t} \left(\mathbf{R} \cdot \frac{\partial \mathbf{N}}{\partial t} \right) = -(\mathbf{A} + \mathbf{B}' + \mathbf{C}'') \left\{ \mathbf{N} \cdot \mathbf{N} - \frac{1}{c^2} \left(\frac{\partial f}{\partial t} \right)^2 \right\};$$

and this equation can be written in the ordinary notation under the form

$$\left\{ \lambda \frac{\partial}{\partial x} + \mu \frac{\partial}{\partial y} + \nu \frac{\partial}{\partial z} + 2(\mathbf{A} + \mathbf{B}' + \mathbf{C}'') \right\} \left\{ \left(\frac{\partial f}{\partial x} \right)^2 + \left(\frac{\partial f}{\partial y} \right)^2 + \left(\frac{\partial f}{\partial z} \right)^2 - \frac{1}{c^2} \left(\frac{\partial f}{\partial t} \right)^2 \right\} = 0. \quad (21)$$

Equation (21) is satisfied for all values of the arbitrary parameters by

$$\left(\frac{\partial f}{\partial x} \right)^2 + \left(\frac{\partial f}{\partial y} \right)^2 + \left(\frac{\partial f}{\partial z} \right)^2 - \frac{1}{c^2} \left(\frac{\partial f}{\partial t} \right)^2 = 0.$$

Hence in this medium a persistent wave-front, consisting of a first-order discontinuity separating two states which are compatible up to the second order, is propagated in the medium with constant normal velocity c .

Moreover we have

$$\left. \begin{aligned} [X, Y, Z] &= c[\text{Curl}(\xi, \eta, \zeta)] = c\mathbf{R} \times \mathbf{N} \\ [\alpha, \beta, \gamma] &= \left[\frac{\partial}{\partial t}(\xi, \eta, \zeta) \right] = \frac{\partial f}{\partial t} \mathbf{R} \\ \mathbf{R} \cdot \mathbf{N} &= 0 \end{aligned} \right\} \quad (22)$$

Thus the two vectors representing the discontinuities in the electric and magnetic forces are at right angles to each other and lie in the wave-front.

Also if $\mathbf{E}$ denote the Poynting radiation vector, we have

$$\begin{aligned} [\mathbf{E}] &= [X, Y, Z] \times [\alpha, \beta, \gamma] = -c \frac{\partial f}{\partial t} \mathbf{R} \times (\mathbf{R} \times \mathbf{N}) \\ &= c \frac{\partial f}{\partial t} R^2 \mathbf{N} = \left(\frac{\partial f}{\partial t} \right)^2 R^2 \mathbf{n}, \end{aligned}$$

where $\mathbf{n}$ is unit vector in the direction of the normal to the wave-front.

Thus the discontinuity in the radiation vector is in a direction normal to the wave-front and is of magnitude equal to the square of the discontinuity in the magnetic force.

§ 9. *Stationary and Absolute Discontinuities.*

Up to the present we have assumed that the equation of the surface of discontinuity contains the time, and we have found that such a surface forms a wave-front expanding outwards with constant normal velocity. Consider the possibility of a stationary discontinuity of the first order. Then from (22) we see that the discontinuity of the magnetic force vanishes; also since (21) becomes

$$\left\{ \lambda \frac{\partial}{\partial x} + \mu \frac{\partial}{\partial y} + \nu \frac{\partial}{\partial z} + 2(A + B' + C'') \right\} \left\{ \left(\frac{\partial f}{\partial x} \right)^2 + \left(\frac{\partial f}{\partial y} \right)^2 + \left(\frac{\partial f}{\partial z} \right)^2 \right\} = 0,$$

it follows that the vector $\mathbf{R}$ must also be zero. Hence there cannot be a stationary surface of discontinuity at which the tangential electric and magnetic forces do not vanish.

In equations (7) and (8), the vector (ξ, η, ζ) is regarded as linear displacement from a natural state of the medium, in which the electric force (X, Y, Z) is everywhere a circuital vector. In order to have electric charges we must suppose the natural state of the medium to include centres of intrinsic

rotational strain. Then a charged surface, or the surface of an electron, might be considered as a surface at which there is given normal discontinuity in (X, Y, Z) ; such a surface in motion is also the seat of a first order discontinuity in (ξ, η, ζ) , the tangential discontinuities in the electric and magnetic forces being given by equations (22) in the case of compatibility in the first order. Thus if a charged surface S' be suddenly given a velocity v , a first-order discontinuity will arise at S' and will be propagated outwards as a wave-front S moving with normal velocity c and leaving behind it the steady state corresponding to S' moving with uniform velocity v .

LXVIII. *Notices respecting New Books.*

Mathematische Einführung in die Elektronentheorie. Von Dr. A. H. BÜCHERER. Teubner: Leipzig, 1904.

THIS is an able presentation of the modern electron doctrine as developed by Lorentz, J. J. Thomson, Heaviside, and others. The modifications which the assumption of this theory make on Maxwell's equations are clearly stated; and the various interesting problems associated with the motion of charged particles, spheres, and ellipsoids are discussed in considerable detail. The Zeeman effect, the aberration of light, and the electromagnetic theory of dispersion form the concluding sections. The general mathematical theorems are given in the particular modification of the Quaternion vector analysis which is familiar to readers of Heaviside's important papers; and an appendix summarizes the meanings of the formulæ and symbols used. Any one familiar with quaternions will have no difficulty in understanding the notation, although he will never cease from wondering why vector analysts like Dr. Bücherer should be content with such a comparatively weak imitation of Hamilton's powerful calculus. The author gives five theorems connecting volume, surface, and line integrals, and connects each with the name (presumably) of its discoverer. Gauss and Stokes very properly get their due; but if other theorems of fundamentally the same character as these are to be named after later investigators, it is simple justice to give the name of him who first published them. Theorems (25) and (26) ascribed to Föppl and B (Bücherer?) were given long ago by Tait; and all the five are special cases of a fundamental theorem first given in its most complete form by McAulay. This historic inaccuracy springs of course from the culpable neglect of quaternions at the hands of certain modern vector analysts, who have wasted time and energy in rediscovering truths thirty, forty, and even fifty years old.

The Dynamical Theory of Gases. By J. H. JEANS, M.A. Cambridge University Press, 1904.

SEVERAL excellent treatises on different departments of Natural Philosophy have been recently issued by the Cambridge University Press ; and the present volume cannot fail to take high rank among the standard works and memoirs on the dynamical theory of gases. After an introductory chapter stating the nature of the problem, the author develops the foundations of the theory in four chapters on the Law of Distribution, giving not only the usual statistical theory, but also his own investigation based upon the methods of general dynamics applied to space of n dimensions. This leads to a concise demonstration of Maxwell's equipartition theorem. The whole of Chapter V., dealing with the extension of the theory to molecules of the most general conservative type, is of the highest interest, and paves the way for valuable applications in later sections. Two chapters then follow on the physical properties of gases and the extent to which they can be deduced from the dynamical theory. The significance of the failure of the theory to explain the relation between the specific heats is ably discussed, and leads to the introduction of the dissipative function and the investigation of non-conservative systems along the lines already laid down. The last nine chapters are grouped together under the title "Free Path Phenomena," and take up questions of viscosity, conduction, diffusion, propagation of sound, the problem of the constancy or dissipation of planetary atmospheres, and other topics. The reader who may have no special interest in the mathematical methods themselves, will find a great deal worthy of his consideration. All the known physical properties of gases are introduced as illustrations of the general theory, but apart from this theory in its severe analytical form there is a physical as distinguished from a mathematical charm about the book, which should give it a place in the library of every real student of physical and chemical science. We do not know which is the more to be admired, the mathematical power or the physical insight of the author. This book is a model of fine mathematical printing.

Die Entwicklung der Elektrischen Messungen. Von Dr. O. FRÖLICH. Mit 124 eingedruckten Abbildungen. Braunschweig : F. Vieweg und Sohn. 1905. Pp. xii + 192.

THIS most interesting volume, which forms No. 5 of the series known as *Die Wissenschaft*, contains an account of the evolutionary process by which our present electrical measuring instruments and methods of measurement have come into being. The book is divided into two sections ; the first section dealing with galvanometers, switchboard instruments, resistance coils, energy meters, and recording instruments, while the second section is devoted to methods of measurement, and deals more particularly with resistance and inductance measurements. The subject is treated in a very lucid and interesting manner, and the book should prove most useful to electrical engineers and physicists.

LXIX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 192.]

May 24th, 1905.—J. E. Marr, Sc.D., F.R.S., President,
in the Chair.

THE following communications were read :—

1. 'On the Igneous Rocks occurring between St. David's Head and Strumble Head (Pembrokeshire).' By James Vincent Elsdon, B.Sc., F.G.S.

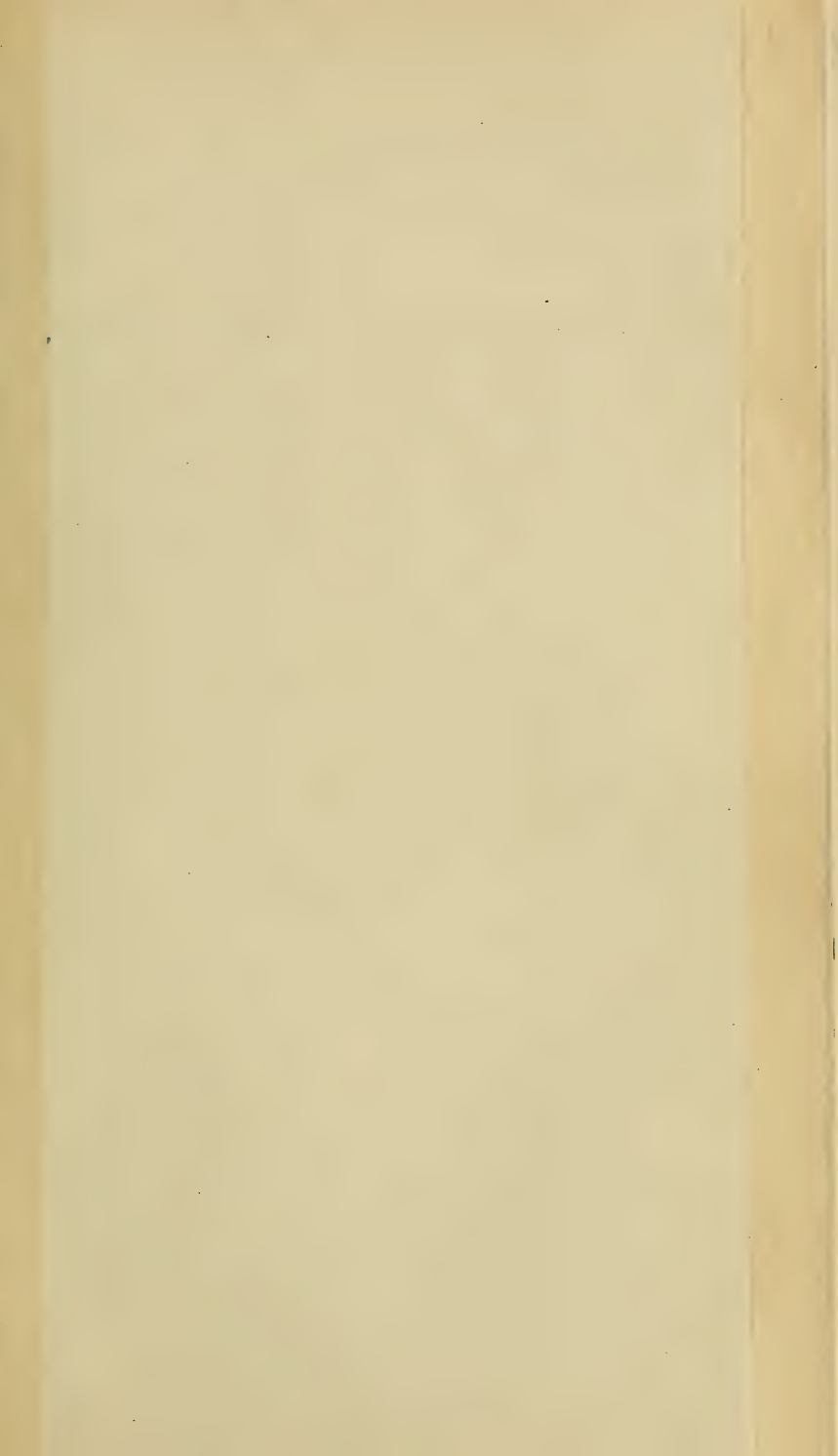
The author finds that the contemporaneous lavas of the Llanrian area agree generally in character with the eruptive rocks of apparently-Ordovician age in the Strumble-Head and Prescelly districts. These are all of an essentially-acid type. The intrusive rocks of the area are of later date, and belong to three distinct types :—(1) The gabbros and diabases of the Strumble-Head area ; (2) the norites and associated rocks of St. David's Head and the surrounding district ; and (3) the lime-bostonites and porphyrites of the Abercastle-Mathry district. Detailed petrographical descriptions of the different types are given, accompanied in many cases by analyses and comparisons with corresponding or related rocks of other areas. The lime-bostonites, which have affinities with those of Mæna described by Brögger, but are much more basic than the Wicklow keratophyres, are apparently the oldest of the intrusive rocks, and seem to belong to the petrographical province of South-Eastern Ireland. The gabbros and norites were intruded approximately during the same interval ; at a later period the norites, enstatite-diorites, and the rest of the rocks associated with them spread north-eastward from St. David's Head, and penetrated the area of the Strumble-Head intrusions. Similarly, the gabbros and diabases spread to a more limited extent south-westward into the norite-area. In the overlapping area the gabbro- and norite-provinces are separated by an ill-defined zone, in which some mixture of the two magmas took place. The latest phase of igneous activity was the formation of the Pen-Caer basalt-laccolite, with apophyses penetrating the Garn-Fawr to Y-Garn intrusions. It is not necessary to assume that each of the several intrusions was confined to any single stage of vulcanicity. The laccolites and bosses were probably the result of injections extending over a prolonged interval from coexisting magma-basins, or from a single differentiated magma. There are clear evidences of some further differentiation *in situ*, but the full extent to which this took place offers a large field for future investigation.

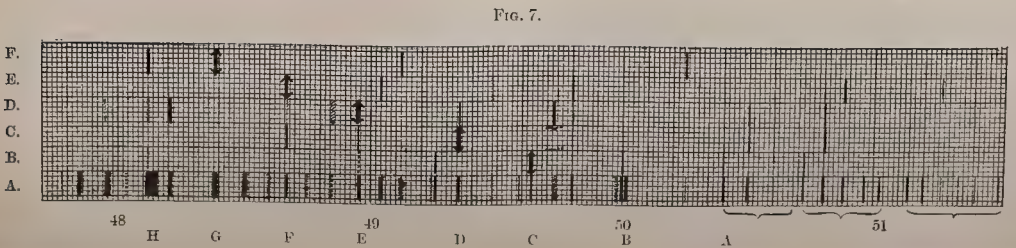
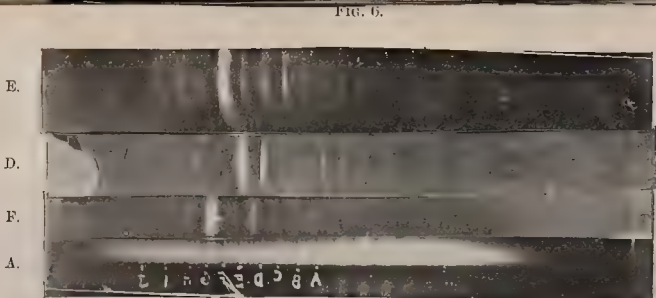
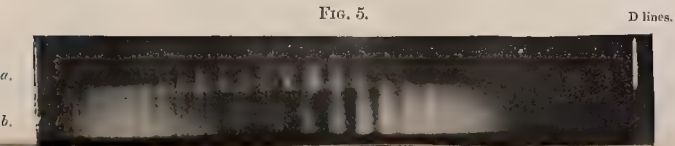
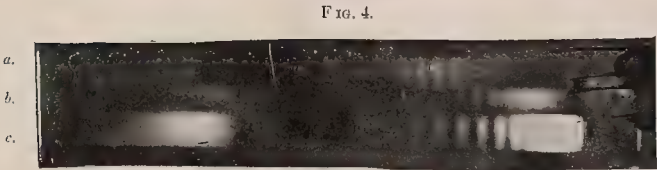
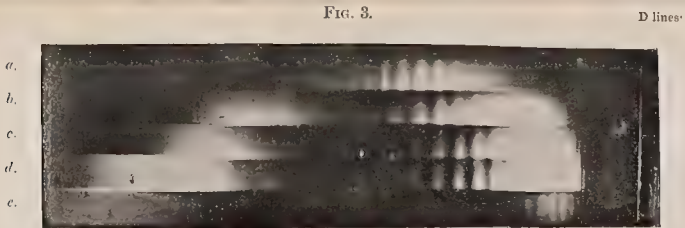
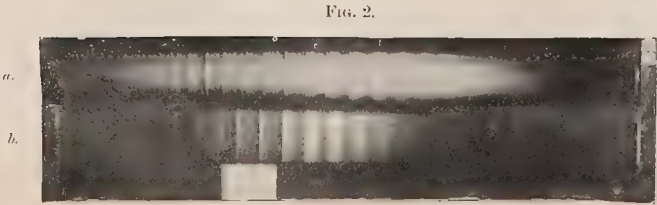
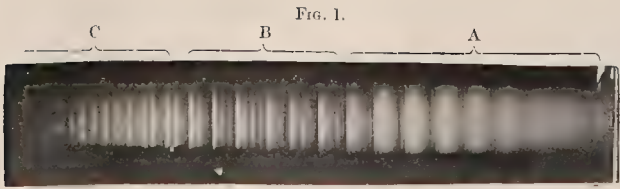
2. 'The Rhætic and Contiguous Deposits of Glamorganshire.' By Linsdall Richardson, F.G.S.

The chief sections in the county are described in detail, the chief being those at Lavernock (near Cardiff), Barry, Tregyff (near Cowbridge), Quarella (Bridgend), and Stormy Down. The Sully Beds, a name given to the fossiliferous portion of the 'Grey Marls' of Etheridge, are determined to belong to the Rhætic Series, on account of the fossils that they contain. They are quite distinct from the 'Tea-Green Marls,' in which fossils have not been observed. Earth-pressures affected the rocks during the formation of the Sully Beds; the *Avicula-contorta* black shales rest upon them with perfect parallelism, but even then non-sequentially. Owing to an upheaval of the Lavernock district early in the age in which the Upper Rhætic Stage was deposited, only a portion of the lowest bed of that stage is found, and this deposit was subjected to sub-aërial denudation during the accumulation of the remaining Upper Rhætic beds elsewhere. But subsidence again in the same district allowed of the deposition of the White Lias, and, as a result, this latter rests non-sequentially upon a portion of the lowest part of the Upper Rhætic deposit. The White Lias, at certain localities in Glamorganshire, contains in abundance *Plicatula intusstriata*, *Pl. hettangiensis*, and *Lima valoniensis*. The deposit intervening between the Sun-Bed and the Upper Rhætic near Bath (Newbridge Hill) is over 11 feet thick; at Lavernock the equivalent deposit measures but 2 feet $2\frac{1}{4}$ inches. At Lavernock, however, above the probable equivalent of the Sun-Bed, are marls 6 feet 4 inches thick, which are provisionally grouped with the White Lias. Above come the Paper-Shales, succeeded by the *Ostrea*-Beds. The Upper Rhætic of North-West Gloucestershire and Worcestershire is not the equivalent of the White Lias of this or of the Bath district. The White Lias occurs above the Cotham Marble (the topmost bed of the Upper Rhætic) and below the Paper-Shales (which occur immediately below the *Ostrea*-Beds). Palæontological notes on certain of the fossils are appended, including *Ostrea Bristovi*, Etheridge MS., which is very abundant in the Sully Beds.

3. 'On the Occurrence of Rhætic Rocks at Berrow Hill, near Tewkesbury (Gloucestershire).' By Linsdall Richardson, F.G.S.

About 2 miles south-east from Chase-End Hill (Malvern Hills) there is a small outlier of Lower Liassic and Rhætic beds, in a basin-shaped area, supported and surrounded by Keuper Sandstone. A detailed section is given, mainly obtained by excavation, and this is compared with the nearest locality where the whole of the Rhætic may be studied, namely, at Wainlode Cliff. At Berrow Hill rock at the base of the Lower Rhætic, at least $3\frac{1}{4}$ feet thick, is missing; consequently Bed 13 rests directly, and with perfect parallelism (so far as can be seen), but non-sequentially, upon the 'Tea-Green Marls.'





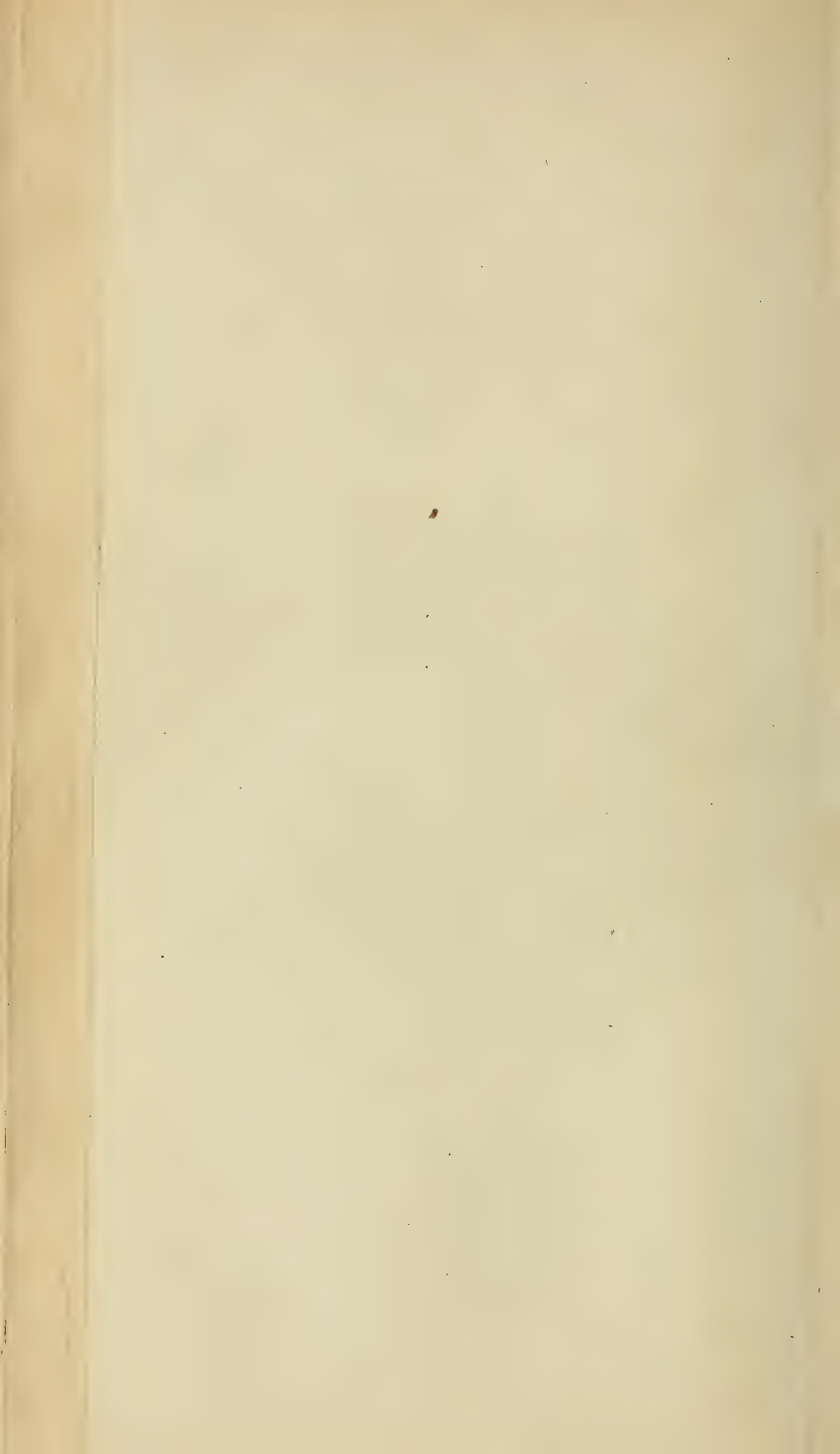


Fig. 2.

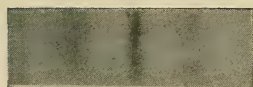


Fig. 4.



Fig. 6.

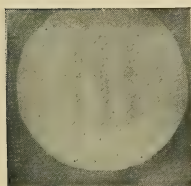


Fig. 5, *d*; 28.32% Ni.

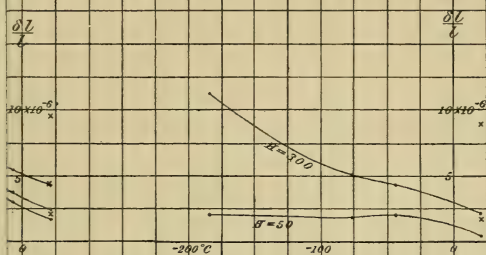


Fig. 5, *g*; 24.0% Ni.

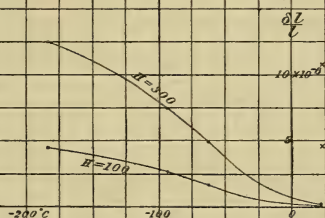


Fig. 6, *c*.

Nickel.

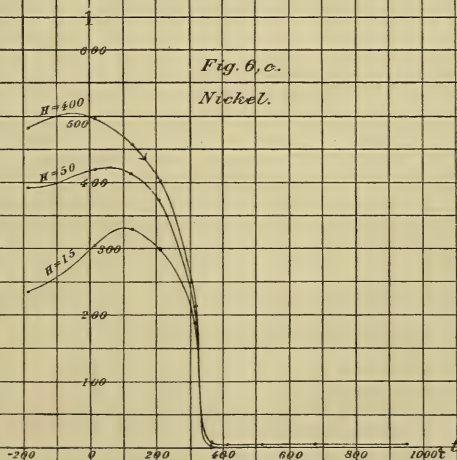


Fig. 5, a, 29.28% Ni.

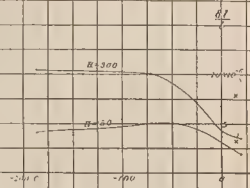


Fig. 5, b, 39% Ni.

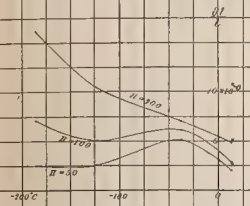


Fig. 5, c, 28.78% Ni.

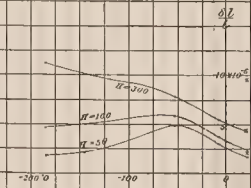


Fig. 5, d, 28.32% Ni.

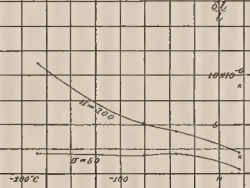


Fig. 5, e, 26.62% Ni.

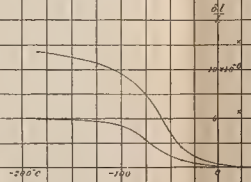


Fig. 5, f, 26.40% Ni.

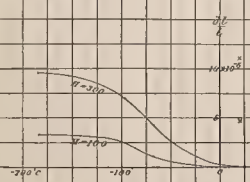


Fig. 5, g, 26.0% Ni.

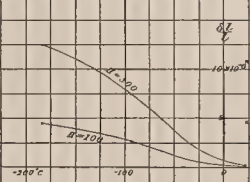


Fig. 6, a.
Svedberg iron.

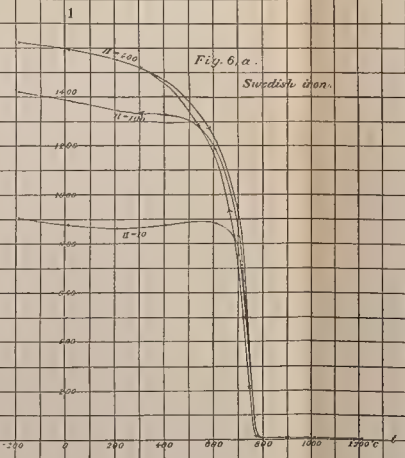


Fig. 6, b.
Cobalt (Annealed)

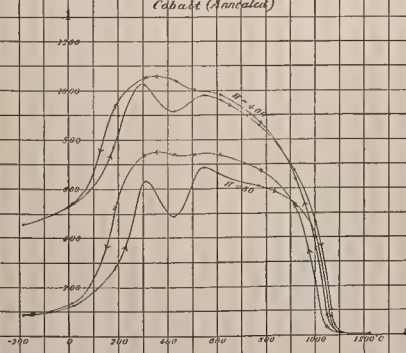
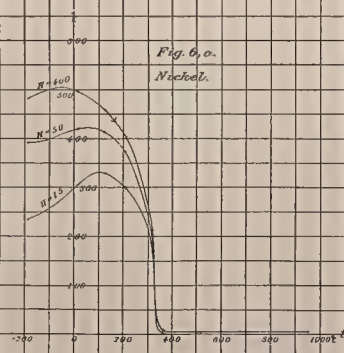
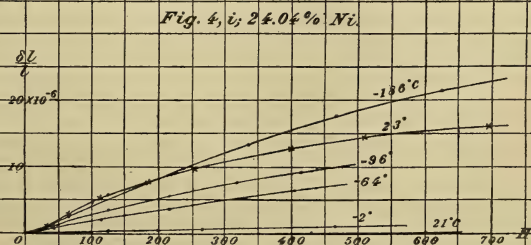
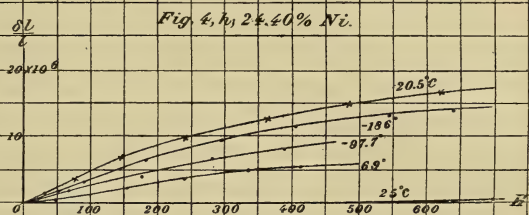
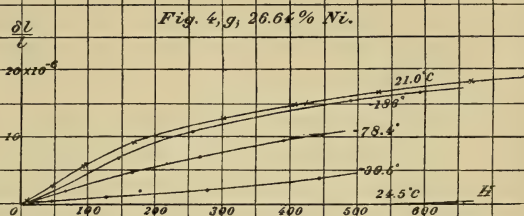


Fig. 6, c.
Nickel.





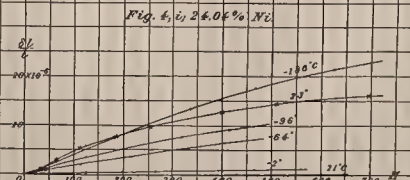
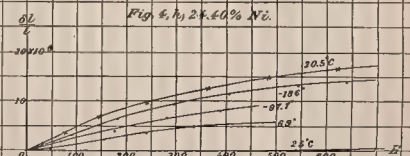
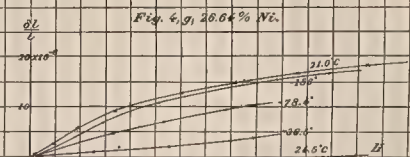
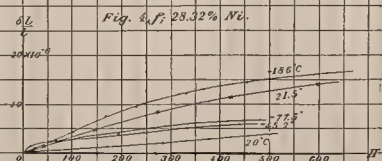
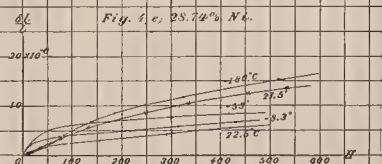
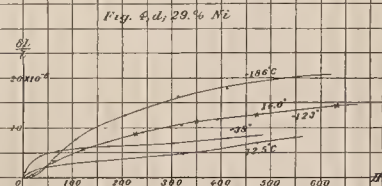
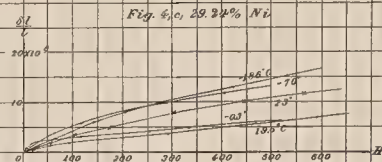
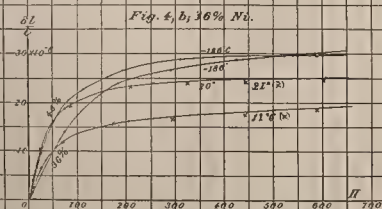
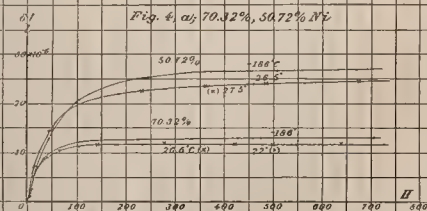
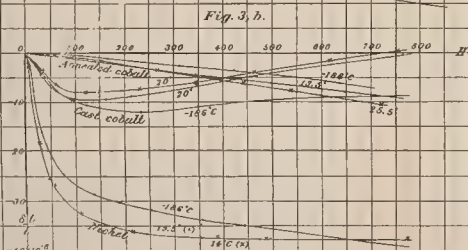
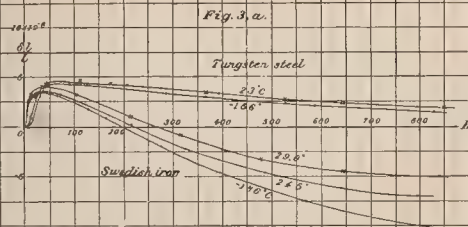


Fig. 2, b. Nickel steels.

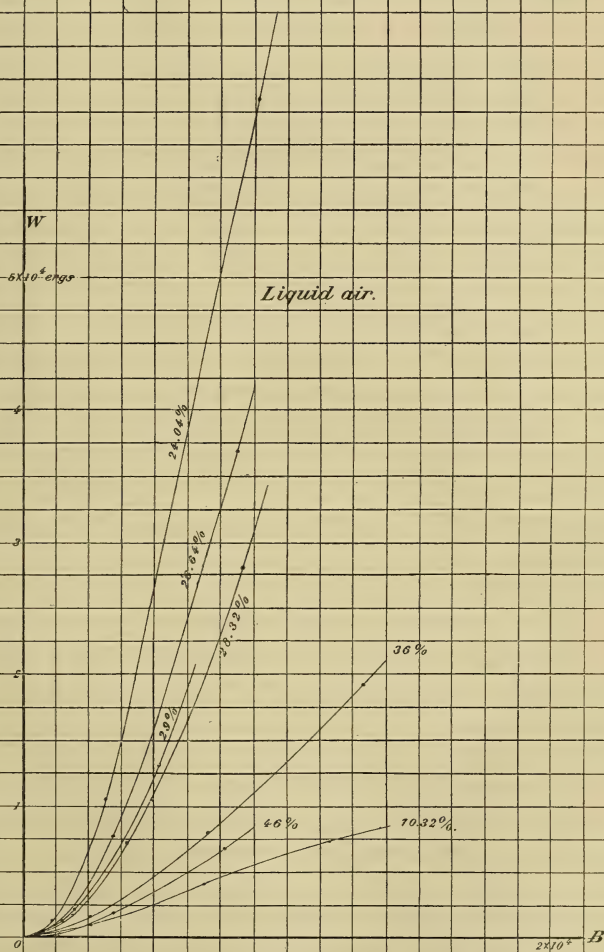


Fig. 1. a.

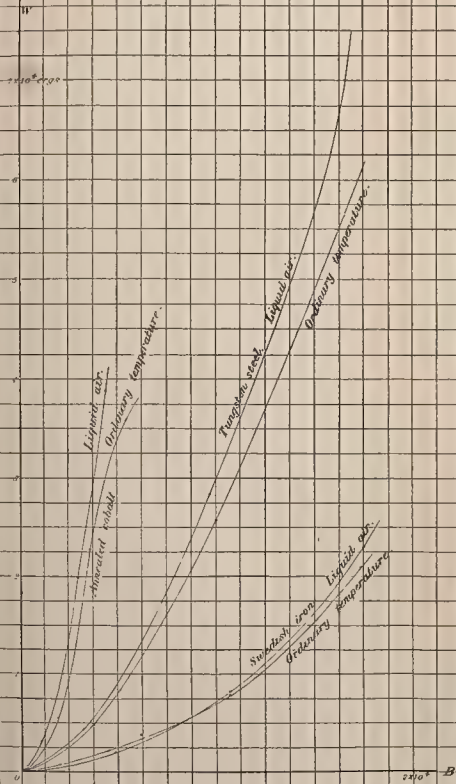


Fig. 1. b.

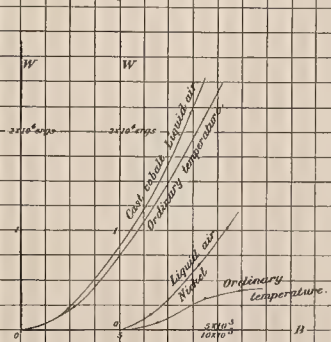


Fig. 2. b. Nickel steels.

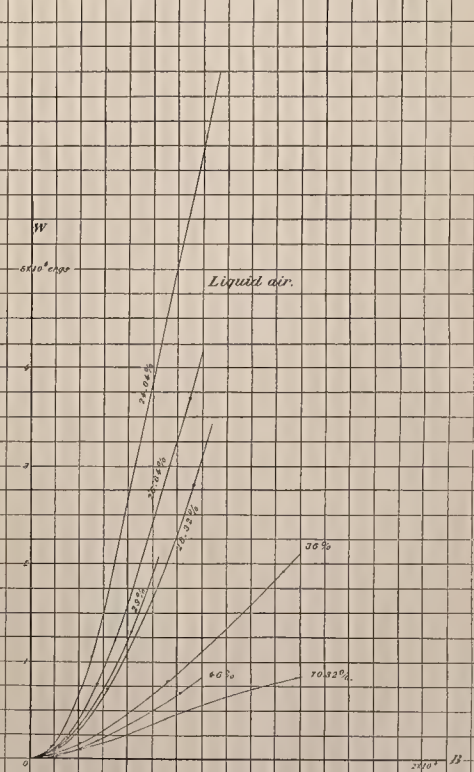


Fig. 7, d ; 36% Ni

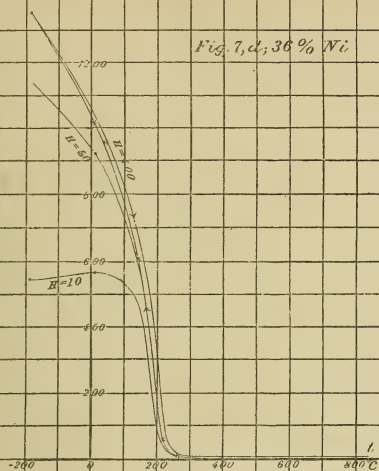
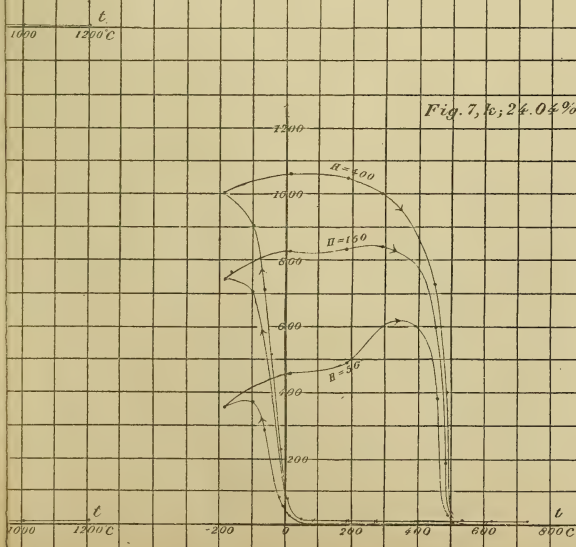
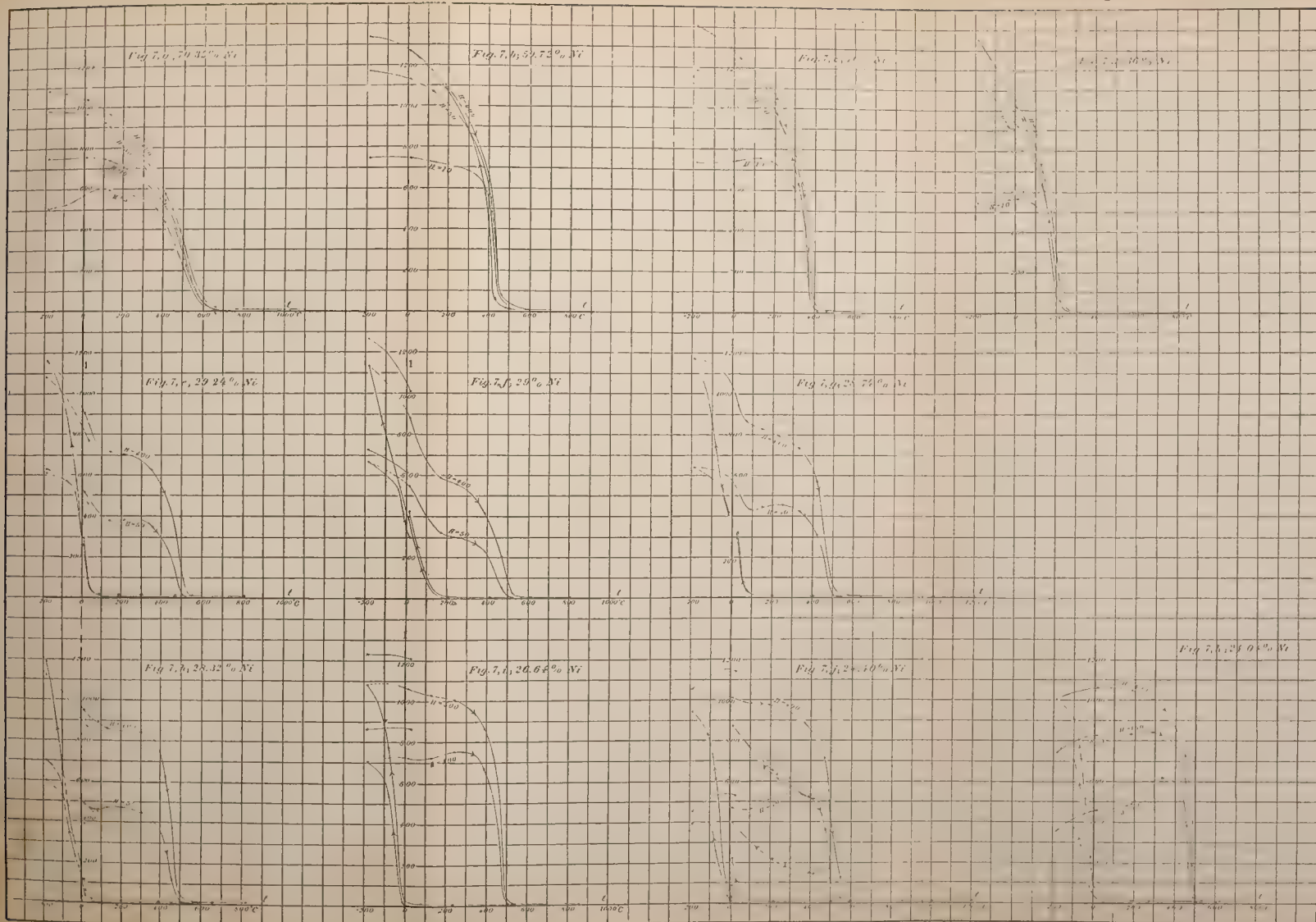


Fig. 7, e ; 24.0% Ni





LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[SIXTH SERIES.]

DECEMBER 1905.

LXX. *The Relation between P.D. and Spark-length for Small Values of the latter.* By GLENN MOODY HOBBS*.

[Plate XIII.]

I. Introduction.

THE general subject of spark-discharge in gases has commanded the attention of investigators for many years, but the recent developments of the electron theory have brought it into especial prominence within the last half-decade. The particular investigations on the spark-discharge between two electrodes and the potential necessary to produce discharge with varying distances have extended over a period of forty-five years, the first observations being made in 1860 by Lord Kelvin †. Since 1880, the field has been covered more rapidly and important investigations have been made by Baille ‡, Liebig §, Paschen ||, Peace ¶, Strutt **, Bouty ††, Earhart ‡‡, and Carr §§. During this period the general behaviour of the discharge between electrodes throughout

* Communicated by the Author.

† Lord Kelvin, "Collected Papers on Electrostatics and Magnetism," p. 247.

‡ Baille, *Annales de Chimie et de Physique* [5] xxv. p. 486 (1882).

§ Liebig, *Phil. Mag.* [5] xxiv. p. 106 (1887).

|| Paschen, *Wied. Ann.* xxxvii. p. 79 (1889).

¶ Peace, *Proc. Roy. Soc.* lii. p. 99 (1892).

** Strutt, *Phil. Trans.* cxci. p. 377 (1900).

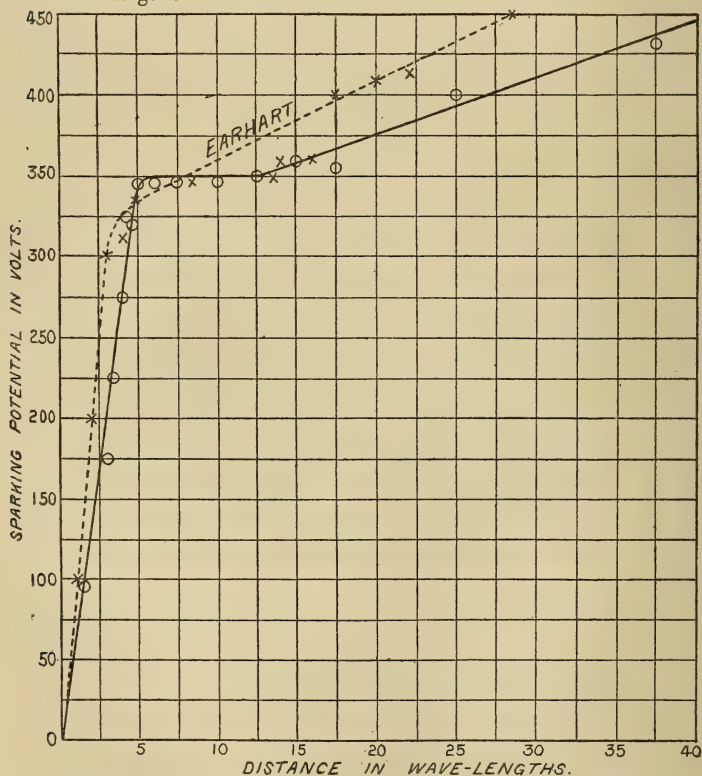
†† Bouty, *Comptes Rendus*, cxxxi. pp. 469, 503 (1900).

‡‡ Earhart, *Phil. Mag.* [6] i. p. 147 (1901).

§§ Carr, *Proc. Roy. Soc.* lxxi. p. 374 (1903).

large ranges of pressures and distance was investigated, and several important laws discovered. The linear relation between spark-potential and distance for comparatively large values of the latter was established, and Peace noticed the existence of a minimum spark-potential. Paschen deduced from a large number of observations the law which bears his name, viz., that for given potential-differences the product of the sparking distance and the maximum pressure for producing a spark is a constant. However, it was not until 1900 that any work was done to prove the correctness of these laws for very small distances. At this time, Earhart carried on an investigation in this laboratory in which, by the aid of the interferometer, he was enabled to make accurate observations down to the point of contact. His results were very interesting and, when plotted with potentials

Fig. 1.—IN AIR AT ATMOSPHERIC PRESSURE.



as ordinates and sparking distances as abscissæ, gave in air at atmospheric pressure a straight-line curve (see fig. 1)

down to a distance of 3μ and a potential of 350 volts. At this point, the curve made a sharp bend and dropped in a straight line to the origin. Earhart also made observations in air at pressures of 228, 152, 40, and 15 cm., and in carbon dioxide at atmospheric pressure.

This investigation was begun in 1902, (1) in order to discover why Earhart's results did not show the minimum which Peace had observed; (2) to ascertain whether or not the material of the electrodes affected the position of the "elbow," as it might be expected to do if the metallic ions took part in the discharge at very small distances; (3) to extend the range of pressures much lower at these small distances than Earhart had done. Since this investigation was begun, there has appeared a paper by Carr* which covers in an admirable manner the third point mentioned above, but leaves the other two points untouched.

It would appear from the following results:—

(1) That the shape of the curve which is obtained with spherical electrodes is indeed precisely what is expected if there is a minimum spark-potential for flat electrodes.

(2) That the material of which the electrodes are composed exerts an important influence upon the spark-potential at small distances.

(3) That the carriers of the discharge for small distances come from the metal and not from the gas.

II. Description of Apparatus.

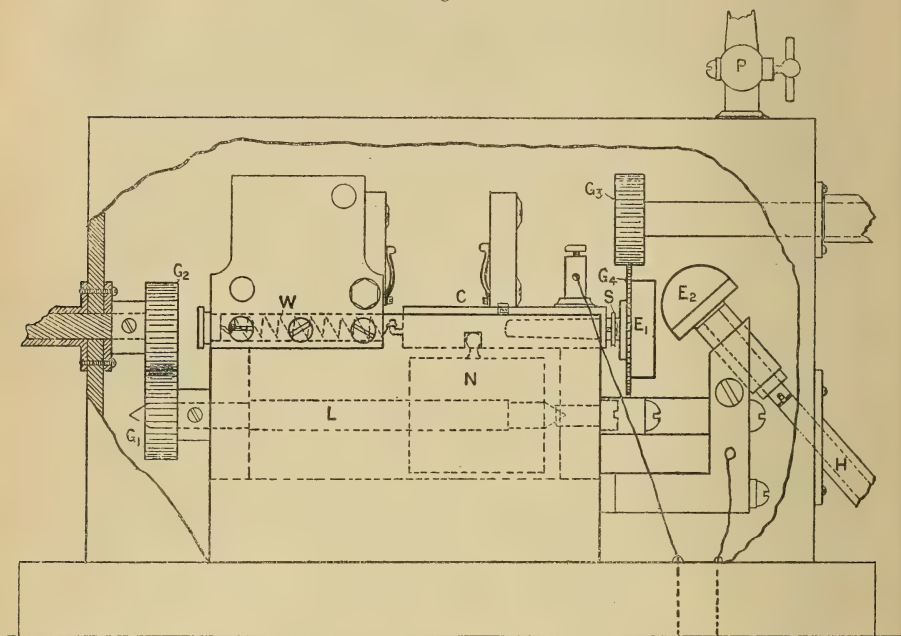
The apparatus, which is shown in fig. 2, consisted of an ordinary interferometer to which the two electrodes were attached. To obviate the necessity of using a tangent-screw and still maintain a sufficient accuracy of setting, the main screw of the interferometer was cut with a fifth-millimetre pitch which made it possible to control variations of a tenth of a fringe (*i. e.* 0.025μ).

The movable carriage (C) of the interferometer supported the plane electrode (E_1), a disk of brass about 1 inch in diameter screwed tightly to a taper shaft which was set in a carefully-ground socket in the carriage and held in place by a spring (S). A thin gear (G_4) $1\frac{1}{2}$ inch in diameter was also concentric on the same shaft and, when engaged in the gear (G_3) above it which was operated outside of the box, could be turned on its axis. In order to get rid of the troublesome backlash in the nut (N), to which the sliding-carriage was fastened, a stiff coiled spring (W) running

* Carr, Proc. Roy. Soc. lxxi. p. 374.

parallel with the screw was attached to the carriage and the latter held in place upon the ways by three spring-clips. This device obviated all drifting contact and made the zero setting very definite.

Fig. 2.

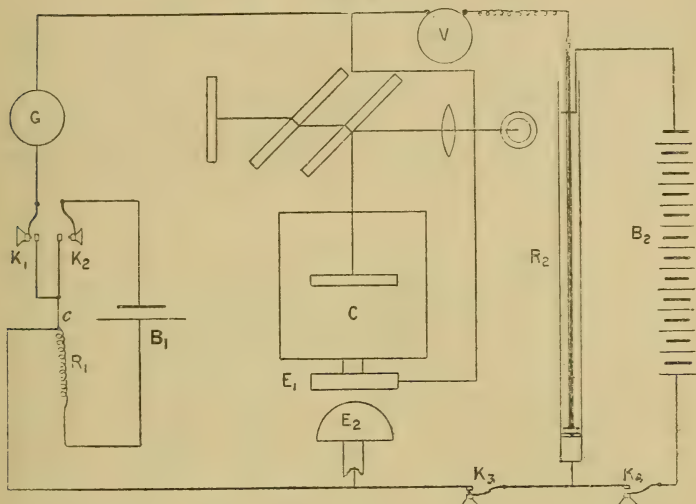


It was found necessary, in order to avoid vibration and similar effects, to attach the support of the spherical electrode to the interferometer-bed itself, insulating it of course so that the circuit would be complete only by the contact of the surfaces. This electrode was also screwed on a taper shaft which turned in a perfectly-ground sleeve, so set that the surface could be rotated on its axis at an angle of 45 degrees from the outside of the box. It is easily seen that by this arrangement the points of contact of both surfaces could be shifted after each discharge, and fifteen or twenty discharges taken before removing the box. This box was made of brass and carried on its sides the shafts of the two gears (G_2 and G_3) by which the rotation of the plane electrode and the main screw (L) of the interferometer was effected. It also carried a socket through which the shaft (H) for turning the spherical surface could be run after the box had been sealed. The joints of the three shafts were made air-tight by pieces of rubber tubing which allowed sufficient rotation for the

necessary adjustments. The box was connected to an air-pump through the stopcock (P), and after sealing the lower edges with bees-wax any desired pressure could be obtained.

A plan of the battery and galvanometer circuits, together with the arrangement of the interferometer mirrors, is shown in fig. 3. The potential was furnished by a battery of about five hundred storage-cells, and the sparking-potential was observed by a direct-reading voltmeter properly standardized.

Fig. 3.



The practice during an experiment was as follows :--The surfaces were carefully polished with very fine emery and rouge, set in position and the fringes adjusted in sodium light. A small quantity of drying substance was placed inside and the box sealed on. The air was pumped out and slowly let in again through a series of drying bottles, and the apparatus allowed to stand for a period of 6 to 24 hours. On beginning the observations, the surfaces were brought into contact by means of the interferometer-screw, the point of contact being shown by the deflexion of a sensitive galvanometer (G, fig. 3) when the spring-keys K_1 and K_2 were pressed. To avoid any possibility of the potential in this circuit effecting a discharge which would tarnish the surfaces, an ordinary dry cell (B_1) was connected to a german-silver resistance-coil (R_1) and a bit of copper wire (c) in series, and the fall of potential across this copper wire was the P.D. applied to the points between which the

discharge was to take place (about 0.01 volt)*. When contact had been established, the reading of the reference point was taken, and the surfaces drawn apart until the required number of fringes had passed the fiducial point. The main discharge circuit was then closed through the switches K_3 and K_4 and the plunger in the water resistance (R_2) raised very slowly until the potential of discharge was reached. In the neighbourhood of the discharge potential the P.D. was raised a volt at a time with a wait of a minute between in order that the effect of lag, first noticed by Warburg†, might be eliminated. The discharge was always indicated by the dropping of the pointer of the voltmeter. The electrodes were then drawn apart and fresh surfaces presented for contact by turning G_3 and H, and after a wait of 8 or 10 minutes to allow the gas to resume its normal condition another discharge was taken. The same pressure was maintained throughout each series of observations.

III. Results in Air with Brass Electrodes.

(1) *Comparison of Curve for Atmospheric Pressure with Earhart's Curve.*—The results with brass electrodes at various pressures are given in Table I., and represented graphically in fig. 4 (Pl. XIII.). In order to make comparisons the more easily, the curve for atmospheric pressure has been plotted with that of Earhart, in fig. 1, the latter being the dotted one. It will be seen that the discrepancy which exists at the "elbow" of the curve is really only apparent; for Earhart's results (indicated by $\times$) show a discharge-potential which is practically stationary at 348 volts for distances from 5λ to 13.5λ . The difference in slope from the elbow to the origin in the two curves is shown by fig. 13 to be due to the metal of which the electrodes were composed, Earhart's surfaces being nickel. The agreement in the case of the curves for 40 and 15 cm. pressure is not so good, the errors in Earhart's observations being due probably to imperfect drying of the air.

* NOTE.—After the departure of the manuscript of this article, the attention of the writer was called to the fact that the indicating potential for different metals should not be below certain minimum values. This explained a difficulty experienced in obtaining definite contact between certain metals, a fact which is mentioned later in the article to explain rather scattered results for zinc, antimony, &c. These curves have been repeated, using a higher indicating potential, and much more consistent results have been obtained. The new results, together with observations on several metals not previously used, will be published in a subsequent article.

† Warburg, *Ann. d. Phys.* vol. lxii. p. 385.

TABLE I.—Brass Electrodes in Air. (Fig. 4, Pl. XIII.)

Pressure Atmospheric.		Pressure 40 cm.		Pressure 25 cm.		Pressure 15 cm.		Pressure 1 cm.	
Dist. in Wave- lgths.	Spark- Poten. in Volts.	Dist. in Wave- lgths.	Spark- Poten. in Volts.	Dist. in Wave- lgths.	Spark- Poten. in Volts.	Dist. in Wave- lgths.	Spark- Poten. in Volts.	Dist. in Wave- lgths.	Spark- Poten. in Volts.
1.5	95	2.0	110	2.5	165	2.0	225	3.0	240
3.0	175	2.0	175	3.0	160	3.0	210	3.0	270
3.5	225	2.5	190	3.0	170	4.0	285	4.0	345
4.0	275	3.0	150	3.0	175	4.5	345	4.5	345
4.2	325	3.2	265	4.0	305	5.0	340	5.0	350
4.5	320	4.0	305	5.0	340	7.5	340	7.5	350
5.0	345	4.5	335	7.5	340	10.0	350	15.0	350
6.0	345	4.7	345	10.0	345	12.5	350	25.0	350
7.5	345	5.0	355	12.5	345	17.5	350	50.0	350
10.0	345	6.0	317	17.5	345	25.0	350	75.0	350
12.5	350	7.5	340	25.0	350	37.5	350		
15.0	360	10.0	340	35.0	350	42.5	350		
17.5	355	10.0	350	37.5	355	50.0	350		
25.0	400	15.0	350	40.0	330	60.0	360		
37.5	430	17.5	350	40.0	370	65.0	375		
		20.0	350	40.0	370	75.0	410		
		22.5	350	42.5	390				
		22.5	345	50.0	390				
		25.0	365	67.5	430				
		27.5	365						
		37.5	375						
		37.5	395						
		50.0	415						
		50.0	420						

(2) *Minimum Spark-Potential for Curved Electrodes.*—

According to Peace and Carr, at a given distance between the electrodes a discharge will take place at the minimum spark-potential (characteristic of the gas) if the pressure has a certain value called the “critical pressure.” Now if either the distance or the pressure be decreased while the other is kept constant, it will be found necessary to increase the potential in order to produce a discharge. These facts were observed with the use of plane electrodes; but where one of the electrodes is spherical as in this case, it will be seen, as indeed Carr pointed out, that when the nearest point of the curved surface is closer to the flat electrode than the distance at which the discharge takes place with a potential equal to the minimum value, the spark can still pass at this potential from a point further out on the curved surface.

It will be seen therefore, that with such electrodes the discharge-potential, instead of passing through a minimum

should remain constant until the electrodes approach so near that the discharge is effected by the metal ions themselves. To take for example the case where the pressure of the gas was 15 cm., as the distance between the electrodes is decreased the sparking potential falls in a straight line to 350 volts, reaching this value when the sparking distance is 57λ (31.6μ). It then remains absolutely constant until the distance has reached 5λ (3μ), when the potential drops rapidly to zero. An examination of the curve for 15 cm. pressure in fig. 4 will show that the results obtained are in perfect accord with this theory.

(3) *Effect of Change in Pressure on the Curves.*—It will be noticed that the curves for the various pressures reach the minimum potential at distances inversely proportional to the pressure. This agrees with Paschen's law which has been beautifully verified by Carr, who showed that for a given potential, *e. g.* the minimum potential, the discharge was only dependent upon the mass of the gas per unit surface between the electrodes. Taking Carr's value of 4.98 mm. for the critical pressure at 1 mm. distance, it is easy to calculate at what points the curves should reach the minimum potential for the various pressures. The following values were obtained:—

Spark-Potential.	Critical Pressure.	Distance in Wave-lengths, calculated.	Distance in Wave-lengths, observed.
350 kil.	75 cm.	13.2	12.5
350	40 "	25	26
350	25 "	40	35
350	15 "	66.6	58
350	1 "	1000	...

The discrepancies in the results for the lower pressures are undoubtedly due to the distortion produced in the field by the spherical electrode, a fact which would seem to indicate a limiting distance of 15μ , within which the field between the electrodes is practically uniform. The minimum spark-potential of 350 volts agrees with the value obtained by Carr and others.

Observations at lower potentials than 150 volts were seldom taken, for the reason that the portion of the curve from this point to the origin has been well explored in an investigation in this laboratory by Professor Kinsley* in connexion with a coherer problem, the apparatus used being susceptible of a higher order of accuracy at these minute distances.

* Kinsley, Phil. Mag. [6] vol. ix. p. 692 (1905).

IV. *Influence of the Kind of Metal in the Electrodes upon the Discharge.*

(1) *Observations in Air at Atmospheric Pressure with different Electrodes.*—In order to investigate the effects of the material of the electrodes upon the spark-potential, which was the primary object of the research, observations at atmospheric pressure similar to those above given for brass surfaces were taken with a series of electrodes of exactly the same size and shape but consisting of the following metals: aluminium, silver, bismuth, zinc, platinum, antimony, magnesium, nickel.

Considerable difficulty was experienced with the crystalline metals, antimony, bismuth, magnesium, and zinc, on account of uncertainty of contact. The results with these metals therefore show much more scattering observations than the others, and the slopes of the curves are consequently somewhat less reliable. The effect on the discharge is nevertheless so unmistakable that the general conclusion is unavoidable. The table and curve of each metal are given below with the exception of brass, which is found in Table I. and fig. 4 (Pl. XIII.). A first and second series of observations for each metal, representing readings taken on widely different days, are given in each table and shown separately on each curve.

TABLE II.—Aluminium. (Fig. 5, Pl. XIII.)

1st Series.		2nd Series.	
Distance, λ.	Potential, volts.	Distance, λ.	Potential, volts.
2	150	2	100
3	210	2.5	140
3	180	2.5	150
4	230	3	150
4	240	3.5	220
4.5	295	4	280
4.5	305	5	320
4.5	290	5.5	350
5	315	6	350
5	315		
6.2	335		
6.2	325		
7.5	335		
7.5	325		
10	345		
12.5	345		

TABLE III.—Silver. (Fig. 6.)

1st Series.		2nd Series.	
Distance, λ .	Potential, volts.	Distance, λ .	Potential, volts.
2	175	1	50
2.5	210	1.5	95
2.7	285	1.5	165
3.5	305	2	190
4	315	2.5	265
4.5	345	3	250
5	350	3	310
		3.5	340
		4	350

TABLE IV.—Bismuth. (Fig. 7.)

1st Series.		2nd Series.	
Distance, λ .	Potential, volts.	Distance, λ .	Potential, volts.
1.5	110	1.5	100
2.2	165	2	160
3	220	2.7	215
3.2	245	3.2	275
3.5	260	3.2	275
3.5	275	4.2	350
4.5	350	3.7	350

TABLE V.—Zinc. (Fig. 8.)

1st Series.		2nd Series.	
Distance, λ .	Potential, volts.	Distance, λ .	Potential, volts.
2	170	1.5	130
2	140	2	220
2	125	2	185
2.5	185	2.5	230
3	170	2.5	235
3	130	3	325
3	230	3	285
3	215	3.5	310
3.5	220	3.5	290
3.5	335	4	260
3.5	335	4.2	315
3.5	285	4.5	350
4	305	4.7	320
4	245	5	350
4.2	285		
4.2	310	4.5	350,
4.2	315	5	225,
4.2	350	6	350

TABLE VI.—Platinum. (Fig. 9.)

1st Series.		2nd Series.	
Distance, λ .	Potential, volts.	Distance, λ .	Potential, volts.
1	110	.9	90
1.5	180	1.5	150
1.5	185	2	350
2	225	2	230
2.2	250	2.5	260
2.5	310	3	350
2.5	280	3	350
2.7	260	4	350
3	280	8	350
3	350	10	350
4	350	12.5	350
		15	360
		25	400
		37.5	435

TABLE VII.—Antimony. (Fig. 10.)

1st Series.		2nd Series.	
Distance, λ .	Potential, volts.	Distance, λ .	Potential, volts.
2	115	1	110
2	245	1.5	185
2.5	270	1.5	125
2.5	250	1.7	285
3	240	2	350
3	220	2	160
3	320	2.5	350
3.5	350	2.5	175
3.5	350	2.5	180
4		3	350
		3	180
		4	350

TABLE VIII.—Magnesium. (Fig. 11.)

1st Series.		2nd Series.	
Distance, λ .	Potential, volts.	Distance, λ .	Potential, volts.
1.5	35	2	170
2	215	2.5	195
2.5	270	2.5	110
2.7	300	3	205
3	155	3.5	275
3.5	260	4	325
4	275	4	335
5	350		

TABLE IX.—Nickel. (Fig. 12.)

1st Series.		2nd Series.	
Distance, λ .	Potential, volts.	Distance, λ .	Potential, volts.
2	170	1.5	180
2.5	220	2	210
3	250	3.2	280
3.7	350	4	350
4	335	4.2	350
4.2	350	5	350
4.5	330		
5	350		
6	350		

(2) *Discharges at less than the Minimum Spark-Potentials carried by Metal Ions.*—If the discharges corresponding to the above curves from the elbow down are carried by the metal ions, it is to be expected that the nature of the electrode would have an effect upon the position of the elbow, for the force which holds the ions within the metals may be assumed to depend upon the nature of the metal. It will be seen that the above curves completely corroborate this inference. The potential begins to fall below the minimum value at the following distances:—

Aluminium	5.6 waves.
Brass	5
Bismuth	4.5
Magnesium	4.5
Zinc	4.5
Nickel	4.2
Silver	3.8
Antimony	3.5
Platinum	3

To illustrate this point the curves previously given are all collected in fig. 13 (Pl. XIII.). It will be observed that in all these cases the curves have been checked by two sets of observations taken at widely different times. With platinum, brass, and nickel, the observations are very definite and the points lie extremely close to the line. For these metals, therefore, the results are thoroughly trustworthy. In the case of the other metals the results are less definite, as the

tables show, but the slopes obtained by the two series of observations are nevertheless in good agreement.

Further evidence that the carriers of the discharge within the limits specified come from the metal and not from the gas, is found in the fact that the slope of the curve below the elbow is completely independent of the pressure of the gas, as is shown in fig. 4, and also independent of the nature of the gas, as shown in fig. 9.

Furthermore, the character of the discharge as observed in the behaviour of the voltmeter is completely different below the elbow from its character on that portion of the curve to the right of the elbow. When the discharge took place at or above the minimum spark-potential, the pointer of the voltmeter experienced a sudden deflexion of a number of divisions towards zero, and quickly returned to the old value as the potential was built up by the cells. On the other hand, when the discharge took place at points on the curve below the elbow, the pointer always fell completely to zero and remained there. Moreover, a test of the circuit with the galvanometer showed that coherence had taken place. That the coherence was not due to any give in the supports because of electrostatic attraction between the electrodes is definitely proved by the facts presented in figures 9 and 13, showing that the elbow varies with the nature of the electrode and with the nature of the gas. (This last result is discussed in the following section.)

(3) *Observations in Hydrogen and Carbon Dioxide at Atmospheric Pressure with Platinum Electrodes.*—In order to check still further the participation of the metal ions in the discharge at small distances, some observations were taken with platinum electrodes in an atmosphere of hydrogen and of carbon dioxide. The results are shown in Table X. and graphically in fig. 9 (Pl. XIII.).

The minimum spark-potentials for these gases are, according to Carr, respectively 280 and 420, while according to these results they are 285 and 420. Now if the metal ions are the carriers of the discharge for all potentials below the minimum value for the gas, then the curve in any gas should follow the slope found for air until the potential reaches the minimum value for the gas, when the curve should bend to the horizontal, *i. e.* the discharge should at this point begin to take place in the gas. This must be true, for if the curve should hold to the straight line beyond this point, the discharge would be produced by the metal ions at a higher potential than was necessary for discharge at the same distance in the gas itself. Taking Carr's values for the critical pressure for

TABLE X.—Platinum in H and CO₂.

Hydrogen.		Carbon dioxide.	
Distance, λ .	Potential, volts.	Distance, λ .	Potential, volts.
1.5	175	1.2	140
2	242	2	255
2.5	275	2.7	320
2.5	275	3.4	390
3	280	3.7	420
4	285	5	420
6	285	7.5	420
10	285	10	420
12.5	285	12.5	420
15	285	15	430
29	290	25	465
37.5	320	37.5	515
50	365		

hydrogen, 10.3 mm. at 1 mm. distance and 280 volts, and for CO₂, 5.03 mm. at the same distance and 420 volts, the calculations can be made for the distances at which the curves should rise above the minimum potential abscissæ. These calculations give 28λ for hydrogen and 12.5λ for CO₂, which agree almost exactly with the experimental values obtained (see fig. 9).

(4) The lag which is characteristic of the discharge at higher potentials disappears entirely for distances below the elbow.

V. *Summary.*

(1) With one spherical and one plane electrode, at constant pressure the spark-potential is directly proportional to the distances between the electrodes, until the potential reaches the minimum value for the gas.

(2) In any gas the potential of discharge reaches its minimum value for the gas at distances which are inversely proportional to the pressures existing between the electrodes.

(3) For the same electrodes the discharge in air at distances from zero to about 3μ is wholly independent of the pressure or of the nature of the gas between the electrodes.

(4) For the same electrodes the distances at which the curves assume a horizontal slope are proportional to the minimum spark-potential of the gas between the electrodes.

(5) When a discharge of electricity occurs between two

electrodes at a lower potential than the minimum spark-potential of the gas in which the discharge occurs, the discharge is produced wholly or in part by the metal ions.

In conclusion I desire to thank Professor Michelson for his kindly interest in my work and for several helpful suggestions.

I wish also to thank Professor Millikan for suggesting the problem and for his valuable counsel throughout this work. I can not adequately express my deep appreciation of his efforts in my behalf.

Ryerson Laboratory,
University of Chicago.
April 1905.

LXXI. *The Thermoelectric Circuit of Three Metals.*
By H. E. SCHMITZ, M.A., B.Sc.*

§ 1. *Introduction.*

THE experiments described in the present paper were undertaken at the instance of Professor E. Riecke, and were carried out in the Physical Institute of the University of Göttingen during the months November 1904 to April 1905.

Let a circuit be formed of three metals A, B, C; and let the junctions opposite these metals be at temperatures (centigrade) represented respectively by t_1, t_2, t_3 . If we assume that the electromotive force E of such a circuit depends only on the temperatures of the junctions, we may suppose intermediate points to be at 0°C. , and may therefore write

$$E = (B, C)_0^{t_1} + (C, A)_0^{t_2} + (A, B)_0^{t_3}, \quad . \quad . \quad . \quad (1)$$

where $(B, C)_0^{t_1}$ is the electromotive force of a circuit formed of the metals B, C when the junctions are at temperatures 0°C. and $t_1^\circ \text{C.}$, reckoned in the direction B to C through the junction at t_1° , and E is reckoned in the direction ABC.

If we further assume that the electromotive force of the circuit is zero when all the junctions are at the same temperature t , we have

$$(B, C)_0^t + (C, A)_0^t + (A, B)_0^t = 0. \quad . \quad . \quad . \quad (2)$$

By the use of this equation we may eliminate $(C, A)_0^{t_2}$ from equation (1), and may write

$$E = (B, C)_0^{t_1} - (B, C)_0^{t_2} + (A, B)_0^{t_3} - (A, B)_0^{t_2}. \quad . \quad . \quad (3)$$

* Communicated by the Author. A short abstract of this paper has appeared in the *Physikalische Zeitschrift* (July 1905).

Various formulæ have been proposed as the expression of the electromotive force of a circuit of two metals in terms of the temperatures of the junctions. The simplest of these is the parabolic formula, in accordance with which we may write :—

$$\left. \begin{aligned} (B, C)_0^t &= a_1 t + \frac{1}{2} b_1 t^2, \\ (C, A)_0^t &= a_2 t + \frac{1}{2} b_2 t^2, \\ (A, B)_0^t &= a_3 t + \frac{1}{2} b_3 t^2. \end{aligned} \right\} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Using these relations, equation (1) assumes the form

$$E = a_1 t_1 + a_2 t_2 + a_3 t_3 + \frac{1}{2} b_1 t_1^2 + \frac{1}{2} b_2 t_2^2 + \frac{1}{2} b_3 t_3^2. \quad (5)$$

To satisfy equation (2) we must have

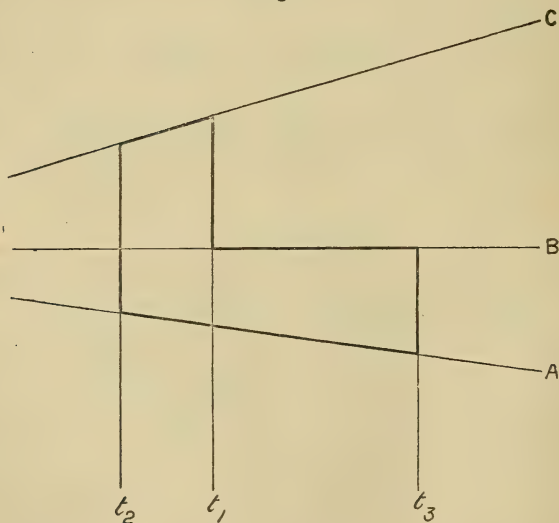
$$a_1 + a_2 + a_3 = 0$$

$$\text{and} \quad b_1 + b_2 + b_3 = 0.$$

Using these relations to eliminate a_2 and b_2 , equation (5) assumes the form corresponding with equation (3), namely :

$$E = a_1(t_1 - t_2) + a_3(t_3 - t_2) + \frac{1}{2} b_1(t_1^2 - t_2^2) + \frac{1}{2} b_3(t_3^2 - t_2^2). \quad (6)$$

Fig. 1.



Equations (4) imply that the lines of the thermoelectric diagram for the metals A and C are straight if the line for the metal B is straight. The diagram is given in fig. 1 for the case where a_1, a_3, b_1, b_3 are all positive and $t_3 > t_1 > t_2$.

The value of E is represented by the area enclosed by the thickened lines. Equation (6) can be written down from inspection of the diagram.

Whatever views may be held as to the ultimate causes of thermoelectric phenomena, the experimental evidence for the assumptions on which equations (1) and (2) depend is too strong to be called in question. On the other hand, the researches of Tait*, Dewar and Fleming†, Holborn and Day‡, and others show that equations (4) do not hold for all metals at all temperatures. In the present paper, the writer confines himself to the application of equations (4), (5), and (6) to the experimental results; and it will be seen that, within the limits of experimental error, these equations are confirmed in the cases examined.

The circuits to which the measurements refer are formed from the metals German silver (A), copper (B), and iron (C), and the limits of temperature are 0°C . and 80°C .

§ 2. Standards.

The ultimate standard of electromotive force for the present measurements was a Weston cell (No. 445) made by the European Weston Company, Berlin; the electromotive force was taken at its certificate value, $1\cdot018$, volts (19°). Four Weston cells were made in accordance with the instructions of W. Jaeger§, except that, following the later practice of the Reichsanstalt, the cadmium amalgam was composed of 12 parts cadmium and 88 parts mercury. Measurements of the electromotive force of these cells, (a) soon after construction, (b) after completion of the thermoelectric experiments, gave the values shown in the following table:—

	(a) 25th Feb.	(b) 15th April.
Cell No. 1	1·0194	1·01935
„ 2	1·0193	1·0193
„ 3	1·0194	1·0194
„ 4	1·01945	1·0194

Cells No. 1, 2, 4 were combined in series, the total electromotive force being assumed $3\cdot0582$ volts; a temperature correction was unnecessary. Cell No. 3 was used to produce an occasional current through 100,019 ohms; wires from

* Trans. R. S. E. vol. xxvii. p. 125 (1876).

† Phil. Mag. vol. xl. p. 95 (1895).

‡ Berl. Sitzungsber. vol. ii. p. 691 (1899).

§ Elektrotechn. Zeitschr. 1897.

points on this circuit separated by 3019 ohms gave a practically constant electromotive force of about $\cdot 03$ volt, serving, in a manner which will presently appear, to standardize the apparatus for measurement of thermoelectromotive forces.

The thermometers on which the main temperature measurements depend were two, graduated respectively from -15° C. to $+60^{\circ}$ C., and from $+50^{\circ}$ C. to $+120^{\circ}$ C., the length of the degree being about 5 mm.; they were manufactured from Jena glass by F. O. R. Goetze. In what follows these thermometers are called respectively G_0 and G_1 . Comparisons with a standard mercurial thermometer gave the following *approximate* results:—

At 20°	G_0 reads	$\cdot 06^{\circ}$	above the gas-thermometer,
„ 60°	G_1 „	$\cdot 06^{\circ}$	„ „ „
„ 80°	„ „	$\cdot 01^{\circ}$	„ „ „

The chief temperatures of observation were chosen with the view of minimizing the importance of any error in the correction for the emergent column; this correction had the following mean values:—

$+\cdot 02^{\circ}$	at	20°	(G_0),
zero	at	60°	(G_1),
$+\cdot 20^{\circ}$	at	80°	(G_1).

After the conclusion of the experiments the thermometers were examined at the Reichsanstalt, with the following results:—

At 20°	G_0 reads	$\cdot 11^{\circ}$	above the gas-thermometer,
„ 60°	G_1 „	$\cdot 08^{\circ}$	„ „ „
„ 80°	„ „	$\cdot 03^{\circ}$	„ „ „

The differences here indicated are, very nearly, the true differences between the scale of Jena glass and the scale of the gas-thermometer, the inference being that the actual reading of the thermometer is, very nearly, the true temperature on the scale of Jena glass. In what follows the thermometers are taken as correct, and the temperature-scale employed is therefore *that of Jena glass*.

§ 3. Method.

The observations were directed to determine (1) the electromotive force of each pair of metals between 0° and 20° , between 0° and 60° , and between 0° and 80° ; (2) the electromotive force of the circuit of three metals, the temperatures

of the three junctions being respectively 0° , the temperature of the water-supply (read on G_0), and 60° (read on G_1).

The thermostat for maintaining the temperature 0° was a vessel of diameter 10 cm. packed with broken ice and filled up with water*. The depth of the thermo-junction below the surface of the ice was about 8 cm.

The thermostat for maintaining a temperature of 20° , 60° , or 80° was a closed metallic vessel of diameter 8 cm. and depth 15 cm., nearly full of water. A powerful stirrer in the base of the vessel, mechanically driven at the rate of 50 to 100 revolutions per minute, together with suitable breakwaters in the interior, ensured a uniform temperature. The vessel was, so far as possible, thermally insulated. A coil conveying an electric current projected into the interior of the vessel; the current was so regulated as to produce a temperature within half a degree of that desired for half-an-hour previous to an observation of electromotive force, and within one-tenth of a degree for five minutes previous to an observation. In most cases an observation was made while the temperature was very slowly rising. The depth of the thermo-junction, which was arranged in the close neighbourhood of the thermometer-bulb, was about 8 cm. below the top of the lid of the vessel†.

The third thermostat was a vessel of diameter 7 cm. and height 13 cm. Water was introduced at the base and taken off at the brim. The thermo-junction and thermometer-bulb were about 8 cm. below the water-level.

The thermo-elements were in the first instance constructed from soft wires of copper (believed to be pure), iron (believed to be pure), and German silver, each of diameter about 1.5 mm. For a second series of experiments these wires were drawn to a diameter of 0.5 mm.

The electrical connexions are shown diagrammatically in fig. 2. AB is a resistance of 10,000 ohms; BB' an adjustable resistance. CD is a resistance of 10,000 ohms; EF a constant resistance of approximately 100 ohms. Connexions are made *either* between $X_1 Y_1$, $X_2 Y_2$, $X_3 Y_3$, $X_4 Y_4$, *or* between $Y_3 Z_3$,

* Ice and water is much more satisfactory for the maintenance of a constant temperature than ice alone.

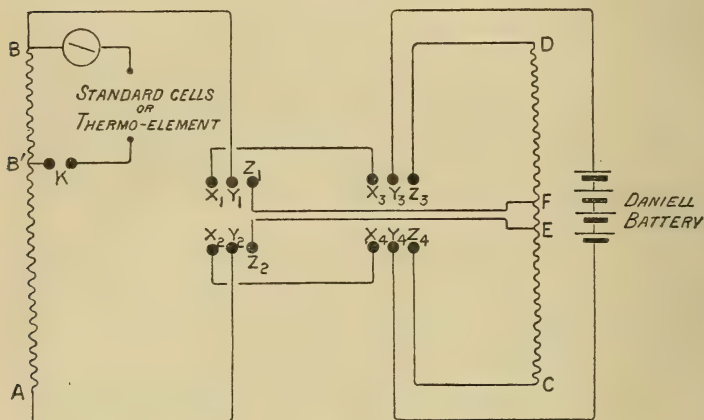
† In control experiments the water in this thermostat was replaced by paraffin, and in other experiments the first thermostat was modified by the introduction of a small vessel of paraffin (well stirred) surrounding the thermo-junction and the base of the thermometer. The use of paraffin in the latter case necessarily gave a somewhat less reliable value for the temperature of the thermo-junction. The results obtained agreed, within the limits of experimental error, with the results of the experiments in which water was used.

$Y_4 Z_4$; the battery current is never interrupted except at the moment of commutation.

Let the first system of connexions produce a current i through the compensation circuit $BB'A$. The second system of connexions either produces no current through the circuit $BB'A$, or, by joining $Y_1 Z_1$, $Y_2 Z_2$ as well as $Y_3 Z_3$ and $Y_4 Z_4$, produces a current approximately $\frac{i}{100}$.

Meanwhile the battery is producing a current through a resistance which never differs much from 10,000 ohms. The larger current is used when the standard cells are to

Fig. 2.



be balanced, the smaller when the thermo-element is to be balanced.

It was undesirable to make the commutation at frequent intervals, and it was therefore of special importance to be able to assume constancy of the auxiliary battery during periods of an hour or two. Accumulators were tried under various conditions, but the rate of fall of electromotive force was in all cases too great. Finally, four large Daniell cells, each containing two litres of saturated copper-sulphate solution and about one-tenth that volume of zinc-sulphate solution of density 1.2, were employed. These could be trusted to give a nearly constant current for several weeks together, the variation during the course of a day being as a rule exceedingly small. In this connection the following record may be of interest:—

Date.	Laboratory temperature.	Resistance compensating E.M.F. of three Weston cells.
Mar. 23		7023 (a few minutes after completion of the circuit) rising to 7044.
„ 24	11°	7050, 7051, 7049·5.
„ 25	11°-13°	7051·5, 7050, 7051, 7051·5.
„ 27	11°·5	7053 and (after opening the circuit for two hours) 7040.
„ 28	11°	7052, 7053.
„ 29	10°-11°	7052·3, 7052·0, 7052·4, 7051·5.
„ 30	11°-12°	7048, 7047, 7046.
„ 31	12°-15°	7047, 7046, 7045.
Apr. 4	10°	7059·5.
„ 5		7056.
„ 11	14°	7056.
„ 12		7049, 7051·5.
„ 13	15°-16°	7051, 7052, 7051.
„ 14	16°	7052·5, 7052.
„ 15	13°-14°	7056, 7057·5.

Let W be the E.M.F. of the three Weston cells (see § 2) and let r be the value of the compensating resistance, so that $W = ri$.

Let E be the E.M.F. of the thermo-circuit, and let s be the value of the compensating resistance. Let the current used in this case (approximately $\frac{i}{100}$) be represented by xi , so that $E = sxi$.

E is to be determined from the equation

$$\frac{E}{W} = x \frac{s}{r},$$

where x is a fraction depending on the resistances of the circuit of fig. 1.

For the determination of x , Weston cell No. 3 was allowed to produce a current in the manner already defined, and the compensation apparatus was used to balance a definite fraction of the E.M.F. of the cell, the current flowing through the compensation apparatus being alternately i and xi . The following specimen of the observations shows the order of magnitudes involved:—

Compensating resistance in ohms (large current).	Compensating resistance in ohms (small current).
70·62	
.....	7058
70·61	
.....	7058
70·61	
$x = \cdot 010004$	

The values of W , r , and x could all be determined with a high degree of accuracy; the accuracy of the value of E depends therefore mainly on the measurement of s . If z is the least recognizable current through the galvanometer and g the resistance of galvanometer and thermo-circuit, then we find

$$E - sxi = \pm (g + s)z,$$

whence proportionate error in $E = \frac{g+s}{s} \cdot \frac{z}{xi}$. In the present measurements it was found convenient to use a moving-coil galvanometer, in spite of its high resistance (600 ohms). The value of s was from about 50 ohms to about 400 ohms. Taking z as the current corresponding with a deflexion of a millimetre on the galvanometer scale, its value was $\frac{3}{10^{10}}$ ampere. Approximate values for x ($\frac{1}{100}$) and i ($\frac{3}{7000}$ ampere) give $\frac{7}{10^5}$ for $\frac{z}{xi}$. Hence, taking the maximum value (13) for $\frac{g+s}{s}$, we find as the proportionate error in E (or in s) 1 in 1000.

This result assumes absence of current through the galvanometer when the galvanometer circuit is completed by closing the key K after removing all sources of electromotive force, a condition not easy to realize in practice. In the earlier measurements the resistance AB (fig. 1) was provided by a new compensation apparatus of the type described in the *Physikal. Zeitschr.* vol. i. p. 167. The fractions of an ohm in the instrument used were provided by exposed wires of "Kruppin," and the various external and internal contacts almost always produced an appreciable thermo-voltage apparently not avoidable by any precautions. In the later measurements the resistance AB was provided by two new "precision" resistance-boxes (Siemens & Halske, No. 19530 and No. 19531) reading from 0.1 ohm upwards. This arrangement gave much more satisfactory results. The chief error was now due to the key K , which in this case consisted of two mercury cups connected by a copper wire (unfortunately not well amalgamated) attached to a sealing-wax handle. It was found that a current, large enough to cause a considerable error in the measurements, might be produced at the contact of copper and mercury if a fresh surface of mercury were not constantly exposed.

§ 4. Results.

The results of the experiments are given in the following tables. Tables I A. and I B. give the results of final experiments made with the thicker (soft) wires of the three metals; the results of preliminary trials with these wires are omitted. Tables II A. and II B. give the results of all the experiments made with the thinner (hard) wires. In view of the possibility of an alteration in the thermoelectric force of a given junction*, it may be well to state that the observations recorded in Table II B. were made before those recorded in Table II A. Owing to the removal of the Physical Institute to a new building the writer was unfortunately unable to carry out his intention of making further experiments with a view to the elucidation of this point. He is however distinctly of opinion that the thermoelectric "constants" are (within the range 0° C. to 100° C.) really constant, and that anomalies such as those recorded by Steele (*loc. cit.*) are due to difficulties of observation. The agreement between observed and calculated results is much better in Table II A. than in Table II B. This is probably due, not to any alteration in the junctions, but chiefly to the better avoidance of the error (previously spoken of) due to the mercury key.

TABLE I A.—Soft Wires.

Electromotive force in microvolts of pairs of metals.

	E.M.F. observed.			E.M.F. calculated *.
	Exp. 1.	Exp. 2.	Mean.	
Copper-Iron, $\hat{0}-20$	231.5	234	233	234.2
" $0-60$	665.5	670.3	668	666.4
" $0-80$	864	865	864.5	864.5
Iron-G.S., $\hat{0}-20$	- 456	...	...	- 457.2
" $0-60$	-1384	...	...	-1382.8
" $0-80$	-1851	...	...	-1851.0
G.S.-Copper, $\hat{0}-20$	223	226	224.5	223.6
" $0-60$	712	718	715	716.3
" $0-80$	986	985	985.5	985.4
Copper-Iron	$a = +12.01,$		$b = -.0301.$	
Iron-G.S.	$a = -22.77,$		$b = -.0092.$	
G.S.-Copper	$a = +10.80,$		$b = +.03795.$	

* Method of least squares.

* See for instance Wiedemann, *Elektricität*, ed. 2, art. 337; also W. H. Steele, *Phil. Mag.* vol. xxxvii. p. 221 (1894).

TABLE I B.—Soft Wires.

Electromotive force in microvolts of circuit of three metals.

Temperature of Junction.			Electromotive Force.	
Copper-Iron.	Iron-G.S.	G.S.-Copper.	Observed.	Calculated *.
0	0	0	+ 491·7	+ 487·2
60	7·85	0	+ 753·6	+ 753·9
8·18	0	60	+ 816·5	+ 813·5
0	8·20	60	+ 533·35	+ 528·9
0	60	8·28	-1292·6	-1292·0
8·31	60	0	-1275·7	-1284·0

* From equation (6), which is here used since the values a_1, b_1, a_3, b_3 for Copper-Iron and G.S.-Copper are deduced from more observations than the values a_2, b_2 for Iron-G.S.

TABLE II A.—Hard Wires.

Electromotive force in microvolts of pairs of metals.

	E.M.F. observed.	E.M.F. calculated *.
Copper-Iron 0-20	224·5	225·3
„ 0-60	639·0	638·3
„ 0-80	825·4	825·9
Iron-G.S. 0-20	- 460·3	- 459·8
„ 0-60	-1390·8	-1391·4
„ 0-80	-1863·3	-1863·1
G.S.-Copper 0-20	237·5	237·4
„ 0-60	756·05	756·2
„ 0-80	1037·5	1037·5
Copper-Iron..... $a = +11·580, b = -·03142$		
Iron-G.S. $a = -22·891, b = -·00995$		
G.S.-Copper $a = +11·506, b = +·03657$		

* Method of least squares.

TABLE II B.—Hard Wires.

Electromotive force in microvolts of circuit of three metals.

Temperature of Junction.			Electromotive Force.	
Copper-Iron.	Iron-G.S.	G.S.-Copper.	Observed.	Calculated *.
60	0	0	+ 441·3	+ 444·1
60	8·46	8·56	+ 728·4	+ 738·0
9·01	0	60	+ 878·2	+ 859·2
0	8·26	60	+ 577·1	+ 566·8
0	60	8·30	-1298·2	-1294·6
8·38	60	0	-1283·9	-1295·5

* From equation (5).

It is well-known that the thermoelectric properties of a metal depend to a marked degree on its purity and on its state of aggregation. The comparison of the constants given in the present paper with those obtained by other observers has therefore no special value. But a rough comparison is not without interest, and the following short table is therefore appended. The unit of electromotive force for this table is the microvolt. Kohlrausch's copper was electrolytic. Tait's copper was probably impure*. Noll's copper was pure. Tait used pure iron, but not Noll; Tait found that different specimens of iron had thermoelectric properties intermediate between those of pure iron and steel. The numbers giving the results of Kohlrausch and of Noll are from direct observations with the couples mentioned; those giving Tait's results are derived from Tait's thermoelectric diagram. Noll experimented with both soft and hard wires; his results would point to slightly higher values for hard than for soft wires in both the cases quoted.

Couple.	<i>a.</i>	<i>b.</i>	Observer.	Remarks.
Copper-Iron.	+10.2	−.030	Kohlrausch. ¹	Hard-drawn wires (16°–80°)
Iron-G.S.	−24.1	−.039	"	" " "
G.S.-Copper.	+14.6	+ .058	"	" " "
Copper-Iron.	+16.0	−.058	Tait. ²	
Copper-Steel.	+10.0	−.042	"	
G.S.-Copper.	+13.4	+ .061	"	Constants for G.S. alter above 150°.
Copper-Iron.	+10.5	−.039	Noll. ³	Soft wires (0°–182°).
G.S.-Copper.	+16.1	+ .036	"	" " (0°–216°).
Copper-Iron.	+12.0	−.030	Schmitz.	Soft wires (0°–80°).
Iron-G.S.	−22.8	−.009	"	" " "
G.S.-Copper.	+10.8	+ .038	"	" " "
Copper-Iron.	+11.6	−.031	"	Hard wires (0°–80°).
Iron-G.S.	−22.9	−.010	"	" " "
G.S.-Copper.	+11.5	+ .037	"	" " "

¹ Pogg. *Ann.* vol. cxli. p. 456 (1870), also Pogg. *Ann. Erg.* vol. vi. p. 35 (1874). The above numbers are calculated from those given by Kohlrausch on the assumption that the lower temperature in the experiments was 16°. A further reduction of all six numbers by about 3 per cent. is (strictly speaking) necessary to bring them to microvolt-units.

² Trans. Roy. Soc. Edin. vol. xxvii. p. 125 (1876), also Everett's 'Units,' ed. 1886.

³ Wied. *Ann.* vol. liii. p. 874 (1894).

In conclusion, it is not without interest to compare the differences between soft and hard wires, as found in the present investigation, with those observed by other experimenters. In constructing the following short table, the

* Steele, *loc. cit.*; Noll, Wied. *Ann.* vol. liii. p. 874.

values given by Noll (*loc. cit.*) and by Maclean* for “undrawn” and “drawn” wires have been made use of. Maclean’s “annealed steel” is taken as the equivalent of iron.

Excess of “Hard” over “Soft.”

	Noll.	Maclean.	Schmitz.
Copper-Iron	+2 per cent.	−1 per cent.	−4½ per cent.
G.S.-Copper	+2 per cent.	+3 per cent.	+5½ per cent.
G.S.-Iron	+2 per cent.	+½ per cent.	+ ½ per cent.

LXXII. *On the Magnetization and the Magnetic Change of Length in Ferromagnetic Metals and Alloys at Temperatures ranging from -186° C. to $+1200^{\circ}$ C.* By K. HONDA, *Rigakuhakushi, Lecturer in Physics, Tokyo University*, and S. SHIMIZU, *Rigakushi, Lecturer in Physics, High Normal School.*

[Concluded from p. 574.]

III. THIRD SERIES.

IN the third series of experiments the magnetization was measured at different stages of ascending as well as descending temperatures, the measurement of the change of length by magnetization being left for future experiments.

The heating was effected by means of an electric current; a porcelain tube (external diam.=1.7 cm., internal diam.=1.05 cm., length=47 cm.) was covered with a few layers of asbestos paper, and the lower part (36 cm.) was wound anti-inductively with a platinum wire 0.4 mm. thick at the rate of 2 turns per cm. It was then wrapped in asbestos papers to a thickness of about 5 mm. To the upper end of the porcelain tube a brass flange was fixed, while a short porcelain cylinder was inserted tightly in its lower end so as to arrest air currents. The length of this cylinder was so chosen that when the tube was placed in the right position in the central line of the magnetizing-coil the ovoid occupied the central position of the coil. The magnetizing-coil was provided with a water-jacketed arrangement and a coil for the compensation of the vertical component of the earth’s field.

The temperature of the ovoid was measured with a platinum rhodium-platinum junction. One of the junctions was placed in contact with the specimen at a point a quarter of the

* Proc. Roy. Soc. vol. lxiv. p. 322 (1899), and vol. lxvi. p. 165 (1900).

distance from the upper end of the ovoid, the rest being well insulated with asbestos paper. The interspace between the leading wires and the wall of the porcelain tube was tightly filled with asbestos fibres, and thus protected as much as possible from convection currents. The other junction was arranged as in the second experiment. The thermoelectric current was measured with a d'Arsonval galvanometer from Keiser and Schmidt, the reading of which was corrected by the authors with a mercury thermometer containing nitrogen below 550° C., and by Professor Nagaoka and Mr. S. Kusakabe with the melting-point of sodium chloride. A low-resistance galvanometer was, at the same time, employed to measure the temperatures lower than 200° C. A simple connexion permitted us to pass the thermoelectric current through the d'Arsonval or the low-resistance galvanometer as the case might be.

The experiment was conducted in the following order:—The adjustments of the magnetometer and the coils, as described in the first experiment, were effected; the heating-coil with the specimen was then placed in the right position. The magnetization at the temperature of the room was first determined; then a current from a dynamo was passed through the heating-coil, till the temperature of the specimen became constant. The direct effect due to the current in the heating-coil was tested by breaking or reversing the current. The small deflexion of the magnetometer, when there was any, was completely eliminated by altering the form of the leading wires. The demagnetization by reversals, while the heating-current was passing, showed no trace of residual magnetism, which indicates that the magnetization due to the heating-current was insensibly small. When the temperature became constant, the magnetizations at gradually increasing fields were measured. Another stronger current was next sent through the heating-coil and the same processes were repeated as before. In this way, we measured the magnetization in the stage of ascending temperature and then that in the descending stage. During each set of observations the temperature was fairly constant, and even in very unfavourable cases the variation did not exceed 2 degrees. The temperature was always noted both before and after each experiment and the mean was taken. When a series of experiments was finished, the specimen was taken out of the coil and the compensation tested. Excepting in a few cases, we found the compensation undisturbed; when, however, the disturbance was such as to require a correction, it was uniformly distributed.

In the present experiments the strength of the heating-current and the temperature thereby caused were as follows:—

Current	1.8 amp.	2.9	3.8	4.7	5.5
Temperature ...	100° C.	300	600	900	1200

The heating of nickel-steels protected in the manner above described showed only a trace of surface-oxidation, if the temperature did not exceed about 800° C. If, however, the temperature was raised to 1200° C. the surface-oxidation became considerable, so that the heating of some specimens was stopped at about 800° C., if there was nothing of special importance to be gained by heating them above that temperature.

(a) *Magnetization of Ferromagnetic Metals.*

The ovoids were first cooled in liquid air; the observations at ordinary and then at higher temperatures were taken; the results are given in Table X.

TABLE X.
Swedish Iron.

$t=714^{\circ}$ C.		$t=736^{\circ}$ C.		$t=757^{\circ}$ C.		$t=761^{\circ}$ C.	
H.	I.	H.	I.	H.	I.	H.	I.
0.13	49	0.13	40	0.05	50	8.1	11.8
0.26	115	0.79	220	3.29	121	75.5	39.0
0.50	338	18.42	373	29.39	166	188.9	64.5
1.39	533	115.6	493	125.8	211	333	85.3
2.48	597	258.0	542	305	241	437	97.6
22.46	744	398	561	425	250		
258.6	829						
377	834						
$t=772^{\circ}$ C.		$t=849^{\circ}$ C.		$t=1009^{\circ}$ C.		$t=1214^{\circ}$ C.	
H.	I.	H.	I.	H.	I.	H.	I.
52	6.3	128	2.0	99	1.7	92	1.4
140	15.4	300	3.4	280	3.4	286	2.8
281	23.1	443	3.9	439	3.9	442	3.1
444	29.0						

Nickel.

$t=205^{\circ}\text{ C.}$		$t=319^{\circ}\text{ C.}$		$t=362^{\circ}\text{ C.}$		$t=410^{\circ}\text{ C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
1.58	73	1.77	99	32.8	0.8	55	1.1
5.62	190	9.06	176	182.4	5.3	179	4.4
18.83	314	31.0	204	363	9.0	345	6.5
85.7	390	124.5	210	481	10.8	479	8.1
207.4	398	285	213				
334	401	464	215				
447	403						

$t=518^{\circ}\text{ C.}$		$t=678^{\circ}\text{ C.}$		$t=874^{\circ}\text{ C.}$		$t=1149^{\circ}\text{ C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
183	3.8	182	3.5	236	4.6	129	2.8
366	6.3	343	5.4	363	5.4	349	5.1
478	7.3	477	6.4	479	5.9	481	5.7

Annealed Cobalt.

$t=185^{\circ}\text{ C.}$		$t=307^{\circ}\text{ C.}$		$t=428^{\circ}\text{ C.}$		$t=546^{\circ}\text{ C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
13.45	49	3.87	45	3.22	22	4.27	57
19.52	96	7.43	112	8.23	83	7.13	141
32.4	198	14.69	319	13.41	185	13.62	406
63.5	325	37.6	570	21.46	307	22.43	547
94.3	411	92.8	770	57.7	514	59.9	710
184.1	568	195.7	913	119.1	662	126.7	832
279.2	677	319	998	240.1	812	253.2	928
376	758	370	1018	379	906	374	978

$t=619^{\circ}\text{ C.}$		$t=770^{\circ}\text{ C.}$		$t=919^{\circ}\text{ C.}$		$t=1060^{\circ}\text{ C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
2.48	32	2.40	50	1.26	46	4.14	56
4.79	108	4.45	151	2.55	141	22.03	84
7.88	250	13.21	411	13.83	399	51.1	98
31.31	573	28.15	531	42.20	518	90.0	106
72.1	716	76.0	674	94.4	591	212.5	126
128.5	806	134.0	749	184.9	639	333	136
271.4	909	262.9	823	291.6	665	439	140
378	951	386	856	400	674		

Annealed Cobalt (cont.).

$t=1066^{\circ}$ C.		$t=1074^{\circ}$ C.		$t=1109^{\circ}$ C.		$t=1219^{\circ}$ C.	
H.	I.	H.	I.	H.	I.	H.	I.
2.25	25	7.5	10.6	142	3.0	130	2.2
7.54	44	41.7	19.4	324	6.1	234	3.6
46.8	62	172.1	32.8	446	7.7	443	4.3
149.6	87	343	41.9				
244.1	100	446	45.0				
436	107						

The magnetization of iron and nickel at high temperatures is so well known that it is superfluous to give all the numerical data obtained by our experiments. Hence in the above table, the numbers for iron and nickel are limited to those at very high temperatures, in which they become of interest.

Swedish Iron.—The magnetization at constant temperature was measured at 20 different temperatures in ascending as well as descending stages; the curves of magnetization were then plotted against the internal field. These curves were cut by an ordinate of constant field. The curves of magnetization in a constant field plotted against the temperature were thus obtained, and are given in fig. 6 *a* (Pl. XI.).

The change of magnetization of Swedish iron with rise of temperature was found to agree well with the results obtained by previous investigators*. The weak magnetization beyond the critical point, as first observed by Curie, was also noticed. Here the magnetization at different temperatures ranging from 800° C. to 1200° C. diminishes very slightly as the temperature rises. Thus the meaning of the critical point becomes vague; H. Du Bois defines this temperature to be a point of inflexion in the temperature-curve of magnetization; but it is more convenient to define the temperature

* J. Hopkinson, Phil. Trans. clxxx. p. 443 (1889); Proc. Roy. Soc. xlv. p. 317 (1888). Lydall and Pocklington, Proc. Roy. Soc. lii. p. 228 (1893). D. K. Morris, Phil. Mag. xlv. p. 213 (1897). Ledeboer, C. R. cvi. p. 129 (1888). Tomlinson, Proc. Phys. Soc. ix. p. 181 (1888). Curie, C. R. cxv. p. 805 (1892); cxviii. pp. 796, 859. Wilde, Proc. Roy. Soc. l. p. 109 (1891). Kunz, *Elekt. Zeits.* xv. p. 194 (1894). Wills, Phil. Mag. l. p. 1 (1900). Nagaoka and Kusakabe, Jour. Coll. Sci. xix. art. 9 (1904).

as the point of the maximum curvature. The critical temperature so defined is, in the case of Swedish iron, 780°C . for $H=400$ c.g.s. It is also to be observed that the critical temperatures for ferromagnetic metals and alloys depend more or less upon the strength of the field.

The magnetization in a stage of descending temperatures falls a little short of the magnetization in ascending temperatures for the same field and temperature. But at ordinary temperatures they coincide with each other. Combining the above results with those of the magnetizations at the liquid air temperature, we obtain a hysteresis curve with regard to temperature, whose lower range is considerably extended by the present experiments.

Annealed Cobalt.—As in the case of Swedish iron, the temperature-curves of magnetization were obtained, and are given in fig. 6 *b*.

The magnetization of annealed cobalt at high temperatures was first observed by Professor Nagaoka and Mr. Kusakabe*. The present results generally agree with those obtained by them, but in our case the cooling in liquid air slightly altered the magnetic property. In fig. 6 *b* the point corresponding to the magnetization in liquid air is also included.

As the temperature rises from -186°C . the magnetization in a constant field increases, at first slowly and then rapidly, till it reaches a maximum at about 300°C ., after which it decreases. The magnetization reaches a small minimum and then begins to increase, and after passing through another maximum, rapidly decreases, reaching its critical point at 1090°C . for $H=400$. The descending branch of the curves cuts the ascending branch at about 850°C . from downward to upward; but its general course is similar to that of the ascending curve. The minimum point in the ascending branch is about 450°C ., and nearly coincides with the singular temperature observed by us in the change of length by magnetization†: at this temperature the sign of the length change is reversed for all fields.

It is also to be noticed that the course of the curve beyond the critical point is nearly parallel to the axis of temperature.

* Nagaoka and Kusakabe, *loc. cit.*

† Honda and Shimizu, Jour. Coll. Sci. xix. art. 10 (1903); Phil. Mag. vi. p. 392 (1903).

Nickel.—The specimen, which was first cooled in liquid air, was heated, and the magnetizations at ten different ascending temperatures were observed; since the dynamo stopped when the temperature attained 1150°C. , the magnetizations at decreasing temperatures were not taken as in the other cases, except for the maximum field only.

The temperature-curves of magnetization are drawn in fig. 6c, in which the results of the first experiment are also included. The character of the change of magnetization by heating coincides with the results obtained by the former investigators*. Here the range of the temperature is considerably extended on the negative side of zero temperature. It is remarkable that though the magnetization falls very rapidly near the critical temperature 360°C. , its further decrease is very small, and even at 1200°C. a magnetization of about 6 c.g.s. for $H=400$ is still observed. This important phenomenon was first observed by Curie.

(b) *Magnetization of Nickel-Steels.*

In nickel-steels the magnetic state after cooling in liquid air slightly changes as the time proceeds. In some alloys it does not return to its initial state when they undergo a cyclic change of temperatures between -186°C. and 1100°C. This change of character is greater in the irreversible alloys than in the reversible.

The magnetization of the alloys at different temperatures presents a striking contrast between the reversible and the irreversible alloys. Some of the interesting results had already been obtained by previous investigators†.

The manner in which the magnetization of the reversible nickel-steels changes with the temperature is similar to that of nickel, as given in Table XI.

* J. Hopkinson, *loc. cit.* Curie, *loc. cit.* Nagaoka and Kusakabe, *loc. cit.*

† H. Becquerel, *C. R.* xciii. p. 794 (1881). J. Hopkinson, *Proc. Roy. Soc.* xlvii. p. 23 (1890); xlviii. p. 1 (1890). H. Le Chatelier, *C. R.* cx. p. 283 (1890); cxi. p. 454 (1890). H. Tomlinson, *Proc. Roy. Soc.* lvi. p. 103 (1894). F. Osmond, *C. R.* cxviii. p. 532 (1894); cxxviii. pp. 304, 1396 (1899). Ch. Ed. Guillaume, *C. R.* cxxiv. pp. 176, 1515 (1897); cxxv. p. 235 (1897); cxxvi. p. 738 (1898); "Les Aciers au Nickel," Paris (1898). E. Dument, *C. R.* cxxvi. p. 741 (1898). L. Dumas, *C. R.* cxxx. p. 357 (1900).

TABLE XI.

Nickel-Steel 70.32 per cent.

$t=11^{\circ}7$ C.		$t=104^{\circ}$ C.		$t=410^{\circ}$ C.		$t=658^{\circ}$ C.	
H.	I.	H.	I.	H.	I.	H.	I.
0.34	9	0.40	16	0.41	256	35	2.8
1.07	42	1.08	69	1.89	368	132	9.1
1.73	232	1.25	164	8.92	485	276	18.6
4.28	570	1.60	374	31.4	536	476	29.2
12.29	716	2.86	516	64.1	545		
25.09	864	29.5	867	153.5	552		
41.1	947	68.3	944	294.6	565		
76.4	1000	166.1	970	434	573		
193.7	1029	264.6	980				
393	1043	405	989				

$t=733^{\circ}$ C.		$t=900^{\circ}$ C.		$t=753^{\circ}$ C.		$t=11^{\circ}5$ C.	
H.	I.	H.	I.	H.	I.	H.	I.
48	3.9	125	3.8	40	1.7	0.29	28
100	5.6	305	7.9	137	5.0	1.08	171
248	14.3	491	10.3	304	8.5	4.22	491
469	26.7			488	11.0	21.64	908
						56.3	1008
						204.4	1034
						349	1041
						405	1045

Nickel-Steel 50.72 per cent.

$t=12^{\circ}0$ C.		$t=126^{\circ}$ C.		$t=296^{\circ}$ C.		$t=410^{\circ}$ C.	
H.	I.	H.	I.	H.	I.	H.	I.
0.54	21	0.95	31	0.76	42	0.40	34
1.91	79	2.90	336	1.27	133	0.70	154
2.70	189	4.30	536	4.44	548	1.67	314
3.29	334	9.54	697	21.39	791	8.81	477
22.68	971	27.40	972	41.9	877	24.43	536
47.5	1123	52.1	1077	91.5	898	57.6	558
108.0	1218	115.9	1146	213.7	913	159.4	565
186.6	1250	215.1	1166	417	921	305	567
309	1263	398	1176			443	579
386	1267						

Nickel-Steel 50.72 per cent. (cont.).

$t=639^{\circ}\text{C.}$		$t=807^{\circ}\text{C.}$		$t=634^{\circ}\text{C.}$		$t=495^{\circ}\text{C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
21	0.7	31	1.1	31	0.7	30	0.6
113	3.9	206	6.9	136	4.7	98	3.4
267	8.2	487	11.5	322	9.0	345	10.7
490	11.3			484	11.6	482	13.6

$t=431^{\circ}\text{C.}$		$t=263^{\circ}\text{C.}$		$t=192^{\circ}\text{C.}$		$t=12^{\circ}.2\text{C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
12.7	4.7	0.06	46	0.07	34	0.20	26
34.7	11.5	0.20	169	0.18	432	0.49	87
101.6	31.2	0.45	586	0.70	649	0.77	355
245.1	46.2	85.3	941	6.53	951	0.94	582
402	58.7	269.8	949	24.20	1048	45.7	1235
478	65.1	408	952	72.0	1069	143.0	1259
				253.5	1078	261.7	1264
				390	1082	378	1267

Nickel-Steel 46 per cent.

$t=12^{\circ}.3\text{C.}$		$t=131^{\circ}\text{C.}$		$t=267^{\circ}\text{C.}$		$t=345^{\circ}\text{C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
0.78	22	0.78	28	0.59	31	0.42	38
1.83	60	2.56	144	1.29	96	0.82	112
3.40	190	3.21	319	2.71	403	2.51	227
7.49	549	5.11	521	5.40	561	4.59	380
9.87	706	9.71	751	15.11	704	13.46	495
17.70	913	22.80	943	34.8	778	40.2	562
42.9	1113	46.6	1047	78.9	834	135.3	588
80.3	1201	106.6	1114	213.4	851	253.9	593
195.4	1260	264.4	1140	317	852	431	597
375	1276	381	1146	409	854		

$t=411^{\circ}\text{C.}$		$t=456^{\circ}\text{C.}$		$t=624^{\circ}\text{C.}$		$t=488^{\circ}\text{C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
17	2.8	47	0.6	56	2.0	87	4.5
50	6.2	105	2.8	240	7.3	234	8.7
137	14.6	305	10.0	387	11.6	385	12.5
477	40.8	483	14.2	479	13.4	475	14.2

Nickel-Steel 46 per cent. (cont.).

$t=371^{\circ}\text{C.}$		$t=318^{\circ}\text{C.}$		$t=210^{\circ}\text{C.}$		$t=33^{\circ}\cdot 2\text{C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
12.3	30	0.59 ₄	31	0.72	27	1.62	49
18.9	45	1.51	209	1.35	80	2.90	132
45.6	91	5.24	400	2.14	204	5.94	512
190.6	143	11.41	490	5.86	527	17.62	827
347	168	45.2	592	50.95	887	69.6	1120
459	183	198.0	626	169.4	964	161.8	1217
		329	631	284	974	280	1243
		425	638	399	979	369	1252

Nickel-Steel 36 per cent.

$t=10^{\circ}\cdot 0\text{C.}$		$t=161^{\circ}\text{C.}$		$t=225^{\circ}\text{C.}$		$t=259^{\circ}\text{C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
1.24	30	1.06	55	5.7	33	31.6	0.7
3.51	119	2.23	191	17.8	45	152.2	6.0
4.32	232	3.99	308	48.2	54	355	13.2
7.79	499	12.89	461	259.0	84	497	16.6
32.9	863	54.9	547	478	102		
64.2	954	135.1	563				
176.2	1021	276.5	572				
299	1035	437	578				
419	1039						

$t=447^{\circ}\text{C.}$		$t=828^{\circ}\text{C.}$		$t=423^{\circ}\text{C.}$		$t=303^{\circ}\text{C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
96	3.3	105	4.7	31	1.8	79	4.2
251	8.4	251	10.5	103	5.7	232	10.5
491	13.5	488	17.1*	283	12.9	363	14.7
				484	18.1	482	17.8

* In this case the compensation of the magnetizing coil was found to be disturbed, and therefore the disturbance was equally distributed over the whole set. This small increase of magnetization is probably due to the impaired compensation.

Nickel-Steel 36 per cent. (cont.).

$t=218^{\circ}\text{C.}$		$t=182^{\circ}\text{C.}$		$t=145^{\circ}\text{C.}$		$t=10^{\circ}\cdot 0\text{C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
30·5	14·7	0·26	54	0·21	42	0·31	32
77·4	23·9	1·54	198	0·46	127	0·71	118
160·2	35·5	8·08	335	1·51	242	1·66	314
320	55·3	20·58	364	6·46	459	4·88	556
481	70·6	71·4	379	49·6	582	16·15	801
		250·7	391	151·4	592	112·1	1013
		447	401	292·3	597	247·9	1027
				428	602	302	1028
						375	1029

Comparing the above values for ordinary temperature with the corresponding values in the first series, we notice that except with 36 per cent. nickel-steel, the magnetizability of these alloys had slightly changed by the repeated heating and cooling which the alloys had undergone since the end of the first series.

From these results, the temperature-curves of magnetization are obtained and given in figs. 7 *a, b, c, d* (Pl. XII.). In these figures we have also included the results obtained in our first and second series of experiments. As seen from the figures, the diminution of magnetization, after the critical point is reached, is very slight; and to judge from the course of the curve it seems probable that the magnetization does not altogether vanish till the melting-points are reached.

The curves of magnetization at a constant field in the ascending and descending stages of temperature do not exactly coincide with each other when the range of temperature is large, the two curves thus enclosing a small area between them.

As the critical points of these nickel-steels for $H=400$, we give the following values:—

Alloys	70·32 %.	50·72 %.	46 %.	36 %.
Ascending branch.....	660° C.	490° C.	412° C.	255° C.
Descending branch		460° C.	395° C.	240° C.

These values nearly coincide with those of M. Osmond and L. Dumas. Thus the critical point falls with the percentage content of nickel.

The manner in which the magnetization of irreversible nickel-steel changes with temperature is very striking. The observed values are given in Table XII.

TABLE XII.
Nickel-Steel 29.24 per cent.

$t=11^{\circ}6\text{ C.}$		$t=140^{\circ}\text{ C.}$		$t=210^{\circ}\text{ C.}$		$t=352^{\circ}\text{ C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
3.21	30	5.83	31	11.27	57	9.87	56
9.33	103	12.57	109	18.51	162	18.10	161
14.20	200	29.8	299	29.1	276	31.3	275
22.35	355	74.3	433	68.0	428	69.4	399
48.3	507	169.5	566	164.7	566	167.7	518
79.0	604	329	678	280.5	646	308	595
150.5	723	449	729	442	713	445	639
244.2	810						
345	870						
417	902						
$t=499^{\circ}\text{ C.}$		$t=547^{\circ}\text{ C.}$		$t=799^{\circ}\text{ C.}$		$t=466^{\circ}\text{ C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
20.3	6.2	51	1.8	51	2.0	52	2.2
56.0	18.9	168	6.1	140	4.8	152	5.8
183.2	57.3	357	10.9	308	9.3	311	12.8
304	80.4	495	13.9	494	12.3	494	13.7
410	94.6						
487	102.5						
$t=299^{\circ}\text{ C.}$		$t=192^{\circ}\text{ C.}$		$t=93^{\circ}\text{ C.}$		$t=12^{\circ}0\text{ C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
31	1.1	31	1.4	45	2.2	0.21	46
105	4.9	103	5.1	107	5.8	1.63	107
256	10.1	254	10.5	272	13.2	5.15	182
376	12.8	375	13.6	494	20.6	27.91	250
497	15.5	494	16.2			95.6	265
						188.7	275
						334	286
						477	295

Nickel-Steel 29 per cent.

$t=14^{\circ}3\text{ C.}$		$t=168^{\circ}\text{ C.}$		$t=268^{\circ}\text{ C.}$		$t=383^{\circ}\text{ C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
4.35	3	5.95	23	7.43	21	9.85	44
5.31	71	12.60	86	14.55	71	26.24	153
12.31	214	19.61	176	23.40	173	49.3	230
20.76	367	30.5	254	36.1	257	109.7	314
29.1	432	64.5	353	115.4	407	231.1	387
57.5	585	208.7	503	281.5	512	441	446
125.2	717	323	561	434	561		
238.0	815	436	600				
416	889						
$t=465^{\circ}\text{ C.}$		$t=515^{\circ}\text{ C.}$		$t=644^{\circ}\text{ C.}$		$t=799^{\circ}\text{ C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
16.9	16	45.7	2.3	54	2.0	94	2.5
45.4	50	161.3	10.8	280	7.7	282	3.6
93.3	96	340	22.9	475	10.3	477	8.9
212.4	161	473	29.9				
321	192						
448	217						
$t=601^{\circ}\text{ C.}$		$t=405^{\circ}\text{ C.}$		$t=206^{\circ}\text{ C.}$		$t=12^{\circ}3\text{ C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
65	2.3	94	3.4	86	3.4	0.41	78
247	7.3	232	7.4	229	8.0	1.36	182
479	10.4	344	9.0	358	10.7	4.54	302
		476	10.8	473	12.2	17.32	381
						138.4	412
						369.9	426
						466	428

Nickel-Steel 28.74 per cent.

$t=12^{\circ}3\text{ C.}$		$t=87^{\circ}\text{ C.}$		$t=143^{\circ}\text{ C.}$		$t=228^{\circ}\text{ C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
3.09	22	3.99	21	5.66	32	3.63	14
7.20	67	8.26	52	10.47	92	10.53	61
14.04	192	12.79	114	15.30	177	16.50	163
21.16	340	20.17	244	23.95	310	25.82	316
51.9	569	27.27	331	47.5	426	54.9	468
127.0	777	52.0	460	75.9	518	126.8	611
225.7	896	82.6	564	203.1	701	249.0	722
307.2	955	185.1	716	318	784	333	766
422	1015	313	820	436	841	450	813
		429	882				
$t=327^{\circ}\text{ C.}$		$t=418^{\circ}\text{ C.}$		$t=497^{\circ}\text{ C.}$		$t=634^{\circ}\text{ C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
8.01	38	6.45	41	23.6	5.9	83	2.5
16.46	167	13.19	111	52.4	14.6	214	6.9
25.33	298	22.50	193	97.1	28.4	239	10.2
59.9	461	33.6	256	158.9	42.6	507	12.6
109.9	556	68.5	352	364	79.4		
224.1	659	117.3	428	506	93.9		
365	726	266.7	527				
443	752	446	584				
$t=795^{\circ}\text{ C.}$		$t=399^{\circ}\text{ C.}$		$t=101^{\circ}\text{ C.}$		$t=24^{\circ}2\text{ C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
60	2.1	98	4.2	24	1.1	0.10	41
157	5.2	262	9.7	107	6.6	3.85	207
342	9.6	511	14.3	258	14.6	23.07	285
511	12.0			507	24.2	96.1	303
						257.5	319
						490	334

Nickel-Steel 28.32 per cent.

$t=12^{\circ}5\text{ C.}$		$t=130^{\circ}\text{ C.}$		$t=300^{\circ}\text{ C.}$		$t=453^{\circ}\text{ C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
3.28	21	5.16	26	7.43	33	12.45	28
6.67	75	9.36	77	17.01	156	28.82	72
14.58	183	17.05	233	25.74	293	45.6	114
24.56	328	25.68	348	47.2	434	89.1	185
41.4	438	40.8	432	95.6	582	189.2	287
65.6	555	81.7	579	194.3	697	314	339
132.1	713	183.5	734	318	778	469	401
266.6	861	315	833	439	824		
420	962	423	872				

$t=504^{\circ}\text{ C.}$		$t=649^{\circ}\text{ C.}$		$t=818^{\circ}\text{ C.}$		$t=581^{\circ}\text{ C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
22	1.4	114	4.5	120	3.9	109	3.9
52	3.9	217	7.2	263	7.9	246	7.9
276	24.3	516	12.5	513	11.4	385	10.8
506	39.5					510	13.7

$t=387^{\circ}\text{ C.}$		$t=200^{\circ}\text{ C.}$		$t=52^{\circ}\text{ C.}$		$t=12^{\circ}8\text{ C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
105	4.8	105	5.1	46	3.7	3.19	20
216	8.2	215	9.1	111	8.4	18.38	36
367	11.7	341	12.5	388	23.1	37.8	46
512	14.2	511	15.9	513	28.8	125.2	72
						289	102
						492	126

Nickel-Steel 26.64 per cent.

$t=15^{\circ}3\text{ C.}$		$t=121^{\circ}\text{ C.}$		$t=239^{\circ}\text{ C.}$		$t=392^{\circ}\text{ C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
2.55	10	5.20	25	6.38	27	5.03	55
10.22	65	9.02	59	13.88	93	9.42	138
16.87	154	16.46	163	19.21	209	13.93	226
25.32	292	24.45	297	26.04	341	24.14	364
37.4	402	33.2	384	39.1	482	60.6	553
57.4	581	50.2	533	54.1	613	111.9	665
105.3	737	104.9	738	100.9	755	226.4	774
180.1	876	179.4	867	192.3	882	420	849
271.1	976	290	969	330	982		
406	1064	401	1035	395	1001		

Nickel-Steel 26.64 per cent. (cont.).

$t=463^{\circ}\text{C.}$		$t=517^{\circ}\text{C.}$		$t=665^{\circ}\text{C.}$		$t=828^{\circ}\text{C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
7.27	28	50	2.4	104	3.9	79	3.5
15.03	63	204	10.3	248	8.8	301	9.8
28.18	121	484	20.2	484	13.9	481	13.6
40.5	167						
101.1	297						
216.1	417						
352	480						
439	538						

$t=514^{\circ}\text{C.}$		$t=401^{\circ}\text{C.}$		$t=227^{\circ}\text{C.}$		$t=80^{\circ}\text{C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
103	4.5	123	5.4	124	5.2	113	5.6
301	10.3	303	10.8	301	10.5	301	12.4
480	14.4	479	14.8	480	15.0	479	16.6

Nickel-Steel 24.40 per cent.

$t=13^{\circ}\cdot 0\text{C.}$		$t=101^{\circ}\text{C.}$		$t=190^{\circ}\text{C.}$		$t=366^{\circ}\text{C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
3.89	14	8.42	37	12.11	71	9.00	58
12.35	66	19.06	155	19.50	184	16.20	187
20.60	171	27.62	282	27.16	304	23.75	331
27.75	297	48.4	450	49.5	508	40.5	475
56.09	497	81.7	618	61.3	579	62.9	589
126.5	744	152.4	775	148.3	791	132.0	733
184.9	837	229.5	864	261.2	903	213.3	805
296	941	308	926	420	988	312.0	856
400	1009	419	991			427	895

$t=466^{\circ}\text{C.}$		$t=490^{\circ}\text{C.}$		$t=392^{\circ}\text{C.}$		$t=207^{\circ}\text{C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
5.25	35	8.48	25	12.42	30	10.39	37
10.72	89	18.47	60	15.66	73	23.25	96
25.72	234	39.2	138	40.5	125	37.78	167
44.7	338	73.2	226	80.3	230	76.7	302
113.7	521	145.4	343	165.8	362	138.5	438
244.9	645	296	456	279.4	460	272.2	575
442	724	458	516	455	542	447	661

Nickel-Steel 24.40 per cent. (cont.).

$t=14^{\circ}2\text{ C.}$		$t=237^{\circ}\text{ C.}$		$t=410^{\circ}\text{ C.}$		$t=490^{\circ}\text{ C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
5.78	32	8.17	28	12.55	33	14.36	29
13.39	86	16.48	61	23.55	68	37.3	90
20.82	155	31.36	126	38.2	126	90.4	209
47.2	375	74.9	267	73.2	201	184.1	327
77.0	543	155.6	425	105.2	270	342	423
164.8	722	309	569	230.6	413	434	465
260	797	449	631	334	489		
412	880			447	538		

$t=542^{\circ}\text{ C.}$		$t=591^{\circ}\text{ C.}$		$t=851^{\circ}\text{ C.}$		$t=1000^{\circ}\text{ C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
29.2	13	56.7	2.0	57.1	1.7	105	2.5
98.8	50	166.5	5.2	152.0	4.7	305	6.0
338	128	308.2	8.2	308.3	7.4	483	7.3
472	146	483.0	10.5	486.0	8.7		

$t=1200^{\circ}\text{ C.}$		$t=861^{\circ}\text{ C.}$		$t=212^{\circ}\text{ C.}$		$t=14^{\circ}6\text{ C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
290	3.4	105.3	2.1	64.4	5.1	19.6	25
448	3.6	304	4.7	165.4	8.9	46.2	83
480	3.7	481	5.4	323	11.9	92.8	124
				485	13.8	193.6	176
						351	214
						464	233

Nickel-Steel 24.04 per cent.

$t=10^{\circ}1\text{ C.}$		$t=182^{\circ}\text{ C.}$		$t=287^{\circ}\text{ C.}$		$t=452^{\circ}\text{ C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
2.44	6	3.68	13	3.48	14	3.81	27
10.74	42	11.43	54	12.36	77	12.27	123
27.60	230	20.48	138	18.63	193	24.91	254
35.6	306	26.97	270	23.06	313	47.7	372
58.2	520	39.1	386	33.2	427	71.9	452
80.6	641	58.9	552	51.8	592	164.3	602
115.0	756	90.7	703	95.7	760	271.2	673
185.6	888	174.7	875	183.4	886	447	745
276.2	985	284.5	982	275.8	954		
393	1060	395	1046	383	1002		

Nickel-Steel 24.04 per cent. (cont.).

$t=531^{\circ}\text{C.}$		$t=586^{\circ}\text{C.}$		$t=717^{\circ}\text{C.}$		$t=604^{\circ}\text{C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
41	1.9	101	3.4	124	4.6	64	3.4
184	7.9	268	7.7	348	10.0	236	7.7
370	13.7	487	11.5	487	12.1	411	11.3
490	16.2					489	12.9

$t=502^{\circ}\text{C.}$		$t=274^{\circ}\text{C.}$		$t=193^{\circ}\text{C.}$		$t=11^{\circ}.3\text{C.}$	
H.	I.	H.	I.	H.	I.	H.	I.
79	3.0	50	2.8	34	1.8	57	2.8
250	9.0	151	7.7	102	5.7	165	8.9
392	11.9	352	14.1	310	12.9	286	13.8
487	13.8	487	16.6	494	16.5	484	18.9

From these values, the temperature-curves of magnetization are obtained and drawn in figs. 7 *e, f, g, h, i, j, k.* In these figures, we have included the results obtained in the first and second series of experiments.

Here we also notice that, except with 28.74 per cent., the magnetizability of these alloys had considerably changed by the heating and cooling which the alloys had undergone since the first experiment. Hence in some of the figures, the portions corresponding to the first series of experiments were displaced parallel to themselves so as to form closed curves. Thus the displaced portions are given in dotted lines.

As the temperature gradually rises from -186°C. , the magnetization of 29.24 per cent. Ni diminishes at first slowly, then rapidly, and after passing through an inflexion point, the diminution becomes slow. The curve passing through a second inflexion point begins to descend very rapidly, as the critical temperature is approached. If this temperature be passed, the diminution of the magnetization by heating is very small, so that the curve is nearly parallel to the axis of temperature. From the course of the curve, it seems probable that the magnetization does not altogether vanish till the melting-point of the specimen is reached. As the temperature is next gradually reduced, the increase of magnetization is very small; this state continues till the temperature falls to about 100°C. ; then the increase becomes very rapid. For example, in $H=400$ c.g.s., the intensity of magnetization at

the descending temperature is only 20 c.g.s. for a temperature of 80°C ., but it amounts to 200 for 20°C ., and at -60°C . it increases to 790. Thus the magnetization of the specimen displays a remarkable difference between the ascending and descending branches of the curve.

The above manner, in which the magnetization is changed by temperature, is common to all other irreversible nickel-steels. As the percentage of nickel decreases, the concave portion of the ascending branch becomes fainter and fainter; and with 24.40 per cent. and 24.04 per cent. Ni, it almost vanishes for strong fields. Apparently, the forms of the two curves for nickel-steels of 29.24 per cent. and 24.04 per cent. Ni are widely different from each other; but if we compare the forms of the curves of two consecutive nickel-steels, we can trace transition stages from one form to the other.

The critical temperatures of the alloys for $H=400$ c.g.s. are given in the following table:—

Alloys	29.24 p. cent.	29 p. cent.	28.74 p. cent.	28.32 p. cent.	26.64 p. cent.	24.40 p. cent.	24.04 p. cent.
Ascending branch ...	530°C .	530°C .	530°C .	510°C .	510°C .	580°C .	520°C .
Descending branch ...	70	140	80	50	10	130	40

Thus, in the ascending branch, the critical temperatures of these irreversible nickel-steels are nearly equal, except with the last but one. The above numbers fairly coincide with those obtained by M. Osmond, except with 24.40 per cent. Ni. With this alloy the critical temperatures are greater, in our case, by about 50°C . for the ascending branch and 100°C . for the descending, than in the experiment by Osmond. The values given by L. Dumas for the first four of these alloys are considerably less than those obtained by us; but for the remaining alloys the contrary is the case. These discrepancies may probably be due to the previous history of the alloys.

It remains to mention a singular phenomenon. If at a point in an ascending branch of the temperature-cycle, the temperature be reduced to the ordinary, the path is utterly different from the ascending one. If, however, the temperature be again increased to its former value, the path nearly coincides with the former one; the further increase of temperature diminishes the magnetization in such a manner that the magnetization is not interrupted by the cooling process. An instance is seen in fig. 7*j*. Hence in

irreversible nickel-steels, the magnetization at ordinary temperature can have *any value whatever* within given limits, if the cooled specimens be heated to a suitable temperature. Becquerel, who first studied the magnetic properties of irreversible nickel-steels, found that in the alloy there were two states of stable equilibrium; but according to our results there are an infinite number of such states, a fact which may possibly prove to be important in the theory of molecular magnetism.

Comparing the magnetization at different temperatures in these nickel-steels, we notice that the critical temperature in the descending branch of the temperature-cycle generally becomes less as the percentage of nickel decreases. As the content of nickel diminishes from 70·32 per cent. to 26·64 per cent., the critical temperature falls from several hundred degrees to the ordinary temperature. It is then highly probable that 25 per cent. nickel-steel, which is feebly magnetic both at ordinary and liquid air-temperatures, would become strongly magnetic, if the cooling should be pushed still further. If it once become strongly magnetic by cooling, it may preserve this property, after the alloy is heated to the ordinary temperature. It will be interesting to investigate, whether other non-magnetic alloys, which consist of a magnetic and a non-magnetic metal, would display a similar phenomenon on being cooled to a sufficiently low temperature.

The fact that the two strongly magnetic metals form a non-magnetic metal is then nothing more than the lowering of the critical temperature of the alloy to the ordinary temperature. Owing to some changes occurring in the molecular configuration during the process of fusion of the constituent metals, the critical temperature of the alloy in the descending branch of the temperature-cycle falls to a low temperature, and therefore the alloy behaves as a weakly magnetic or non-magnetic alloy at ordinary temperature. The same remark will also apply to a non-magnetic alloy which consists of a magnetic metal and a non-magnetic one. The above view is also favoured by the fact that in irreversible alloys the hysteresis-loss at ordinary temperature is markedly small, which corresponds to the hysteresis of iron or nickel at high temperatures, but its value at a low temperature considerably increases, corresponding to the hysteresis of the same metal at ordinary temperature.

LXXIII. *The Theory of Electrolytic Dissociation.* (*A Rectification of the "Correction" by Professor Harry C. Jones.*)
 By LOUIS KAHLBERG, Ph.D., Professor of Physical Chemistry in the University of Wisconsin*.

THE "Correction" by H. C. Jones† which recently appeared in this Journal is founded upon an error on the part of its author to which I desire to call attention. The supposed "correction" relates to the following passage which Jones quotes from p. 215 of my paper‡:—"In 1901 I published a list of results of cryoscopic and ebullioscopic determinations made with typical aqueous solutions of electrolytes and non-electrolytes, and also a list of molecular conductivity determinations of the same electrolytes at 0° and at 95°. It is unnecessary to discuss again the details of these results which are rather voluminous. Suffice it to state that a comparison of the freezing-point values with the molecular conductivity at 0°, and also of the boiling-point values with the molecular conductivity at 95°, revealed the fact that there is no such connexion between freezing-points and boiling-points of solutions on the one hand, and their conductivity on the other, as is claimed by the theory of Arrhenius. In numerous cases not even a qualitative agreement exists. The facts presented in the paper cited have since been corroborated by Smits in his careful vapour-tension measurements, and by H. C. Jones and co-workers in their molecular weight determinations on solutions."

Now it is clear that this passage relates to *aqueous* solutions as the first sentence of it explicitly states, which fact has been entirely overlooked by Jones, for after quoting the above he continues §:—"Referring to the paper cited by L. Kahlenberg ||, and turning to page 342, we find the following:—Under the heading 'Behaviour of non-aqueous electrolytic solutions' the following supposed 'facts are presented.' 'Again, many solutions have been found in which the solute according to molecular weight determinations is undissociated, and which nevertheless possess excellent power of conducting electricity.'... 'According to Dutoit and Friderich, CdI₂, LiCl, NaI, HgCl₂, and NH₄CnS have normal molecular weights in acetone, and yet these solutions are conductors of electricity.'"

Jones then proceeds to give a general statement

* Communicated by the Author.

† Phil. Mag. [6] x. p. 157 (1905).

‡ Phil. Mag. [6] ix. p. 214 (1905).

§ Loc. cit. p. 158.

|| Journ. Phys. Chem. v. p. 339 (1901).

of the results of his molecular weight determinations of the salts just mentioned in *acetone* solutions. It is clear, therefore, that he has wrongly applied the passage which he quotes from p. 215 of my paper * to *non-aqueous* solutions, for it relates to *aqueous* solutions †, as has been pointed out above, and his "correction" is consequently not pertinent.

The passage quoted by Jones from p. 215 of my paper does refer to the molecular weight determinations on *aqueous* solutions made by him and his co-workers,—see the article by Jones and Getman, Amer. Chem. Journ. xxxi. p. 303 (1904) ‡. In this paper experimental data are presented showing the change of molecular lowering of the freezing-point and also the alteration of the molecular conductivity with change of the concentration of the solutions. A goodly number of the salts which were thus investigated are the same as those I had used, and an examination of the *experimental results* which Jones obtained with these salts shows that there is no such relation between molecular weight determinations on the one hand and conductivity measurements on the other as the theory of Arrhenius requires. Moreover, the behaviour of the salts which they studied in addition to those which I measured, also substantiates this contention. The determinations of Jones and his co-workers therefore constitute, indeed, a corroboration of my work as was stated in this Journal §.

I would like to emphasize here that in my article|| I have not referred to the *interpretations* which Jones himself puts upon his experimental data, or the mode of reasoning by which he arrives at his conclusions. A scrutiny of the method of "reasoning" adopted by Jones shows that when summed up it simply consists of *assuming* for any specific solution just so much polymerization and electrolytic dissociation of the dissolved molecules, together with combination of the latter with the solvent molecules, as may be necessary to make the results of the experimental measurements conform to the

* Phil. Mag. [6] ix. (1905).

† Indeed, I do not consider the relations between molecular weight and conductivity in the case of *non-aqueous* solutions till seven pages later in my paper. See Phil. Mag. [6] ix. p. 222 (1905).

‡ The earlier work by Jones and Chambers, and Chambers and Frazer, Amer. Chem. Journ. xxiii. p. 89 and p. 512 (1900), had previously been considered by me, Journ. Phys. Chem. v. p. 359 (1901).

§ The measurements of Smits, which were mentioned in the same connexion, also confirm my *experimental* work, which that author has frankly admitted, Zeit. phys. Chem. xxxix. p. 385 (1902). Smits mentions the work of Jones, Chambers, and Frazer in the same connexion, *l. c.* bottom of p. 429.

|| Phil. Mag. [6] ix. p. 214 (1905).

requirements of the simple gas laws and the theory of electrolytic dissociation. Such cases as the acetone solutions of CdI_2 , NH_4CnS , and NaI , for instance, which are good electrolytes though the ebullioscopic determinations yield molecular weights of 448.6 to 510.7, 88.1 to 101.6, and 133.2 to 143.0 for the respective salts, whereas the corresponding theoretical values are 336, 76.2, and 149.9 *, are consequently readily "harmonized" with the theory of electrolytic dissociation by Jones, by assuming the required amount of polymerization and dissociation necessary for his purpose. It is quite unnecessary to dwell upon this further, for in a recent article, which should prove interesting reading to all who belong to what he calls the "dilute school," J. J. Van Laar † has thoroughly exposed the fallacy of such a course of reasoning as that adopted by Jones and Getman.

Finally, as to whether the facts upon which I have based my arguments against the theory of electrolytic dissociation are real or merely "supposed," as Jones would have it, I shall gladly leave to the judgment of the reader.

Laboratory of Physical Chemistry,
University of Wisconsin, Madison.
July 1905.

LXXIV. *On the Conduction of Electricity through Gases between Parallel Plates.*—Part II. By ALFRED A. ROBB ‡.

THE writer has already shown § that the differential equation

$$\frac{d^2 X^2}{dx^2} = 8\pi e \left(\frac{1}{R_1} + \frac{1}{R_2} \right) \left\{ q - \frac{\alpha}{e^2 X^2 (R_1 + R_2)^2} \right. \\ \left. \times \left(i + \frac{R_2}{8\pi} \frac{dX^2}{dx} \right) \left(i - \frac{R_1}{8\pi} \frac{dX^2}{dx} \right) \right\} \quad (1)$$

* The data are those of Jones, Amer. Chem. Journ. xxvii. p. 16 (1902). They are somewhat different from those of Dutoit and Friderich who simply state that the molecular weights are normal, without giving specific figures, *Bull. Soc. Chim. Paris*, [3] xix. p. 334 (1898). Jones did not investigate LiCl in acetone on account of its slight solubility. The HgCl_2 solution had but slight conductivity, and its molecular weight determination yielded the values 267.9 and 271.2 (but two determinations were made) as compared with 271.2, the theoretical. This latter case then presents no difficulties for the dissociation theory, as Jones well states.

† *Chemisch Weekblad*, ii. pp. 1-16 See also the abstract in *Chem. Centralblatt*, lxxvi. p. 491 (1905).

‡ Communicated by the Author.

§ *Phil. Mag.* August 1905.

may be reduced to a form which is independent of both i and q by putting

$$\text{and} \quad \left. \begin{aligned} X^2 &= \frac{i^2}{q} y^2 \\ x &= \frac{i}{q} v \end{aligned} \right\} \dots \dots \dots (2)$$

The equation then becomes

$$\frac{d^2 y^2}{dv^2} = 8\pi e \left(\frac{1}{R_1} + \frac{1}{R_2} \right) \left\{ 1 - \frac{\alpha}{e^2 y^2 (R_1 + R_2)^2} \right. \\ \left. \times \left(1 + \frac{R_2}{8\pi} \frac{dy^2}{dv} \right) \left(1 - \frac{R_1}{8\pi} \frac{dy^2}{dv} \right) \right\}. \quad (3)$$

The writer also showed that for any gas for which R_1 and R_2 are unequal there exist two pressures at which this equation takes a soluble form. The solution is

$$\left. \begin{aligned} y &= \frac{4\pi}{\sqrt{\alpha}} \left\{ \frac{-\epsilon}{1-\epsilon} \int \frac{d\omega}{\cosh \frac{1}{1-\epsilon} \omega} + k_1 \right\} \cosh^{\frac{1}{1-\epsilon}} \omega, \\ v &= \frac{R_2 - R_1}{\sqrt{\alpha}} \frac{\epsilon}{1-2\epsilon} \left\{ \int y d\omega + k_2 \right\}, \end{aligned} \right\} \quad (4)$$

in which

$$\epsilon = \frac{R_1}{R_1 + R_2} \quad \text{or} \quad = \frac{R_2}{R_1 + R_2}$$

according as the pressure is such as to make

$$\alpha = 4\pi e R_1 \quad \text{or} \quad = 4\pi e R_2.$$

The integrals which here occur can only be evaluated in finite form when $\frac{1}{1-\epsilon}$ is an integer, and when accordingly the velocity of one ion is an exact multiple of that of the other. This is not, however, usually the case, and for air the ratio $R_2 : R_1$ is very nearly 5 : 4. It seemed desirable to render my former paper more useful by calculating the values of the integrals in this case.

Different physicists have obtained slightly different values for the ratio, and we can hardly as yet say whether $R_2 : R_1$ in the case of air is greater or less than 5 : 4.

The tables which follow are calculated on the supposition that

$$R_2 : R_1 = 5 : 4,$$

and that the pressure is such that

$$\alpha = 4\pi e R_1.$$

This pressure is roughly $1\frac{1}{2}$ atmospheres. It will be well to give a short account of the method of calculating these tables.

The first function to be calculated is

$$\frac{-\epsilon}{1-\epsilon} \cosh^{\frac{1}{1-\epsilon}} \omega \int \frac{d\omega}{\cosh^{\frac{1}{1-\epsilon}} \omega}.$$

This may be expressed as a series in two forms, of which the one possesses the advantage that it converges more rapidly, while the other is better adapted for the second integration. We accordingly give both series.

Writing $\cosh \omega = \theta$ we have $d\omega = \frac{d\theta}{\sqrt{\theta^2 - 1}}$.

Thus

$$\frac{-\epsilon}{1-\epsilon} \cosh^{\frac{1}{1-\epsilon}} \omega \int \frac{d\omega}{\cosh^{\frac{1}{1-\epsilon}} \omega} = \frac{-\epsilon}{1-\epsilon} \theta^{\frac{1}{1-\epsilon}} \int \frac{d\theta}{\theta^{\frac{1}{1-\epsilon}} \sqrt{\theta^2 - 1}}.$$

Expanding the expression under the integral sign in descending powers of θ and integrating term by term, we get

$$\frac{-\epsilon}{1-\epsilon} \theta^{\frac{1}{1-\epsilon}} \left\{ -(1-\epsilon) \theta^{-\frac{1}{1-\epsilon}} - \frac{1}{2} \frac{(1-\epsilon)}{(3-2\epsilon)} \theta^{-\frac{1}{1-\epsilon}-2} - \frac{1 \cdot 3}{2^2 \cdot 2} \frac{(1-\epsilon)}{(5-4\epsilon)} \theta^{-\frac{1}{1-\epsilon}-4} - \dots \right\}.$$

The expression inside the brackets $\{ \}$ represents the integral and clearly vanishes when we put $\theta = \infty$. We may thus write

$$\begin{aligned} \frac{-\epsilon}{1-\epsilon} \cosh^{\frac{1}{1-\epsilon}} \omega \int_{\infty}^{\omega} \frac{d\omega}{\cosh^{\frac{1}{1-\epsilon}} \omega} &= \frac{\epsilon}{1-\epsilon} \cosh^{\frac{1}{1-\epsilon}} \omega \int_{\omega}^{\infty} \frac{d\omega}{\cosh^{\frac{1}{1-\epsilon}} \omega} \\ &= \epsilon \left\{ 1 + \frac{1}{2} \frac{1}{(3-2\epsilon) \cosh^2 \omega} + \frac{1 \cdot 3}{2^2 \cdot 2} \frac{1}{(5-4\epsilon) \cosh^4 \omega} + \dots \right\}. \quad (5) \end{aligned}$$

This series converges for all real values of ω including $\omega=0$, and is the one most suitable for calculating the function. We shall next obtain the other series which is adapted for the second integration. For this purpose we make use of the formula

$$\int \frac{d\omega}{\cosh^m \omega} = -\frac{1}{m} \frac{\sinh \omega}{\cosh^{m+1} \omega} + \frac{m+1}{m} \int \frac{d\omega}{\cosh^{m+2} \omega}.$$

By successive applications of this formula we get

$$\begin{aligned} \int_{\infty}^{\omega} \frac{d\omega}{\cosh^m \omega} &= -\frac{1}{m} \frac{\sinh \omega}{\cosh^{m+1} \omega} \\ &\quad - \frac{(m+1)}{m(m+2)} \frac{\sinh \omega}{\cosh^{m+3} \omega} - \frac{(m+1)(m+3)}{m(m+2)(m+4)} \frac{\sinh \omega}{\cosh^{m+5} \omega} \\ &\quad - \dots + \frac{(m+1)(m+3) \dots (m+2r-1)}{m(m+2)(m+4) \dots (m+2r-2)} \int_{\infty}^{\omega} \frac{d\omega}{\cosh^{m+2r} \omega}. \end{aligned}$$

The former method of treatment shows, however, that

$$\begin{aligned} \int_{\infty}^{\omega} \frac{d\omega}{\cosh^{m+2r} \omega} \\ = -\frac{1}{(m+2r) \cosh^{m+2r} \omega} \left\{ 1 + \frac{1}{2} \frac{(m+2r)}{(m+2r+2) \cosh^2 \omega} + \dots \right\}. \end{aligned}$$

Thus the remainder is

$$\begin{aligned} -\frac{(m+1)(m+3) \dots (m+2r-1)}{m(m+2)(m+4) \dots (m+2r)} \frac{1}{\cosh^{m+2r} \omega} \\ \times \left\{ 1 + \frac{1}{2} \frac{(m+2r)}{(m+2r+2) \cosh^2 \omega} + \dots \right\}. \end{aligned}$$

The coefficient

$$-\frac{(m+1)(m+3) \dots (m+2r-1)}{m(m+2)(m+4) \dots (m+2r)}$$

is clearly a proper fraction which decreases in absolute value as r increases.

The expression

$$\frac{1}{\cosh^{m+2r} \omega} \left\{ 1 + \frac{1}{2} \frac{(m+2r)}{(m+2r+2) \cosh^2 \omega} + \dots \right\}$$

clearly has the limit zero so long as $\cosh \omega > 1$.

Thus the remainder vanishes for $r = \infty$.

We can therefore write

$$\int_{\infty}^{\omega} \frac{d\omega}{\cosh^m \omega} = -\frac{1}{m} \frac{\sinh \omega}{\cosh^{m+1} \omega} - \frac{(m+1)}{m(m+2)} \frac{\sinh \omega}{\cosh^{m+3} \omega} - \dots$$

and this is valid so long as $\cosh \omega > 1$.

If, however, $\cosh \omega = 1$ then $\sinh \omega = 0$ and each term of the series vanishes. Since we know that the integral $\int_{\infty}^0 \frac{d\omega}{\cosh^m \omega}$ is not zero, it follows that the function represented by the series has a discontinuity for $\omega = 0$. It will be found,

however, that this does not interfere with the attainment of our object.

Putting for m the value $\frac{1}{1-\epsilon}$ we get

$$\frac{\epsilon}{1-\epsilon} \cosh^{\frac{1}{1-\epsilon}} \omega \int_{\omega}^{\infty} \frac{d\omega}{\cosh^{\frac{1}{1-\epsilon}} \omega} = \epsilon \left\{ \frac{\sinh \omega}{\cosh \omega} + \frac{(2-\epsilon)}{(3-2\epsilon)} \frac{\sinh \omega}{\cosh^3 \omega} \right. \\ \left. + \frac{(2-\epsilon)(4-3\epsilon)}{(3-2\epsilon)(5-4\epsilon)} \frac{\sinh \omega}{\cosh^5 \omega} + + \right\}, \quad (6)$$

which is valid except for $\omega=0$.

That this series is uniformly convergent for any range of values of ω greater than zero is shown if we write it in the form

$$\epsilon \left\{ \sqrt{1 - \frac{1}{\cosh^2 \omega}} + \frac{(2-\epsilon)}{(3-2\epsilon)} \sqrt{1 - \frac{1}{\cosh^2 \omega}} \frac{1}{\cosh^2 \omega} \right. \\ \left. + \frac{(2-\epsilon)(4-3\epsilon)}{(3-2\epsilon)(5-4\epsilon)} \sqrt{1 - \frac{1}{\cosh^2 \omega}} \frac{1}{\cosh^4 \omega} + + \right\}.$$

If now we select any real quantity θ_1 greater than but as nearly as we please equal to unity, we observe that the successive terms of the above series are less than the corresponding terms of the series

$$\epsilon \left\{ 1 + \frac{1}{\theta_1^2} + \frac{1}{\theta_1^4} + + \right\}$$

for all values of $\cosh \omega \geq \theta_1$.

Since the last is a convergent series of real positive terms, it follows that our series is uniformly convergent through any interval not including $\omega=0$.

We are therefore at liberty to integrate it term by term.

Now we saw that

$$v = \frac{R_2 - R_1}{\sqrt{\alpha}} \frac{\epsilon}{1-2\epsilon} \left\{ \int y d\omega + k_2 \right\}.$$

Thus putting for y its value we have

$$v = \frac{4\pi(R_2 - R_1)\epsilon^2}{\alpha(1-2\epsilon)} \left\{ \frac{1}{\epsilon} \int \left(\frac{\epsilon}{1-\epsilon} \cosh^{\frac{1}{1-\epsilon}} \omega \int_{\omega}^{\infty} \frac{d\omega}{\cosh^{\frac{1}{1-\epsilon}} \omega} \right) d\omega \right. \\ \left. + \frac{k_1}{\epsilon} \int \cosh^{\frac{1}{1-\epsilon}} \omega d\omega + k_2 \right\}, \quad \dots \quad (7)$$

in which the meaning of the arbitrary constant k_2 has been slightly altered for simplicity.

Thus integrating term by term we get

$$\frac{1}{\epsilon} \int \left(\frac{\epsilon}{1-\epsilon} \cosh^{\frac{1}{1-\epsilon}} \omega \int_{\omega}^{\infty} \frac{d\omega}{\cosh^{\frac{1}{1-\epsilon}} \omega} \right) d\omega$$

$$= \log \cosh \omega - \frac{1}{2} \left\{ \frac{(2-\epsilon)}{(3-2\epsilon)} \frac{1}{\cosh^2 \omega} + \frac{1}{2} \frac{(2-\epsilon)(4-3\epsilon)}{(3-2\epsilon)(5-4\epsilon)} \frac{1}{\cosh^4 \omega} + + \right\}. (8)$$

Now we know that this integration is valid for all values of $\cosh \omega > 1$.

If, however, we apply the usual test we find that the series converges even when $\cosh \omega = 1$.

Since the part of the series in brackets is a power series in $\frac{1}{\cosh^2 \omega}$, it follows by Abel's continuity theorem (see Chrystal's Algebra, vol. ii. p. 133) that the value of the series obtained by putting $\cosh \omega = 1$ is continuous with those for which $\cosh \omega > 1$. Since these latter represent the integral required and the integral itself is continuous up to $\cosh \omega = 1$, it follows that the above series (8) is valid for all real values of ω .

We have yet a third integral to evaluate, namely

$$\frac{1}{\epsilon} \int \cosh^{\frac{1}{1-\epsilon}} \omega d\omega.$$

Putting as before $\cosh \omega = \theta$, this becomes

$$\frac{1}{\epsilon} \int \frac{\theta^{\frac{1}{1-\epsilon}} d\theta}{\sqrt{\theta^2 - 1}}.$$

Expanding this in descending powers of θ and integrating term by term, we get:

$$\frac{1}{\epsilon} \int \frac{\theta^{\frac{1}{1-\epsilon}} d\theta}{\sqrt{\theta^2 - 1}} = \frac{1-\epsilon}{\epsilon} \theta^{\frac{1}{1-\epsilon}} \left\{ 1 + \frac{1}{2} \frac{1}{(2\epsilon-1)\theta^2} + \frac{1.3}{2^2 \cdot 2} \frac{1}{(4\epsilon-3)\theta^4} + + \right\}$$

or

$$\frac{1}{\epsilon} \int \cosh^{\frac{1}{1-\epsilon}} \omega d\omega = \frac{1-\epsilon}{\epsilon} \cosh^{\frac{1}{1-\epsilon}} \omega \left\{ 1 + \frac{1}{2} \frac{1}{(2\epsilon-1) \cosh^2 \omega} \right.$$

$$\left. + \frac{1.3}{2^2 \cdot 2} \frac{1}{(4\epsilon-3) \cosh^4 \omega} + + \right\} (9)$$

The usual tests show that this series is convergent even when $\cosh \omega = 1$.

It thus appears that the three series given by equations (5), (8), and (9) are valid expansions for all real values of ω , and therefore may be used to calculate tables of the required functions.

It will be shown later that $\omega=0$ corresponds to the position of one of the plates. Thus the values of the functions for $\omega=0$ are of special importance. Although the series which we give are convergent for this value of ω , yet the convergence is very slow, and it appears desirable to obtain other expressions for them in this case.

Let us first take

$$\frac{\epsilon}{1-\epsilon} \cosh^{\frac{1}{1-\epsilon}} \omega \int_{\omega}^{\infty} \frac{d\omega}{\cosh^{\frac{1}{1-\epsilon}} \omega}.$$

For $\omega=0$ this becomes

$$\frac{\epsilon}{1-\epsilon} \int_0^{\infty} \frac{d\omega}{\cosh^{\frac{1}{1-\epsilon}} \omega}.$$

Putting $\cosh \omega = p^{-\frac{1}{2}}$ we have

$$\frac{\epsilon}{1-\epsilon} \frac{1}{2} \int_0^1 p^{\frac{1}{2(1-\epsilon)}-1} (1-p)^{\frac{1}{2}-1} dp,$$

and therefore

$$\frac{\epsilon}{1-\epsilon} \int_0^{\infty} \frac{d\omega}{\cosh^{\frac{1}{1-\epsilon}} \omega} = \frac{\epsilon}{1-\epsilon} \frac{1}{2} \frac{\Gamma\left(\frac{1}{2(1-\epsilon)}\right) \Gamma\left(\frac{1}{2}\right)}{\Gamma\left(\frac{1}{2(1-\epsilon)} + \frac{1}{2}\right)}. \quad (10)$$

This may be at once evaluated by means of the table of Γ functions.

We shall next find the value of the series given by equation (8), when $\omega=0$.

The series in this case becomes

$$-\frac{1}{2} \left\{ \frac{(2-\epsilon)}{(3-2\epsilon)} + \frac{1}{2} \frac{(2-\epsilon)(4-3\epsilon)}{(3-2\epsilon)(5-4\epsilon)} + \frac{1}{3} \frac{(2-\epsilon)(4-3\epsilon)(6-5\epsilon)}{(3-2\epsilon)(5-4\epsilon)(7-6\epsilon)} + + \right\} \quad (11)$$

Consider now the general hypergeometric series of argument unity $F(a, b, c, 1)$. This is convergent if $c-a-b$ is positive. It has then (see Whittaker's 'Modern Analysis,' p. 241) the value

$$\frac{\Gamma(c)\Gamma(c-a-b)}{\Gamma(c-b)\Gamma(c-a)}.$$

Thus

$$\frac{F(a, b, c, 1) - 1}{a} = \frac{\Gamma(c)\Gamma(c-a-b) - \Gamma(c-b)\Gamma(c-a)}{a\Gamma(c-b)\Gamma(c-a)}$$

$$= \frac{1}{\Gamma(c-a)} \frac{\Gamma(c-a) - \Gamma(c)}{-a} - \frac{\Gamma(c)}{\Gamma(c-a)\Gamma(c-b)} \frac{\Gamma(c-a-b) - \Gamma(c-b)}{-a}.$$

Taking the limit when a becomes zero we have

$$\text{Lt}_{a=0} \frac{F(a, b, c, 1) - 1}{a} = \frac{\Gamma'(c)}{\Gamma(c)} - \frac{\Gamma'(c-b)}{\Gamma(c-b)}.$$

But

$$\text{Lt}_{a=0} \frac{F(a, b, c, 1) - 1}{a} = \frac{b}{c} + \frac{1}{2} \frac{b(b+1)}{c(c+1)} + \frac{1}{3} \frac{b(b+1)(b+2)}{c(c+1)(c+2)} + \dots$$

and if we put

$$b = \frac{1}{2(1-\epsilon)} + \frac{1}{2}$$

$$c = \frac{1}{2(1-\epsilon)} + 1,$$

this becomes

$$\frac{2-\epsilon}{3-2\epsilon} + \frac{1}{2} \frac{(2-\epsilon)(4-3\epsilon)}{(3-2\epsilon)(5-4\epsilon)} + \frac{1}{3} \frac{(2-\epsilon)(4-3\epsilon)(6-5\epsilon)}{(3-2\epsilon)(5-4\epsilon)(7-6\epsilon)} + \dots$$

Thus for $\omega=0$ the series given by (8) takes the value

$$-\frac{1}{2} \left\{ \frac{\Gamma' \left(\frac{1}{2(1-\epsilon)} + 1 \right)}{\Gamma \left(\frac{1}{2(1-\epsilon)} + 1 \right)} - \frac{\Gamma' \left(\frac{1}{2} \right)}{\Gamma \left(\frac{1}{2} \right)} \right\} \dots \quad (12)$$

(For tables of $\frac{\Gamma'}{\Gamma}$ functions see Gauss, *Werke*, Bd. iii. p. 161.)

As regards the limiting value of the third integral

$\frac{1}{\epsilon} \int \cosh^{\frac{1}{1-\epsilon}} \omega d\omega$, we have

$$\frac{1}{\epsilon} \int \cosh^{\frac{1}{1-\epsilon}} \omega d\omega = \frac{1-\epsilon}{\epsilon} \cosh^{\frac{1}{1-\epsilon}-1} \omega \sinh \omega + \int \cosh^{\frac{2\epsilon-1}{1-\epsilon}} \omega d\omega. \quad (13)$$

For infinite values of ω the value of $\frac{1}{\epsilon} \int \cosh^{\frac{1}{1-\epsilon}} \omega d\omega$ given by equation (9) is $\frac{1-\epsilon}{\epsilon} \cosh^{\frac{1}{1-\epsilon}} \omega$ if the value of ϵ be less than $\frac{1}{2}$. Thus, if the series is to represent the same integral as (13), we must insert ∞ as the lower limit of the integral on the right-hand side of the latter equation. Further, if $\epsilon < \frac{1}{2}$, then $\frac{2\epsilon-1}{1-\epsilon}$ is negative. Thus the series of equation (9)

is equal to $\frac{1-\epsilon}{\epsilon} \cosh^{\frac{1}{1-\epsilon}-1} \omega \sinh \omega - \int_{\omega}^{\infty} \frac{d\omega}{\cosh^{\frac{1}{1-\epsilon}} \omega}$.

Writing as before $\cosh \omega = p^{-\frac{1}{2}}$ we get for the value of the series when $\omega = 0$,

$$\begin{aligned} - \int_0^{\infty} \frac{d\omega}{\cosh^{\frac{1}{1-\epsilon}} \omega} &= - \frac{1}{2} \int_0^1 p^{\frac{1-2\epsilon}{2(1-\epsilon)}-1} (1-p)^{\frac{1}{2}-1} dp \\ &= - \frac{1}{2} \frac{\Gamma\left(\frac{1-2\epsilon}{2(1-\epsilon)}\right) \Gamma\left(\frac{1}{2}\right)}{\Gamma\left(\frac{1-2\epsilon}{2(1-\epsilon)} + \frac{1}{2}\right)} \dots \dots (14) \end{aligned}$$

A similar result may be readily obtained for the value of ϵ greater than $\frac{1}{2}$. We have thus got all the formulæ necessary for calculating the tables. It is desirable, however, that the integrals which go to make up v or x should be tabulated with the lower limit zero, as in this way we get x at once measured from one of the plates, and have therefore no trouble with the second constant of integration k_2 . Thus, putting for ϵ the value $\frac{4}{9}$ corresponding to $\frac{R_2}{R_1} = \frac{5}{4}$, we have as the solution of (1)

$$\begin{aligned} X &= \frac{4\pi i}{\sqrt{q\alpha}} \{P + k_1 Q\} \\ x &= \frac{4}{9} \frac{i}{eq} \{R + k_1 S\} \end{aligned} \dots \dots (15)$$

in which

$$\begin{aligned} P &= \frac{4}{5} \cosh^{\frac{5}{3}} \omega \int_{\omega}^{\infty} \frac{d\omega}{\cosh^{\frac{5}{3}} \omega} \\ &= \frac{4}{9} \left\{ 1 + \frac{1}{2} \frac{9}{19 \cosh^2 \omega} + \frac{1.3}{2^2 \underline{2}} \frac{9}{29 \cosh^4 \omega} + \frac{1.3.5}{2^3 \underline{3}} \frac{9}{39 \cosh^6 \omega} + + \right\}. \\ Q &= \cosh^{\frac{5}{3}} \omega. \\ R &= \int_0^{\omega} \left(\frac{9}{5} \cosh^{\frac{5}{3}} \omega \int_{\omega}^{\infty} \frac{d\omega}{\cosh^{\frac{5}{3}} \omega} \right) d\omega \\ &= \frac{1}{2} \left\{ \frac{\Gamma'(1.9)}{\Gamma(1.9)} - \frac{\Gamma'(.5)}{\Gamma(.5)} \right\} + \log \cosh \omega \\ &\quad - \frac{1}{2} \left\{ \frac{14}{19} \frac{1}{\cosh^2 \omega} + \frac{1}{2} \frac{14 \times 24}{19 \times 29} \frac{1}{\cosh^4 \omega} + + \right\}. \\ S &= \frac{9}{4} \int_0^{\omega} \cosh^{\frac{5}{3}} \omega d\omega \\ &= \frac{1}{2} \frac{\Gamma(.1) \Gamma(.5)}{\Gamma(.6)} + \frac{5}{4} \cosh^{\frac{5}{3}} \omega \left\{ 1 - \frac{1}{2} \frac{9}{\cosh^2 \omega} - \frac{1.3}{2^2 \underline{2}} \frac{9}{11 \cosh^4 \omega} - - \right\} \end{aligned} \dots (16)$$

Now at the positive plate

$$i + \frac{R_2}{8\pi} \frac{dX^2}{dx} = 0 \quad \text{or} \quad \frac{dy}{dv} + \frac{4\pi}{R_2 y} = 0,$$

while at the negative

$$i - \frac{R_2}{8\pi} \frac{dX^2}{dx} = 0 \quad \text{or} \quad \frac{dy}{dv} - \frac{4\pi}{R_1 y} = 0.$$

But equations (4) give

$$\frac{dy}{dv} = \frac{\frac{dy}{d\omega}}{\frac{dv}{d\omega}} = - \frac{4\pi(1-2\epsilon)}{(1-\epsilon)(R_2-R_1)y} + \frac{\sqrt{\alpha}(1-2\epsilon)}{\epsilon(1-\epsilon)(R_2-R_1)} \tanh \omega.$$

For $\epsilon = \frac{R_1}{R_1+R_2}$ this gives

$$\frac{dy}{dv} = - \frac{4\pi}{R_2 y} + \sqrt{\alpha} \left(\frac{1}{R_1} + \frac{1}{R_2} \right) \tanh \omega.$$

Thus at the positive plate $\omega = 0$.

At the negative plate

$$-4\pi \left(\frac{1}{R_1} + \frac{1}{R_2} \right) \frac{1}{y} + \sqrt{\alpha} \left(\frac{1}{R_1} + \frac{1}{R_2} \right) \tanh \omega = 0,$$

or

$$\frac{\sqrt{\alpha}}{4\pi} y \tanh \omega = 1.$$

Thus if $P_1, Q_1, R_1, S_1, \omega_1$ are the values of the respective functions at the cathode,

$$(P_1 + k_1 Q_1) \tanh \omega_1 = 1. \quad . \quad . \quad . \quad (17)$$

Suppose l is the distance between the plates, we have

$$l = \frac{4}{9} \frac{i}{eq} \{R_1 + k_1 S_1\}. \quad . \quad . \quad . \quad (18)$$

From (17) and (18) we may eliminate k_1 and obtain

$$\frac{S_1}{Q_1} \left(\frac{1}{\tanh \omega_1} - P_1 \right) + R_1 = \frac{9}{4} \frac{eq}{i} l. \quad . \quad . \quad (19)$$

This is a transcendental equation giving the value of ω or $\cosh \omega$ at the cathode.

In order to facilitate its solution, the value of the function

$$\frac{S}{Q} \left(\frac{1}{\tanh \omega} - P \right) + R \equiv T$$

is tabulated along with the corresponding values of P , Q , R , and S . Having in this way determined the value of $\cosh \omega$ at the negative plate we may find k_1 thus

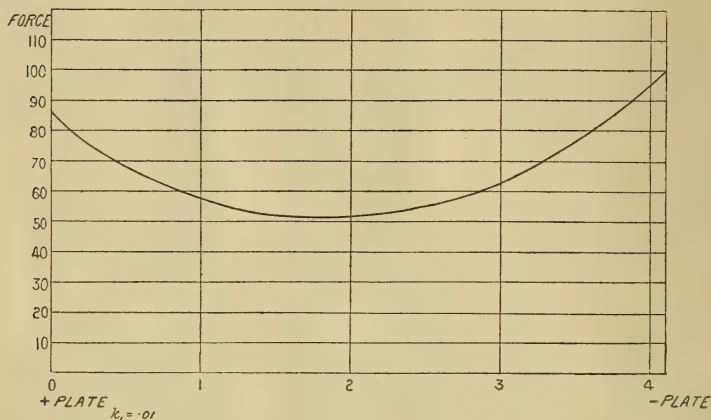
$$k_1 = \frac{T_1 - R_1}{S_1}, \quad . \quad . \quad . \quad . \quad . \quad (20)$$

and so complete the solution.

$\cosh \omega$.	P .	Q .	R .	S .	T .
1.0	.854	1.000	0.000	0.000	0.000
1.1	.615	1.187	0.717	1.059	2.310
1.2	.564	1.388	0.953	1.574	2.365
1.3	.536	1.604	1.119	2.024	2.417
1.4	.518	1.832	1.250	2.450	2.468
1.5	.503	2.075	1.360	2.869	2.516
1.6	.496	2.330	1.455	3.287	2.562
1.7	.489	2.599	1.540	3.709	2.607
1.8	.483	2.881	1.616	4.139	2.650
1.9	.478	3.175	1.686	4.576	2.692
2.0	.475	3.482	1.750	5.023	2.731
2.1	.471	3.802	1.809	5.481	2.769
2.2	.469	4.134	1.865	5.950	2.806
2.3	.466	4.478	1.917	6.430	2.842
2.4	.464	4.835	1.966	6.923	2.876
2.5	.463	5.203	2.013	7.428	2.909
2.6	.461	5.584	2.057	7.945	2.942
2.7	.460	5.977	2.099	8.475	2.974
2.8	.459	6.381	2.140	9.017	3.005
2.9	.458	6.797	2.178	9.573	3.034
3.0	.457	7.225	2.215	10.141	3.063
3.1	.456	7.664	2.251	10.722	3.091
3.2	.455	8.115	2.285	11.316	3.119
3.3		8.578	2.319	11.924	3.145
3.4	.454	9.050	2.351	12.544	3.171
3.5		9.535	2.381	13.177	3.197
3.6	.453	10.031	2.411	13.823	3.222
3.7		10.538	2.440	14.482	3.246
3.8	.452	11.056	2.469	15.154	3.270
3.9		11.586	2.496	15.839	3.293
4.0	.451	12.126	2.523	16.537	3.316
4.1		12.677	2.548	17.248	3.338
4.2		13.239	2.574	17.972	3.360
4.3	.450	13.812	2.598	18.708	3.381
4.4		14.395	2.622	19.458	3.402
4.5		14.989	2.645	20.220	3.422
4.6		15.594	2.668	20.995	3.442
4.7		16.210	2.690	21.783	3.462

cosh ω .	P.	Q.	R.	S.	T.
4.8	449	16.836	2.712	22.584	3.481
4.9		17.472	2.733	23.397	3.500
5.0		18.119	2.754	24.223	3.519
5.1		18.777	2.775	25.061	3.537
5.2		19.445	2.795	25.912	3.555
5.3		20.123	2.814	26.776	3.573
5.4	448	20.812	2.833	27.652	3.590
5.5		21.511	2.852	28.541	3.607
5.6		22.220	2.871	29.442	3.624
5.7		22.939	2.889	30.356	3.641
5.8		23.668	2.907	31.281	3.657
5.9		24.408	2.924	32.220	3.673
6.0		25.158	2.941	33.170	3.689
6.1		25.918	2.958	34.133	3.704
6.2		26.687	2.975	35.109	3.719
6.3		27.467	2.991	36.096	3.734
6.4	447	28.257	3.007	37.096	3.749
6.5		29.057	3.023	38.108	3.764
6.6		29.866	3.038	39.131	3.778
6.7		30.686	3.054	40.168	3.793
6.8		31.515	3.069	41.216	3.807
6.9		32.354	3.084	42.276	3.820
7.0		33.203	3.098	43.348	3.834
7.1		34.062	3.113	44.433	3.847
7.2		34.930	3.127	45.529	3.861
7.3		35.808	3.141	46.637	3.874
7.4		36.696	3.155	47.757	3.887
7.5		37.593	3.168	48.889	3.900
7.6		38.500	3.182	50.033	3.913
7.7		39.417	3.195	51.189	3.925
7.8		40.343	3.208	52.356	3.937
7.9		41.279	3.221	53.536	3.949
8.0		42.224	3.234	54.727	3.961
8.1		43.179	3.246	55.930	3.973
8.2	446	44.143	3.258	57.144	3.985
8.3		45.117	3.271	58.370	3.997
8.4		46.100	3.283	59.608	4.008
8.5		47.093	3.295	60.858	4.020
8.6		48.095	3.307	62.119	4.031
8.7		49.106	3.318	63.391	4.042
8.8		50.127	3.330	64.676	4.053
8.9		51.157	3.341	65.971	4.064
9.0		52.196	3.353	67.279	4.075
9.1		53.244	3.364	68.597	4.086
9.2		54.302	3.375	69.928	4.096
9.3		55.369	3.386	71.269	4.107
9.4		56.446	3.396	72.622	4.117
9.5		57.531	3.407	73.987	4.127
9.6		58.626	3.418	75.363	4.137
9.7		59.730	3.428	76.750	4.147
9.8		60.842	3.438	78.149	4.157
9.9		61.965	3.449	79.558	4.167
10.0		63.096	3.459	80.980	4.177
13.8	445				
∞	$444 = \frac{4}{9}$				

We append a sample curve drawn with the help of these tables.



The case of $\epsilon = \frac{R_2}{R_1 + R_2}$ gives $\tanh \omega = 0$ at the negative plate, while the value at the positive is determined by a transcendental equation.

LXXV. *On Dielectric Strain along the Lines of Force.* By LOUIS T. MORE, Ph.D., Professor of Physics in the University of Cincinnati*.

ALTHOUGH I have reviewed in former papers the work accomplished in electrostriction, yet, as the stress-problem is undoubtedly the fundamental one in the theory of electricity, I may be permitted to summarize briefly what has been done in this branch of the stress-problem before describing any new experiments.

By electrostriction is meant the strain in dielectrics and their consequent mechanical deformation, produced by the action of electricity in them. The effect is supposed to occur in solid, liquid, and gaseous non-conductors when they are electrified. The first observations date back to the beginning of the last century. In its early stages, electrostriction was looked upon as an isolated property of electricity, and although laws were formulated, it was not until Faraday and Maxwell adduced the effect as one of the fundamental phenomena on which to build their new theory of ætherial stresses, that any

* Communicated by the Author. Read in part before the International Electrical Congress, St. Louis, 1904; and in part before the American Physical Society, March 1905.

considerable theoretical importance was attached to this property of electricity. That they did so regard it as one of the corner-stones of their theory is clearly shown by their own statements. Thus Faraday says (Par. 1297):—"The direct inductive force, which may be conceived to be exerted in lines between the two limiting and charged conducting surfaces, is accompanied by a lateral or transverse force equivalent to a dilatation or a repulsion of these representative lines;" and (Par. 1224):—"The attractive force which exists amongst the particles of the dielectric in the direction of the induction is accompanied by a repulsion or a diverging force in the transverse direction." Maxwell ('Electricity and Magnetism,' vol. i. p. 165) writes even more explicitly:—"The hypothesis that a state of stress of this kind exists in a fluid dielectric, such as air or turpentine, may at first sight appear at variance with the established principle that at any point in a fluid the pressures are equal. But in the deduction of this principle from a consideration of the mobility and the equilibrium of the parts of the fluid, it is taken for granted that no action such as that which we here suppose to take place along the lines of force exists in the fluid. The state of stress which we have been studying is perfectly consistent with mobility and equilibrium of the fluid, for we have seen that, if any portion of the fluid is devoid of electric charge, it experiences no resultant force from the stresses on its surface, however intense these may be. It is only when a portion of the fluid becomes charged that its equilibrium is disturbed by the stresses on its surface, and we know that in this case it actually tends to move."

As is well known, he measures this stress by the famous formula $p = \frac{\bar{K} V^2}{8\pi d^2}$, where p is the numerical value of the tension, K the specific inductive capacity of the dielectric, and V/d the fall of potential per unit length in the dielectric.

In support of this theory and equation a number of investigators have obtained an expansion both in the volume of an electrified glass thermometer and an elongation of a glass tube electrified radially, and have apparently found these deformations agree with the theoretical formula. When the mechanical is substituted for the electrical pressure, by the

formula $p = F/S = \mu \frac{\delta l}{l}$, we obtain $\frac{\delta l}{l} \cdot \frac{d^2}{V^2} = \frac{K}{8\pi\mu}$. The left-

hand member is the expansion per unit length of the tube under unit electrical conditions at right angles to the lines of force. By taking the average of Cantone's results we get

the value $\frac{\delta l}{l} \cdot \frac{d^2}{V^2} = 6.5 \times 10^{-13}$; when the best value for Young's modulus, μ , and the dielectric constant for the glass are substituted in the right-hand member, there results a value of the same order and about one-half as large. Quincke's average results are in somewhat closer agreement. This discrepancy they assumed to be due to the inadequacy of Maxwell's formula, which should contain another term, signifying the relation between mechanical and electrical pressures.

Passing over the cruder work of early writers, the best direct results were obtained by Quincke* and Cantone†. In previous papers‡ I attempted to show, by their own results, that both neglected to consider extraneous effects which were of the same magnitude as their recorded values and which, if introduced, would have accounted for the entire deformations observed. These errors are due chiefly to heating, lateral displacements, distortions caused by using thin tubes more or less irregular in diameter and thickness, or by the lack of sphericity in any blown bulb of glass. Quite recently Wüllner and Wien§ attacked the problem in an indirect manner, attempting to show that the elasticity of glass is greater when obtained from electrical stress than when found in the usual manner by mechanical or acoustical methods. It is interesting to note the contradictions of these different observers. Wüllner and Wien confirm Quincke's conclusions, though not his values, and condemn Cantone's and mine; while Sacerdote and Cantone consider Quincke's to have little value. In the first place, the measurement of the rise or fall of an electrified capillary water column, such as Wüllner and Wien use, is in my opinion a faulty method, and their agreement with Quincke, who often employed the same method, is natural. I may add that this method is condemned by other investigators. Wüllner and Wien themselves have to introduce elaborate corrections to obviate the irregularities in the motion of the water column. Secondly, their results show great variations in the value of the elasticity, measured mechanically and electrically, for different kinds of glass. For some glass the latter is not more than one-half of one per cent., but for others it is as much as one hundred per cent.

* Quincke, Wied. *Ann.* Bd. x. pp. 161, 374, 513; Bd. xix. pp. 545, 705; Bd. xxviii. p. 529; Bd. xxxii. p. 503.

† Cantone, *Rend. d. R. Accad. Linc.* t. iv. pp. 341, 471; *Rend. d. R. Ist. Lomb.* [2] t. xxxiii.; *Nuovo Cim.* [4] t. xii. p. 150.

‡ More, *Phil. Mag.* [5] vol. i. p. 198, [6] vol. ii. p. 527, [6] vol. vi. p. 1; *Elect. World and Engineer*, vol. xliii. p. 127.

§ Wüllner and Wien, *Drude's Ann.* [4] Bd. ix. p. 1217.

greater. A variation of such magnitude due solely to differences in the composition of the glass is out of all proportion, and certainly if electrifying glass may more than double its ordinary elasticity, such an elaborate method of experimentation is unnecessary. Lastly, a variation in elasticity is directly contrary to theory. Lippmann, and his formulæ are shown by Sacerdote to be essentially in agreement with the other writers on the theory of this subject, states :—"That the dilatation produced electrically must be due to a direct action of electricity *and cannot be caused by a variation of the coefficient of elasticity*, or, *the coefficient of elasticity is independent of the electrification*; on the other hand, the dielectric constant varies with the electrification."

Some years ago I published a paper in the Philosophical Magazine containing experiments which did not confirm the conclusions just cited, and in fact made me doubt the existence of electrostriction. It was criticised by Sacerdote and Cantone on the ground that my method was faulty; they claimed I had employed a mechanical instead of an optical device for magnifying the elongations, and that it was not sufficiently delicate to observe this minute effect. The criticism was easy to meet, and in a letter to the Philosophical Magazine I demonstrated that my apparatus was an optical system, as sensitive or even a little more sensitive than interference methods, and that their criticism of its lack of sensibility and accuracy was founded on a misinterpretation. Later work has confirmed my opinions.

Maxwell's formula has been discussed by Korteweg *, Lorberg †, Kirchhoff ‡, Lippmann §, Sacerdote || and others. Their methods may be divided into two categories: first, the determination of the attractions and repulsions of electrified conducting particles immersed in a non-conducting medium, or in other words the results of a polarization of the dielectric; and secondly the application of the thermodynamic equation and the interdependence of the laws of conservation of energy of electricity and matter. Sacerdote has made a careful synopsis of all the articles published on this subject, with a critical discussion of methods and results. He finds, barring small differences, that both general methods lead to the same conclusion, which may be expressed by the formula

$$p = (K_1 + a) \frac{KV^2}{8\pi d^2},$$

* Korteweg, Wied. *Ann.* Bd. ix. p. 48.

† Lorberg, Wied. *Ann.* Bd. xxi. p. 300.

‡ Kirchhoff, Wied. *Ann.* Bd. xxiv. p. 52.

§ Lippmann, *Annal. de Chim. et de Phys.* [5] t. xxiv. p. 159.

|| Sacerdote, *Thèse*.

where a is the inverse of Young's modulus and K_1 the variation of the dielectric constant with mechanical pressure, represented by $\delta K/\delta f$. It is then Maxwell's equation with the addition of a second constant. This variation of the dielectric constant must be an exceedingly small quantity, for of the few who have investigated it, some have obtained a positive value, some a negative, and others a null effect. So that the pressure p may be increased, diminished, or unaffected by this quantity.

If one is willing to grant the fundamental assumption made for these theories, there is little doubt that the conclusions are correct. It is easy to see that the assumption made for each of them is essentially the same; namely, that a stress in the æther is communicated to matter immersed in it, or, in other words, the granting of the vital point of the discussion. Those who develop the polarization theory suppose material particles to be immersed in a fluid æther, as may be illustrated by fragments of silk thread in pure turpentine. If charged electrodes are placed in the turpentine, the silk threads are observed to form chains extending approximately along the Faraday lines of force. We may readily imagine the ends of each fibre to be polarized by induction which by their mutual attractions and repulsions form lines of stress tending to shorten along the lines of force and to repel each other perpendicularly to them. In the first place, it is rather doubtful to consider this phenomenon as due to a static system of forces which the Faraday lines demand. But the real error lies deeper,—to assume that particles of matter in the æther act like particles of silk in turpentine is to grant the whole question. We know that a stress in one kind of matter is transmitted undiminished to any other kind of matter, as in this case from turpentine to silk. But we not only have no confirmation, but in fact many reasons for believing that static stresses in the æther are not communicated to matter. Lorentz indeed goes so far as to say that it is absurd to imagine a strain in the æther itself and so impossible to conceive of a strain imparted to matter. One of the chief reasons for the abandonment of the elastic solid theory of light in favour of the electro-magnetic theory is to obviate the absurdities introduced when the æther was supposed to be an elastic solid. As to the theories viewed from a thermodynamic standpoint, the assumption is just as glaring as it is in fact the same. Sacerdote and the other writers on electrostriction from this standpoint claim that the connecting link between the æther and matter is expressed by the coefficient $K_1 = \delta K/\delta f$, that is by the variation of the inductive capacity

with mechanical pressure. In the first place, no such variation has been found experimentally, as the results so far are discordant and subject to serious criticism. The question still remains, whether if it were found that K changes when a body is distorted mechanically, electrostriction necessarily exists. We know already that as a general rule this constant is least for gases, greater for liquids and greatest for solids, or it varies approximately as the density of the matter. We may then readily imagine that an increase in density caused by pressure will change the dielectric constant slightly because of a variation in the elasticity; but the theorists maintain that electrostriction is due to a direct action of electricity and not to a variation of the coefficient of elasticity, as shown by my former quotation from Lippmann. Wüllner and Wien, who alone regard the effect as being due to such a variation in elasticity, have certainly mistaken the nature of the problem. Their results, as stated before, show large variations, equal at times to the value of the whole coefficient of elasticity, in the elasticity of electrified and unelectrified glass. And though I believe that this difference must have been exaggerated by extraneous effects, still such a variation may exist in a less degree, but it has no bearing on the subject of electrostriction. The question may be put in this form. Does a mechanical pressure (a stretching force which produces a decrease in density should theoretically increase the dielectric power) cause a variation in the mutual relations of the æther and matter? This is the same question discussed with respect to the polarization theory.

There seems to be a considerable degree of confusion prevalent in regard to the significance of electrostriction. It does not involve the mechanical pressure produced by the attraction of two charged bodies, but is supposed to be a property of the electric flux in a non-conductor. For example, two charged bodies attract each other, and if they touch a dielectric, glass for example, placed between them, the glass must be deformed mechanically whatever theory is adopted. But those who believe in electrostriction maintain that if the charged plates be separated from the glass, and if the intervening spaces be filled with a fluid dielectric of the same dielectric constant as the glass and filled in such a manner that the mechanical force of attraction of the electrified armatures cannot be communicated by the fluid to the glass, still the latter will contract in the direction of the lines of force and expand perpendicularly to them. This mechanical deformation, called electrostriction, is due then entirely to the electrical stresses in an æther and communicated by it to

the matter immersed in it. As an illustration, we can make use of the system of glass and liquid dielectrics just described. The lines of electric flux in the æther stretch from one charged armature to the other and may experience, for all we know, the tensions and pressures generally ascribed to them. Since both the glass and the liquid have the same dielectric constant, there is no discontinuity of the lines at the surface of the glass, yet it is supposed to contract in their direction. The only mechanical arrangement I can think of, is to assume that each line does not extend from armature to armature but is like a chain linking particle to particle of the glass. Let us, to be more precise, consider the action of a tension in one of these chain lines on three particles,—one in the surface of the liquid adjoining the glass, a second in the surface of the glass, and the third just within the surface of the glass. Since the dielectric constant is the same in the two substances, the pull on the middle particle is the same in the two directions, and there is no tendency to displacement of the surface of the glass. This is also true for any three particles *in* the glass, and a contraction does not occur. Of course, where there is a discontinuity of the lines, either in passing from one dielectric to another of a different inductive capacity or from a dielectric to a contiguous conductor, an unbalanced force exists, but that is merely the measure of the attraction of the two charged bodies. If the lines of flux are supposed to pass through the glass without being attached to its particles, it is even more difficult to see how the stress in the æther effects the glass.

If we assume Maxwell's formula for electric stress in the æther, and I know no other adequate, we must not lose sight of a most important fact, that theories involving the æther must always remain purely hypothetical, as we have no possible method of experimentation upon it. And only when matter is immersed in the æther does experimentation become possible. Now all our observation goes to show, that for static and even for kinetic phenomena, at least where the velocity is less than that of light, the æther is unmodified by the presence of matter and does not affect the properties of it: witness the absence of friction between the earth and æther in its motion about the sun; the Kerr effect of the polarization of transparent matter when electrified; Brace's experiments on the action of a magnetic field on transparent media showing that the Faraday stresses affect the velocity of light by an amount less than 2.0×10^{-14} for a c.g.s. unit of intensity per centimetre; and many others all supporting the doctrine of the independence of æther and

matter. My claim is, not that Faraday stresses may not exist in the æther, but that these stresses are not transferred in any appreciable degree to matter as *unbalanced* mechanical forces. The fact is, a dynamic system in a hypothetical medium may be either affirmed or denied, but it cannot be proved as the question is a metaphysical one. The absence of the electrostrictive effect does not preclude the existence of ætherial stresses, but it does prevent it from serving as a fundamental experimental verification of the stress-problem.

The recent discussion of Maxwell's stress theory by Bjerknes* is opportune to this subject, and his conclusion should be quoted:—"In the meanwhile it will therefore be safest to consider the Maxwell stresses as only *fictional* stresses that might have produced the required forces, and not as the real stresses which do produce them. Other authors have also termed them fictional stresses, especially Lorentz, who also considers the stress formulæ only as useful analytical formulæ, but not as representing any physical reality. The reason, however, why he has come to this opinion is quite different from the reason brought forward here. His view is that the stress-problem in itself has no physical meaning. For, according to the doctrine of the immobility of the æther, it is an absurdity to speak of forces acting upon or stresses existing in the æther."

Dielectric Strain along the Lines of Force.

Maxwell's theory, which supposes an expansion perpendicularly to the lines of force, also demands an equal contraction in their direction. A test of this latter effect has never, so far as I know, been attempted: it is obviously a much more difficult piece of work, but it also should prove to be of greater importance. In the former method, the effect sought and the extraneous effects are all in the same direction and are difficult to separate. But the effect along the lines of force should be a contraction, and so opposite in effect to the heating and other sources of error. Furthermore, it does not introduce a correction for the little-known value for Poisson's ratio. It is besides possible to reduce the magnitude of the disturbances themselves.

It is evident that a sheet of dielectric placed between two metal electrodes will be compressed by the attraction of these electrodes when they are charged. We must neutralize this attractive force by making a system of balanced electric forces. This can be best done by building up a pile of alternate metallic and dielectric plates, each held horizontal and

* V. Bjerknes, *Phil. Mag.* vol. ix. p. 491 (1905).

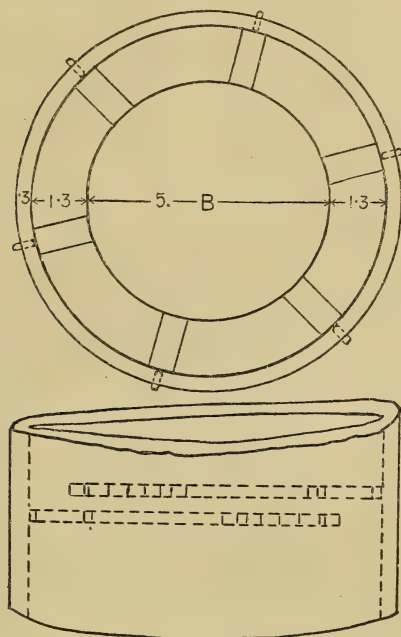
accurately equi-distant. Alternate metal plates are then connected by wires into two sets: one set to be charged positively and the other negatively. The number of metal plates must be odd, so that the top and bottom plates may be charged with electricity of the like sign and connected together rigidly by metal rods. Each plate in this system is thus attracted equally up and down, or is electrically in equilibrium. Whereas, if the dielectric itself expands or contracts along the lines of force, the whole pile will tend to shorten or lengthen according to the formula $\mu \frac{\delta d}{d} = \frac{nKV^2}{8\pi d^2}$, where d is the thickness of each dielectric plate, K the dielectric constant, V the potential, n the number of dielectric plates in the pile, and μ is Young's modulus.

To accomplish this object, it was necessary to cast a large number of plates, which should be exactly parallel, equi-distant and close together, in a dielectric, to form a solid block. A great deal of difficulty was found in getting a dielectric which was a good insulator for high potentials, and at the same time rigid and not viscous, and which would not contract when solidifying. The last was very important and debarred paraffin, as this and similar substances contract sufficiently to fissure in the thin places between the electrodes. The substance finally adopted was a mixture of four parts by weight of the best shellac, one of resin and two of Venice turpentine. This compound gives a clean and smooth surface when broken, and is highly elastic (Young's modulus about 10^{10}). It melts at 110° and when solidified shows little contraction,—a two-litre flask, half full of the hot liquid, presents an almost level surface when it solidifies.

Several types of moulds for the casting were made and tried before the one was devised which answered. Thirty-one brass plates, 5 cm. in diam. and 2.5 mm. thick, were polished and made true. Three lugs, 12 mm. long and 6 mm. wide, were soldered radially to each plate. A seamless brass tube, 21.5 cm. long and 7.4 cm. inner diameter, was then ruled with thirty-one circular lines, each 5.5 mm. apart, and three holes were bored and tapped, equally distant apart on each line. The plates were then placed each in the plane of one of these circles, with a lug opposite each hole and held in position by three small pointed screws for each plate. Two of the plates are shown mounted in a section of the tube in fig. 1. In the next to the top plate was screwed a brass tooth of the same dimensions as the ebonite tooth shown in fig. 2 (p. 686), and a hole was bored in the top plate so that the tooth passed through it. The bottom plate was tapped with three screw-holes and a large plate, only a little less in

diameter than the mould, was fastened to it by screws. The ends of the moulds were closed by brass screw-caps. After

Fig. 1.

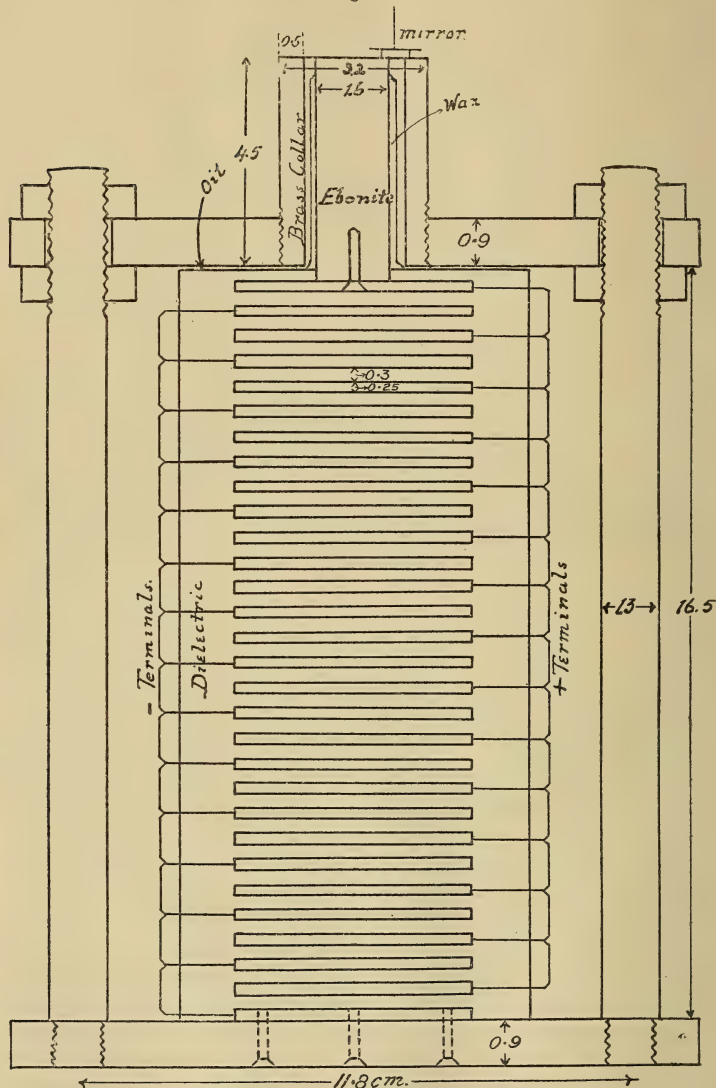


the casting was made, the caps and holding screws were removed, and the perfect casting slipped out of the mould by heating it quickly. The large brass plate at the bottom was removed by heating it gently, and the bottom electrode, thus exposed, was fastened to the base plate of the frame of the apparatus, fig. 2. Then the brass tooth was replaced by the one of ebonite. During this operation the top brass plate was slipped off, this being done so that the upper plate of the frame might replace it as an electrode.

When the casting was out of the mould, the ends of the three lugs on each plate were exposed, forming two sets of three vertical lines each. Two of these lines of lugs of each set were covered with insulating wax to prevent leakage, and the lugs on the third line of each set were connected by wires so that one set might be charged and the other grounded, fig. 2. After the electrical connexion was made and tested, these terminals were also covered with the insulating wax. We thus have sixteen plate-electrodes (counting the upper plate of the frame as one), connected to the ground, of which the top and bottom ones were fastened rigidly together, and

fifteen plate-electrodes connected to the electrical machine. The attraction of these plates when charged is evidently

Fig. 2.

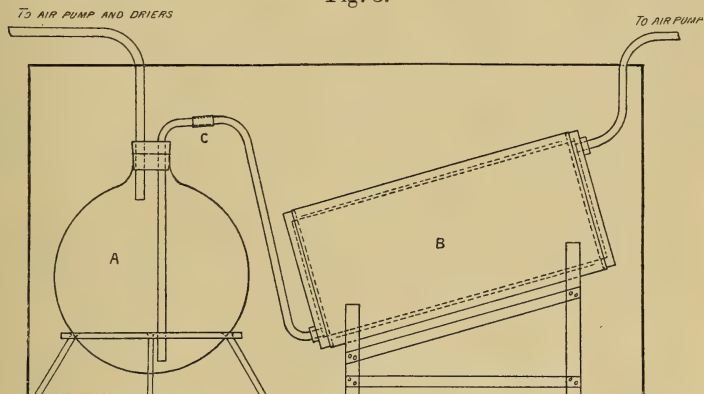


neutralized as they are equally attracted up and down. Between the electrodes are thirty dielectric plates, the whole forming a solid mass except for the discontinuity between the upper dielectric plate and the upper electrode.

The greatest difficulty experienced while making these

condensers is to avoid air-holes and fissures, as the electrodes are only three millimetres apart. I did not accomplish this until I used the arrangement shown in fig. 3, where the

Fig. 3.



whole process takes place in a single bath and with a good vacuum. The constituents, ground and thoroughly mixed, were placed in the two litre-flask, A, in a bath of machine-oil kept at a temperature a little above 110° . From the bottom of the flask a glass and copper tube connects with the bottom of the mould, B, through a stop-cock at C. A second tube from the flask but above the surface of the mixture connected the flask to a water vacuum-pump through driers; and a copper tube from the other end of the mould made connexion with a "Geryk" vacuum-pump. The cock, C, was first closed, and the mixture liquefied under a vacuum made by the water-pump. This heating was continued until the mass formed a homogeneous, smooth fluid. The "Geryk" pump was then started, and when a high vacuum in B was obtained the cock, C, was opened and the fluid gradually forced into the mould by regulating the pressure above the fluid in the flask,—the "Geryk" was worked vigorously until the mould was entirely filled. The pumps were then disconnected and the bath allowed to cool slowly.

Returning now to the completed casting, a stout frame was made of two brass plates, connected by two brass rods (fig. 2). The bottom electrode was fastened, as stated before, to the base plate of this frame and it, in turn, to heavy beams bolted to masonry piers in a basement room. A hole was bored in the top plate of the frame and a brass cylinder screwed into it so that the base of the cylinder was flush with the lower surface of the plate and the upper end on a level with the top of the ebonite tooth. The legs of a tilting mirror were then placed on these upper ends of the cylinder

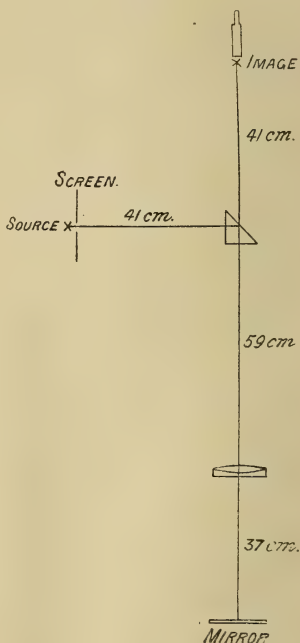
and tooth. The position of the top plate of the frame was regulated by the set screws at the top of the rods. The dielectric exposed above the uppermost charged electrode was made slightly less thick than that between the other electrodes, and the space between the upper surface of the dielectric and the top plate of the frame was filled with a thin layer of oil of the same dielectric constant as the shellac mixture, and the distance between the two electrodes was adjusted so that this heterogeneous dielectric was electrically equivalent to that between any other two of the electrodes. The ebonite tooth was covered with insulating wax to prevent leakage or sparking from the positively charged electrode.

The method of measuring the deflexions was the same as that employed in my former work, and the reader is referred to those papers for a detailed description. I give here only a brief account of it.

The system of optical levers used to magnify the expansion of the condensers is shown in fig. 4. An incandescent electric lamp with a ground glass globe was clamped behind a metal screen pierced with a round hole, five millimetres in diameter. Across this opening fine platinum wires were fastened horizontally, to serve for the deflected image. The light from the lamp, after reflexion at right angles by a totally reflecting prism, passed through an achromatic lens to a tilting mirror mounted vertically on a little tripod table. Upon reflexion, the light again passed through the lens and on, above the prism, to a micrometer microscope.

The mirror, 1.5 cm. $\times$ 2.0 cm., furnished by Brashear, was silvered on its front face and plane to $1/4 \lambda$. The tilting table of brass, 1.6 cm. square, had three legs made of the finest needle points, one of which rested on the brass cylinder and the other two on the ebonite tooth. By slightly raising or lowering the cylinder, the beam of light could be directed at a

Fig. 4.



desired angle. Small weights which hung below the level of the table from an arm increased the stability to such a degree that the image showed no oscillation from outside disturbances. The prism and lens were both of the best construction, and the microscope was a new instrument obtained especially for these experiments. The platinum wires and the microscope were each placed in the principal focus of the lens; the image was then very sharp and distinct. The least change in length which can be observed is calculated from the following dimensions:—

Dimensions for Magnifying Power.

Focal length of lens	100 cm.
Distance between feet on tripod I	6 mm.
Distance between feet on tripod II	3 mm.
One division on micrometer of microscope.	0·002 mm.

A deflexion of one division of the micrometer, using tripod I, is equal to a change of length in the condenser of $\frac{6}{2 \times 1000} \times 0\cdot002 = 6 \times 10^{-6}$ mm., and with tripod II 3×10^{-6} mm. This minimum deflexion could be easily observed. In compiling readings, they were reduced to the basis of 6×10^{-6} mm. for one division, whichever tripod was used*.

The criticisms made on this type of apparatus have been a surprise to me, as I supposed such a standard form would impress all with its capabilities for delicacy and accuracy. In the first place, it is as much a form of optical device as the interferometer, and it has just about the same refinement as all optical devices, namely, its limit involves the wave-length of light just as interference methods do, and on the other hand it seems to me better adapted to the experiments under discussion.

During preliminary experiments made to test the insulation and the dielectric strength of the condenser, I inserted a sheet of soft rubber between the lower surface of the top plate of the frame and the upper plate of the dielectric, using no oil. With this arrangement I noticed small deflexions of the mirror, but in every case the deflexion showed an elongation of the dielectric, and not the contraction the theory calls for. This elongation was also much smaller than the theories of electrostriction demand. I thought at first that it was due to

* I wish here to express my thanks and indebtedness to Mr. P. B. Evens, the University mechanician, for the skill and patience he has exercised in the making of all this apparatus which made these experiments possible.

some action in the dielectric, but later found that it was caused by charges induced on the rubber which attracted the tilting mirror statically.

During the final experiments which continued through two months, the rubber sheeting was replaced by pure lard oil. The dielectric constant is almost exactly that of the shellac mixture. The height of the frame was carefully adjusted to make the electrical forces a balanced system, as I have previously shown to be necessary.

When these precautions were taken the mirror showed absolutely no deflexion when the plates were charged to any potential up to the breaking point for the dielectric.

The conditions were varied as far as possible. Sometimes the charging was done slowly and sometimes quickly, ranging from five seconds to a minute or more, and at times Leyden jars were included in the circuit and sometimes not, so that the quantity of charge might be varied.

From the nature of the construction of the condenser surface leakage was obviated, as little or no metallic surface was exposed, and in my former work I showed that surface currents were principally responsible for the heating. The currents through the dielectric are always small, and their effect was so much retarded in time by the large mass of dielectric through which the heat must diffuse, that the condenser could be charged and discharged before the slow creep of the image due to this occurred.

Potentials which produced a spark of 4 millimetres between balls 1 centimetre in diameter were first used, and then increased gradually a millimetre at a time until a spark of 16 millimetres was reached. This latter corresponds to a potential of about 130 c.g.s.

In not a single case did the mirror show either an elongation or a contraction.

When the spark passed, there was usually a slight tremor or jump of the mirror of not more than six or seven divisions of the micrometer, and this was sometimes in the direction of an elongation and sometimes a contraction. These were found to be due to the explosion of the spark transmitted through the air. To prove this, the condenser was disconnected from the electric circuit and sparks passed between the balls of the micrometer; in every case the same jump of the mirror was observed, although the condenser was not electrified. Shearer mentions this same disturbance while using an interferometer as a measuring device.

That my apparatus was amply sensitive is seen from the following calculation.

The equation for elasticity introduced into Maxwell's formula gives the result :

$$F/S = \mu \frac{\delta d}{d} = \frac{nKV^2}{8\pi d^2} \text{ or } \delta d = nKV^2/8\pi d\mu,$$

where

n , the number of plates	= 30
K , the dielectric constant	= 2.5
V , max. potential difference	= 130 C.G.S.
d , distance between electrodes ...	= 0.3 cm.
μ , Young's modulus	= 10^{10}

substituting,

$$\delta d = 168 \times 10^{-7} \text{ cm.}$$

This value corresponds to 56 divisions of the micrometer, using tripod II, or 28 divisions using tripod I.

These results certainly are difficult to ignore and can lead but to one interpretation, and that is the lack of influence of the æther on matter in this respect.

Dielectric Strain Perpendicular to the Lines of Force.

Before the experiments already described in this paper were begun, a series of tests for the dielectric strain perpendicular to the lines of force was undertaken with this same shellac mixture for a dielectric. The method of experimentation and the apparatus, with the exception that the cylindrical condensers were of the shellac mixture instead of glass, were exactly the same as described in the *Philosophical Magazine*, vol. vi. p. 1, 1903. Cylinders of the shellac mixture were cast in a mould made of two coaxial brass cylinders. Condensers of paraffin were also cast in the same mould.

Dimensions of Cylinders.

No.	Composition.	Length.	Outer diam.	Inner diam.	Thickness.	μ .	K.
7.	Shellac.	64.6 cm.	4.7 cm.	3.9 cm.	0.4 cm.	10^{10}	2.5
9.	Shellac.	64.8	4.7	3.9	0.25	10^{10}	2.5
8.	Paraffin.	64.8	4.7	3.9	0.4	10^{10}	2.5

These condensers were mounted in the same manner as the glass ones in my earlier experiments. The values for Young's modulus were obtained in the usual manner by placing weights on the ends of the cylinders and measuring their contraction. They are fairly accurate for the shellac, but the value for the paraffin is not so reliable as the substance is decidedly viscous. The values for both are given rather too large than too small.

Tube No. 7 was a little irregular in thickness, as it distorted somewhat when taken from the mould. But the results are of importance as they show the effect of a slightly non-uniform field. Tube No. 9, after removal from the mould, was turned in a lathe and was quite uniform in bore and thickness.

Tube 7.—Adherent Armatures.

Spark-length.	Potential.	Mean elong. micr. div.	Mean elong. $\delta l \times 10^4$.	Time of charging.
5.0 mm.	60 c.g.s.	27.0	1.62	0.25 min.
7.5	85	35.0	2.10	0.25
7.5	85	70.0	4.20	1.00
10.0	100	55.0	3.30	0.25
10.0	100	100.0	6.00	1.00
10.0	100	180.0	10.80	5.00
12.0	110	133.5	8.01	0.25
12.0	110	242.5	14.55	1.00

The elongations, δl , are the mean in each case of a series of readings. Since one division of the micrometer equals 6×10^{-6} mm., the absolute value of the deflexion is obtained by multiplying the observation in divisions of the micrometer by that number.

In the first place, for the same time of charging the elongation does not increase as the square of the potential. Secondly, the dependence of the deflexion on the length of time employed in charging is so pronounced as to make it imperative to account for it by the heat evolved by the electrical charge. When the condenser was discharged the return of the image, as Cantone also noticed, is quite irregular; at times the image returned quickly to the original zero, very often about half way, but most often there was no recovery. This is contrary to theory, as there should be no residual effect of the charge.

Tube 9.—Adherent Armatures.

Spark-length.	Potential.	Mean elong. micr. div.	Mean elong. $\delta l \times 10^4$.	Time.
5.0 mm.	60 c.g.s.	35.0	2.10	0.25 min.
5.0	60	62.5	3.75	1.00
5.0	60	67.5	4.05	2.50
7.5	85	67.5	4.05	0.25

Greater potentials could not be used as the tube was ruptured with that spark-length. The tube could of course be repaired and used again.

According to the theory, the elongation varies inversely as the square of the thickness, thus for the same potential the ratio of the deflexions of tubes 7 and 9 should have been three to eight. This is not verified, as the deflexions for equal potentials and time of charging are about equal. In fact, the effect I ascribe to heating is so preponderant as to overshadow all others.

Tube 8.—Paraffin, Adherent Armatures.

While experimenting with paraffin tubes the potentials were carried to the breaking point, which occurred at a spark-length of 10 mm., or 110 c.g.s. I observed no changes of length at all. I think that experiments with this substance have little significance as it is so viscous that not much could be relied upon.

Non-Adherent Armatures.

The tinfoil was removed from the tubes and the non-adherent brass-tube armatures were mounted in place. The space between the armatures and the dielectric was filled with the lard-oil. Thus the fall of potential between the armatures was uniform, and there was neither a free charge on the surface of the shellac tube nor any discontinuity in the lines of force. The elongation due to the mechanical pressure of the electrodes was eliminated, and according to the value assumed for Poisson's ratio, a decrease of from one-fourth to one-third of the deflexion for adherent armatures of the same dimensions should result. Again, the distance between the armatures is now increased to 7 millimetres, instead of the former less amount. For number 7 it should reduce the deflexion in the ratio of sixteen to forty-nine; for number 9 of six and a quarter to forty-nine.

The results actually obtained are given in the table.

Tube 7.—Non-adherent Armatures.

Spark-length.	Potential.	Mean elong. micr. div.	Mean elong. $\delta l \times 10^4$.	Time.	Date.
10 mm.	100 c.g.s.	33.5	2.01	0.25 min.	Apr. 13
10	100	64.5	3.87	0.25	14
12	110	58.0	3.48	0.25	13
12	110	80.5	4.83	0.25	14
16	130	61.0	3.66	0.25	13
16	130	98.5	5.91	2.00	13
16	130	132.0	7.92	0.25	14
16	130	170.0	10.20	2.00	14

The table shows that the deflexions, though somewhat less, are not nearly so small as they should be. This would be accounted for satisfactorily if we believe most of this elongation to be caused by the heat evolved. Again, the influence of time of charging is clearly shown. The values on different days seem to be consistently different. I cannot account for this except to suppose that the humidity was so different on the two days as to cause a great variation in leakage currents and so a difference in the heating of the tubes.

Tube 9.—Non-adherent Armatures.

Spark-length.	Potential.	Elongation micr. div.	Elongation $\delta l \times 10^4$.	Time of charging.
10 mm.	100 c.g.s.	80	4.8	1.00 mm.
10	100	55	3.3	0.75
10	100	35	2.1	0.50
10	100	25	1.5	0.25
10	100	75	4.5	1.00
10	100	30	1.8	0.25
10	100	35	2.1	0.25
10	100	42.5	2.6	0.25
12	110	130	7.8	1.00
12	110	115	6.9	1.00
12	110	115	6.9	1.00
12	110	85	5.1	0.50
12	110	77	4.6	0.50
12	110	82.5	5.0	0.50
12	110	70	4.2	0.50
14	120	150	9.0	0.50
14	120	150	9.0	0.50
14	120	165	9.9	0.50
14	120	145	8.7	0.50
14	120	165	9.9	0.50
14	120	160	9.6	0.50
14	120	150	9.0	0.50

The most convincing results were obtained with tube 9, mounted with non-adherent armatures.

The separate readings are here given of a number of observations. With a 10-millimetre spark the elongation for a charging of about one minute is approximately 4.5×10^{-4} mm., and this is reduced to one-third of its value when the tube is charged four times as quickly. This same effect of the time is noticeable throughout the table. Evidently very little of the effect would remain if the charging were made instantaneously. The times of charging are only given approximately, and this accounts for the variations in the readings when the potentials and the times are the same. There is no evidence of the law that the deflexions are proportional to V^2/d^2 .

In concluding this work on electrostriction, which has extended over years, I have some hope that the results given will be sufficiently convincing to prove that the effects which have been observed by myself and others can be fully accounted for by extraneous effects, and that when these are eliminated the distortion of dielectrics by an electrical charge also vanishes.

University of Cincinnati,
June, 1905.

LXXVI. *Plan of an Atom to be capable of Storing an Electrion with Enormous Energy for Radio-activity.* By Lord KELVIN*.

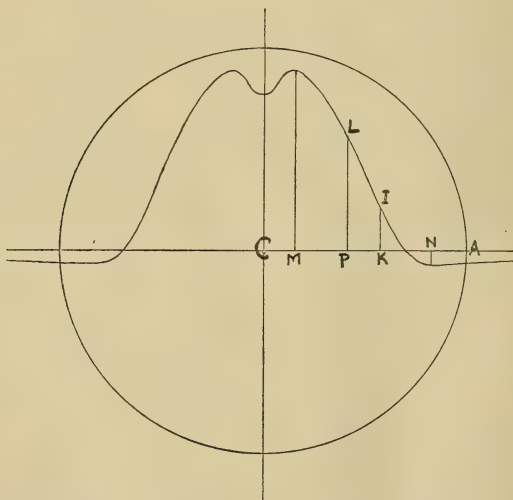
§ 1. **I**N a communication to the Philosophical Magazine of October 1904, I described combinations of atoms and electrions having certain definite qualities of radio-activity; holding myself, for the time, bound to the precise description of the electric property of an atom of ponderable matter, which I had suggested in § 4 of "Aepinus Atomised" (Baltimore Lectures, Appendix E). In that description each atom of ponderable matter is supposed to have ideal electric matter of the vitreous kind distributed uniformly through it. No longer binding myself by this limitation to uniformity of vitreous electric density, I now propose to consider an atom of ponderable matter intrinsically charged with concentric strata of electricity, vitreous and resinous, of equal electric density at equal distances from the centre; and with an excess of the total quantity of the vitreous above the total quantity of the resinous. I still suppose (with, I believe, all at present concerned with radio-activity) that free resinous

* Communicated by the Author.

electricity consists of equal atoms. I assume, and I believe there is general agreement in this assumption, that each of these atoms of resinous electricity, which I am calling electrions, has, besides its ordinarily defined property of electric attraction or repulsion, a property of somehow acting upon ether, and in virtue of this action having quasi-inertia. The nature of this action I believe to be attraction and consequent enormous condensation of ether around the centre of the electrion. (Baltimore Lectures, Lecture XX. §§ 238, 239.)

§ 2. My present assumption is Boscovichianism pure and simple. It merely declares that there is, between a single electrion and a single atom of ponderable matter void of electrions, a definite force in the line of their centres varying according to the distance ; which for distances greater than the radius of the atom is attraction according to the inverse square of distance between the electrion and the centre of the atom. It leaves us absolutely free to assume any law of force whatever that suits our purpose, when the electrion is within the atom. To give capacity to the atom for storing enormous electric energy by placing a single electrion at its centre, or at a very small distance from its centre, I assume, as indicated in the accompanying diagram, fig. 1, that the force on the

Fig. 1.—WORK-CURVE.



On the right side of the diagram, slope up to right implies attraction ;
slope down implies repulsion.

electrion at distances less than a certain distance CM from the centre is towards the centre ; and that at all distances

between CM, and CN (the latter being slightly less than the radius of the atom), there is repulsion from the centre, rising to a maximum of enormously great amount at some distance CK between CM and CN, and coming to zero at the distance CN. Between CN, and CA the radius of the atom, the force becomes again attractive, and continues so, varying inversely as the square of the distance for all the distances of the electrion from the centre of the atom, greater than CA. The curve shown in the diagram may for brevity be called the *work-curve*. It shows by the ordinate PL the work, positive or negative, required to move an electrion from an infinite distance to any point P within or without the atom; which I denote by w . Thus if we denote CP by r , we have

$$F = \frac{dw}{dr} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1),$$

where F denotes the force (positive when attractive, negative when repulsive) of the atom on an electrion in the position P. Hence this force is indicated in the diagram by the tangent of the inclination of the curve at any point L, to the line of centres, CA.

§ 3. For all points outside the atom we have

$$F = \frac{\alpha e}{r^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2),$$

where α denotes the excess of the vitreous over the resinous electricity permanently belonging to the atom; and e denotes, according to the general usage of scientific writers on radio-activity, the quantity of resinous electricity in any one electrion. Thus for the equation of the curve outside the atom, we have

$$w = - \frac{\alpha e}{r} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3).$$

By this we see that our curve outside the atom consists of portions of two rectangular hyperbolas.

The maximum ordinate through M, and the minimum ordinates through C and N, show that the point M is a position of unstable equilibrium, and that the points C and N are positions of stable equilibrium; for a single electrion placed within the atom. The point I on the curve, being a point of inflexion in the branch sloping downwards to the right, indicates that K is a position in which the atom experiences a maximum of repulsive force. Considering a spherical surface

through any point P within the atom, we see that if Q denote the excess of vitreous over resinous electricity of the portion of the atom within this sphere, we have

$$F = \frac{Qe}{r^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (4),$$

because the resultant force of all the electricity of the atom in the shell of outer and inner radii CA and CP is zero for every point inside its hollow.

§ 4. We may vary the work-curve within the atom as we please, with a view to trying to explain the different radio-activities of different atoms, and the different modes of radio-activity which seem to be presented by one and the same atom at different times. Thus for example we may draw the curve with four or six or eight or any even number of minimums, instead of the two minimums at C and N. There will of course be in every case an odd number of maximums, being less by one than the number of minimums. Thus we may arrange for any even number of stable positions of equilibrium within the atom. The work-minimum for the stable position nearest to the boundary of the atom is essentially negative, and somewhat less (somewhat more negative) than the negative work required to carry an electrion to the surface of the atom from an infinite distance outside. All the other minimums may be as large positive quantities as we please. The magnitude of any one of them is the explosive energy which will be spent in shooting the electrion outwards or inwards by any shock or any kind of influence, if it is displaced away from its position of equilibrium far enough to reach an unstable position on either side of it. Look for example to the diagram. An electrion placed at C has stability, but only through a narrow range. If it is shaken away farther from the centre than M, the electric force of the atom upon it will shoot it out of the atom, with prodigious velocity, which will be but slightly diminished by the attraction of the whole atom when it gets outside. If it gets quite out of the atom, it will be shot through the ether outside with a velocity whose kinetic energy is something greater than the value of w at the unstable position from which it was shot, provided of course we can neglect its loss of energy by motions which, while it is in the atom, it gives to the ether in the atom and outside it.

LXXVII. *Notices respecting New Books.*

A Treatise on the Analytical Dynamics of Particles and Rigid Bodies ; with an Introduction to the Problem of Three Bodies. By E. T. WHITTAKER, M.A. University Press, Cambridge. 1904.

THIS treatise differs in many respects from the usual kind of English text-book on dynamics. With few exceptions these may be described as more or less ingenious collections of modes of solution of abstract problems which may correspond to something natural. The authors of most of such books have had present to their mind the needs of a student whose knowledge of dynamical methods is to be tested sooner or later in an examination hall. One mischievous effect of this system was to bring into special prominence solvable cases without due consideration of why they should be solvable. For why should a candidate waste his precious time over the difficult question of solvability when he was certain to be examined only on cases which could be solved, that is, reduced to elliptic functions? Ever since the publication of Thomson and Tait's 'Natural Philosophy,' there has gradually grown up a healthier public opinion; and there has been a marked advance in the preparation of our best physical treatises. And now Mr. Whittaker has given us a treatise, the aim of which is to lead up to the best recognized methods of analytical dynamics as a means towards a great end, namely, the interpretation of nature. Chapter I., on kinematic preliminaries, gets us almost immediately into touch with the real essence of rotation. In the succeeding chapter on the equations of motion, the convenient term holonomic is introduced, Lagrange's equations established, and the Lagrangian Function or Kinetic Potential defined and used. Chapter III., on principles available for integration, discusses ignorable coordinates and the general principle of moment of momentum and energy. Then, and not till then, do we encounter the ordinary well-known simpler problems of particle dynamics. Chapters V. and VI. deal with the dynamics of rigid bodies. The theory of small vibrations about equilibrium configurations form the subject of chapter VII.; and with chapter VIII., on non-holonomic and dissipative systems, the more elementary part of the book may be said to end. It corresponds fairly well with the kind of dynamics which the examination paper can attain to; and yet it is but a preparation for the more abstruse developments which follow. Without particularizing too much, we may say that the powerful methods and developments associated with the names of Gauss, Hamilton, Jacobi, Liouville, Lie, Poincaré, Bruns, and others are brought forward and discussed with admirable skill, the later chapters dealing with the problem of three bodies and the general theory of orbits. Like Lagrange's great classic, this treatise is remarkable in the entire absence of a single diagram.

The Analytical Theory of Light. By JAMES WALKER, M.A.
Cambridge University Press, 1904.

THE aim of this excellent treatise is "to give an account of physical optics without having recourse to any hypothesis respecting the nature of the influence that constitutes light or the character of the medium in which it is propagated." In this respect it resembles Lord Rayleigh's well-known article on Wave Theory in the *Encyclopædia Britannica*, and differs markedly from Poincaré's *Théorie Mathématique de la Lumière*, which builds on elastic foundations, and from Drude's *Optics*, in which the characteristic feature is the electron. It is well perhaps when our imagination is being excited by whirls of charged corpuscles that we should be recalled to what is essentially the original standpoint of Fresnel's theory. This is what Mr. Walker has done, and done effectively and well. Nevertheless, although the notions underlying the electromagnetic theory of light have not been introduced explicitly, the influence of this theory is shown in the particularizing of two related quantities which the author calls the polarization-vector and the light-vector. By use of these related vectors the mathematical investigations are in many cases distinctly simplified. It is obvious, indeed, that the author has taken full advantage of the analytical methods which belong peculiarly to Maxwell's theory without adopting the physical significance of the symbols used. It might seem at first sight difficult to take account of the phenomena of absorption and dispersion, without some distinct understanding as to the nature of the dynamical link connecting æther and matter. But a very general discussion of the facts of absorption and dispersion suggests to the mathematician familiar with workable functions that we have simply to introduce an exponentially diminishing amplitude and a forced periodic vibration; and, behold, the deed is done. Similarly, when Mr. Walker comes to treat of magnetically active media, he obtains a sufficient basis for the mathematical representation by introducing a suitable vector perpendicular to the magnetic force and the polarization-vector. In a sense there may be no recourse to any ultimate hypothesis; but the reader can hardly fail to see the modern electromagnetic theory winking at him through the lines and symbols of the fundamental equations. Nevertheless, the author has to an interesting degree kept wondrously clear of definite theoretical assumptions as to the inner nature of æthereal and molecular processes. The treatment is instructive as showing how far we can go in coordinating phenomena without committing ourselves to a definite physical theory. In regard alike to the experimental illustrations chosen to make the argument intelligible as well as to the analytical methods employed, the book is thoroughly up to date; and all students of optical theory will welcome it as a valuable addition to our scientific literature.

Landolt-Börnstein Physikalisch-Chemische Tabellen. Dritte umgearbeitete und vermehrte Auflage, herausgegeben von Dr. RICHARD BÖRNSTEIN und Dr. WILHELM MEYERHOFFER. Julius Springer, Berlin, 1905: pp. xvi+861.

THE appearance of a new revised and enlarged edition of this standard and deservedly popular work will be cordially welcomed by all workers in physical science. Since the issue, in 1894, of the previous edition of the work, numerous additions have been made to our knowledge of physical constants, and of these the editors of the present edition, assisted by numerous experts and the Prussian Academy of Sciences, have fully availed themselves. The work as it stands at present is a remarkable monument of human industry and of that conscientious attention to detail which has been so largely instrumental in developing modern science.

Among the special features of the new edition, besides the corrections necessitated by more recent researches, may be mentioned the numerous bibliographical references which occur throughout the body of the work, and the new tables relating to absorption, emission, and reflexion; those relating to thermo-electric constants, sparking potentials in gases, and magnetic properties of materials used in dynamo construction; and lastly, the very large and important additions to the section which deals with thermo-chemistry and physical chemistry generally.

Researches on the Affinities of the Elements and on the Causes of the Chemical Similarity or Dissimilarity of Elements and Compounds. By GEOFFREY MARTIN, B.Sc. London: J. & A. Churchill. 1905. Pp. xii+287.

THIS original and interesting work should strongly appeal to all interested in the wider generalisations of modern chemistry. "The chemical properties of an element depend entirely upon its chemical affinities; and the similarity or dissimilarity of two atoms or radicals depends on the proportionality or otherwise of the affinities of the one to those of the other,"—may be regarded as the text from which the author preaches his sermon, and the text is supported by an immense mass of facts culled from the field of experimental research. Were the affinities of each element in each state of valency known, then it would be possible to construct for each its "affinity surface," and chemistry would then take rank as a mathematical science. The "affinity surface" is constructed by the author as follows. In the x - y plane the various elements, arranged according to the periodic law, are represented by points, the x -coordinate corresponding to the group number of the element, and the y -coordinate to its series number. The affinity, for any element, of the element whose affinity surface is required is then represented by the z -coordinate, and the surface passing through the extremities of the z -coordinates is the affinity surface. Owing to the absence of sufficient data, it is at present impossible to construct accurately the affinity surfaces. The

general shape of the surfaces may, however, be in many cases indicated, and this is done by the author in a large folding-plate for a number of the more important elements. An examination of these surfaces leads to the "wave law" of affinity, the affinity surfaces presenting the successive appearances of an advancing wave which repeats itself at the end of every fresh cycle of elements of the periodic system.

Numerous questions of outstanding theoretical importance are discussed by the author, and his daring and original interpretation of the significance of alcohol drinking, contained in one of the appendices, will be read with interest even by those who may not be prepared to agree with him.

We wish that the literary merit of the book were equal to its scientific value. Unfortunately, we cannot say that such is the case. The list of *errata* given at the end of the book is but a drop in the ocean of errors of typography and grammar in which the book abounds throughout. We cannot believe that the proof-sheets were honestly read by any person whose powers of observation are not utterly atrophied. The reader is kept in a constant state of irritation by the altogether inexcusable and frequent violations of the commonest rules of grammar and spelling which assail his eye on almost every page; and it is little to the credit of the publishers that they should have allowed the book to appear in its present state.

Neuere Anschauungen auf dem Gebiete der Anorganischen Chemie.

Von Prof. Dr. A. WERNER. Braunschweig: F. Vieweg und Sohn. 1905. Pp. xii+189.

THIS interesting and stimulating work forms the eighth volume of the series of monographs now being issued by Messrs. Vieweg & Son under the general title of "*Die Wissenschaft*," and deals with modern views regarding the periodic classification of the elements, the theory of valency, and the constitution of the more complicated inorganic compounds. The existence of these latter has been very largely ignored by many writers, and the importance of questions of constitution has very generally been supposed to be confined to the domain of organic chemistry. The narrowness and inadequacy of this view are clearly brought out by the author. The gradual abandonment of the old notions regarding valency, and the generalisations of this idea so as to meet the demands of growing knowledge, form highly instructive and suggestive reading. The author is severely critical and extremely cautious in his exposition, going, indeed, so far as to suggest that the evolution of helium from salts of radium may possibly be capable of some interpretation other than that which attributes it to the breaking up of the radium atom.

Index of Spectra. Appendix P. By W. MARSHALL WATTS, D.Sc
Manchester: Abel Heywood & Son. 1905. Pp. 103.

THIS appendix to the author's *Index of Spectra* contains tables of the spectra of ruthenium and yttrium, and of the line and band spectra of sulphur.

Verflüssigtes Ammoniak als Lösungsmittel. Von J. BRONN. Berlin: Julius Springer. 1905. Pp. xi + 252.

THE author of this book is to be congratulated on having written a very useful monograph dealing with the properties and methods of handling liquid ammonia, and with its applications as a solvent. Hitherto no sufficiently detailed information on these subjects was available without a very laborious search through the chemical literature of the past forty years; and we can cordially recommend the book to all chemists who are either interested in the subject in a general way, or who may be engaged on experiments in connexion with which the solvent properties of liquid ammonia would prove useful. A special chapter is devoted to the uses of liquid ammonia in physico-chemical researches.

LXXVIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 616.]

June 7th, 1905.—J. E. Marr, Sc.D., F.R.S., President,
in the Chair.

THE following communications were read:—

1. 'The Microscopic Structure of Minerals forming Serpentine, and their Relation to its History.' By Prof. T. G. Bonney, Sc.D., LL.D., F.R.S., V.P.G.S., and Miss C. A. Raisin, D.Sc.

The authors, after a brief reference to investigations of serpentine during the last thirty years, which still leave some points unsettled, describe the formation of that mineral from sundry ferromagnesian silicates. Having given a summary of the changes in olivine, they describe more fully the alteration of separate grains in the so-called 'kimberlite' of South Africa. Then, after referring to the serpentinization of amphibole, as illustrated in the well-known Rauchenthal rock, they enter more fully into the changes, first, of the orthorhombic pyroxenes; then of the monoclinic. To illustrate the latter, they describe the conversion of malacolite into serpentine in the well-known 'Eozoon'-rock of Côte St. Pierre, and of some augite-bearing serpentines in the Vosges. An investigation follows of the form of serpentine called 'antigorite,' described by Dr. Hussak (in 1883) from Sprechenstein on the Brenner Pass, and asserted to exhibit a 'netting-like' (gestrickte) structure, which is a record of the nearly-rectangular cleavage of the original augite. They show, by study of specimens collected in the Sprechenstein district by Miss Raisin, that no such connection exists and that the netting-like structure has often only a subjective existence.

The mica-like mineral called antigorite is shown to be abundant in the Pennine Alps about the head of the Vispthal, and to lead to the same conclusion, suggesting, no less than the Sprechenstein specimens, a relation between that mineral and pressure. After a brief notice of the chemical changes in the conversion of the various

anhydrous silicates into serpentine, the authors embody their investigations in the following conclusions:—

(1) That both a tint and pleochroism are accidental rather than essential characteristics of antigorite.

(2) Neither are low polarization-tints characteristic, unless two mica-like minerals exist, otherwise indistinguishable.

(3) That it is doubtful whether any hard-and-fast line can be drawn between antigorite and the more fibrous forms in ordinary serpentine-rocks.

(4) That the most typical antigorite appears when the rock has been considerably affected by pressure, but it becomes less so when the latter has been very great.

(5) That so far from the nearly-rectangular cleavage of augite originating the ‘*gestrickte struktur*,’ it is worse preserved than any other original one in the process of serpentinization. Typical antigorite, however, apparently is rather more readily produced from augite than from the other ferromagnesian silicates, but is more directly a consequence of pressure than of chemical composition.

2. ‘The Tarns of the Canton Ticino.’ By Prof. E. J. Garwood, M.A., Sec.G.S.

The lakes dealt with comprise the larger Alpine tarns which occur in the Canton Ticino. Most of these drain into the Ticino basin; one or two, however, flow into the Reuss or the Rhine.

Val-Piora Lakes.—These are chiefly rock-basins which lie in all cases along the junction of two dissimilar rock-masses. Detailed soundings prove that the axes of greatest depth of the lakes coincide with these lines of junction: one side being a dip-slope, the other an escarpment. It is shown that in the case of three of the larger lakes, soluble calcareous beds have been brought by thrusts against crystalline rocks.

The origin of the lakes cannot be attributed to ice-excavation, as the ice must not only have invaded the district from outside, but must also have come from several different directions at once. Other difficulties in the way of accepting this mode of origin are also pointed out. They appear to be due to structural peculiarities of the district, aided by solution. Analyses of the *rauchwacké* are given, and its characters are described.

The lakes appear to be connected with a reversal of the drainage of the Piora Valley, consequent on the over-deepening of the Val Levantina in Interglacial times and the elevation of the upper end of the district as the ice-cap melted away.

Two of the other lakes of this group lie on or near the watershed: their origin is difficult to account for, except by differential weathering along the lines of junction.

Lago-Tremorgio Group.—These tarns are all situated, either on the calc-mica-schists, or on outcrops of crystalline limestone and dolomite. Lago Tremorgio itself is almost certainly due to solution. A typical analysis of the schists gives 75 per cent. of calcium-carbonate. This occurs chiefly in the form of eyes of granular calcite which crumble between the fingers, and is found to be dissolved out in the submerged rock, while sharp reefs run out under water.

Soundings show a depth of 150 feet near the exit, which is a narrow water-cut gorge. The lake is otherwise entirely surrounded by a steep wall of rock, the lowest point in which is 718 feet above the lake. It is impossible to suppose that ice has scooped out this basin. The fact that springs issue from the side of the valley below the lake all the year round, and that the level of the lake falls 26 feet below its exit in winter point strongly to solvent action.

Of the remaining lakes scattered along the southern side of the Ticino watershed, the Lago di Naret has a wedge of crystalline limestone running through the centre, while the Lago Sciundrau is situated on the junction of dolomite and gneiss, and the Laghetti to the east of Lago di Naret lie along the junction of the calc-schist and the gneiss.

The St. Gothard Lakes.—The rock-basins here also occur along the line of junction of schists and gneiss. The Lago Lucendro is the most important, and soundings show an axis of greatest depth running along this junction. Solution does not appear to have played much part in its formation. Possibly the glacier which descended from Piz Lucendro has removed fragments along the junction, as a transporting, not an excavating agent, as proved by the soundings at the lower end of the lake.

Lago Sella and Lago Orsino are shallow tarns which come under the same category as Lago Lucendro.

Lago d'Elio, draining into the Lago Maggiore, is due to reversal of drainage by a landslip.

These lakes, then, owe their origin, when they are rock-basins, to the presence of lines of weakness, along which in many cases solution has taken place, while in some shallow tarns ice may have removed detached fragments; but in no case has a lake been found which can reasonably be assigned to ice-excavation independent of rock-structure.

June 21st.—J. E. Marr, Sc.D., F.R.S., President,
in the Chair.

The following communications were read :—

1. 'The Relations of the Eocene and Cretaceous Rocks in the Esna-Aswan Reach of the Nile Valley.' By Hugh John Llewellyn Beadnell, F.G.S., of the Geological Survey of Egypt.

At the meeting of the International Geological Congress held in Paris in 1900, the author brought forward evidence from the Baharia Oasis and Abu Roash to show that there was a marked unconformity between these two systems in the northern part of the country. The Jebel-Awaina succession shows that in the southern part of the country, where the Upper Cretaceous and the Lower Eocene occur in their fullest development, there is no sharp line of demarcation between the Cretaceous and the Tertiary, and no disturbances in the stratigraphical succession. This is confirmed by the succession in the Kharga Oasis, where there is no trace of an unconformity. Dr. J. Ball's conclusions to the contrary were mainly based on the supposed irregular variation of the Esna Shales; but, where this occurs, it is mainly due to the fact that, with a slight increase of carbonate of lime, these

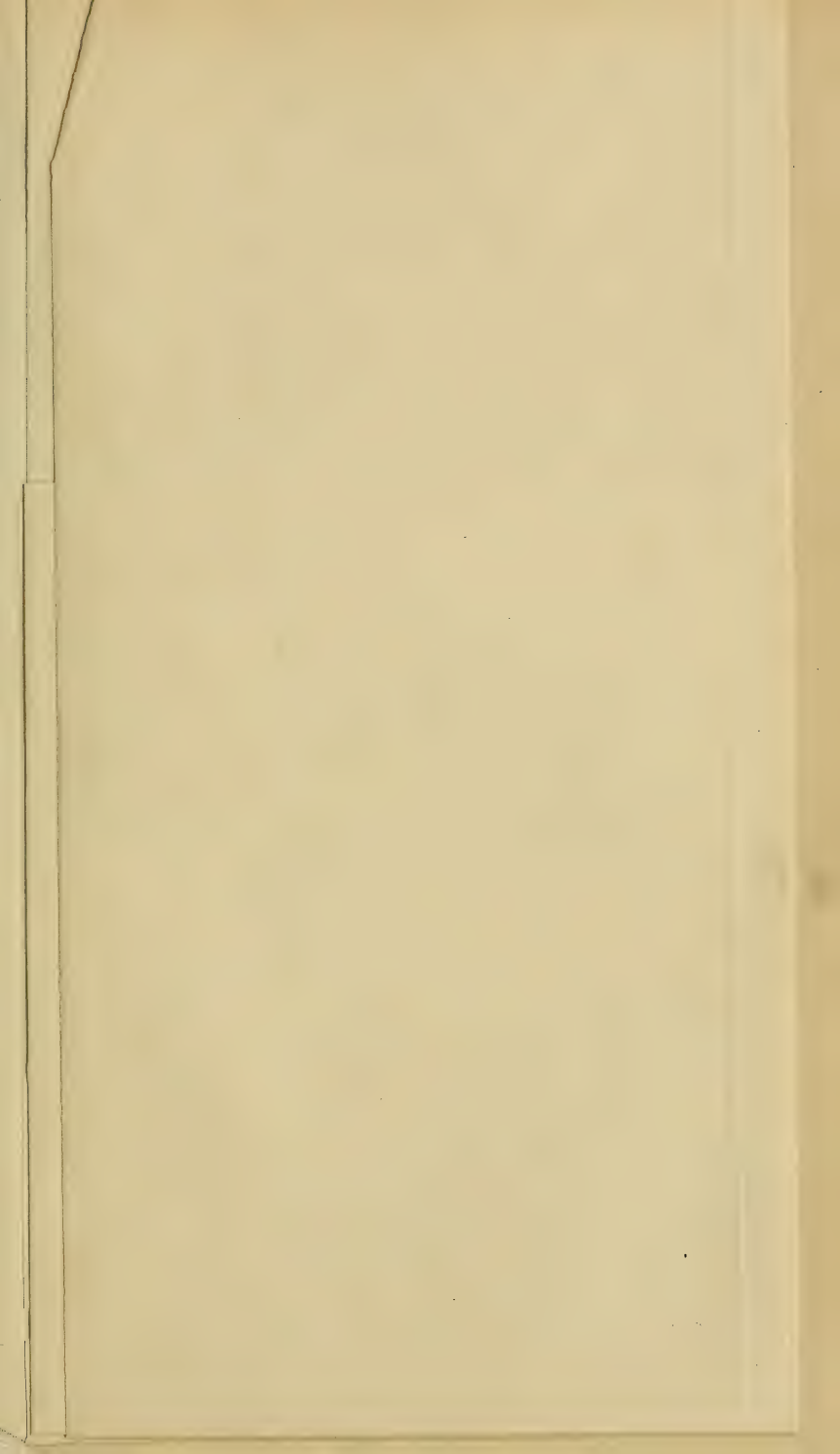
beds became almost indistinguishable from the overlying marls and marly limestones of the Eocene. The author finds in Jebel Nur el Ghenneiem some 180 feet of green clays between the *Echinocorys*-Chalk and the Eocene marls and limestones, and a perfectly conformable succession throughout. Near Ain Amur there is a considerable development of fossiliferous limestones at the summit of the Cretaceous rocks, and many of the fossils are hardly distinguishable from Eocene species. The author is of opinion that the Farafra succession falls into line with that which obtains in the southern part of the country. An important piece of confirmatory evidence is furnished by the discovery of a rich fauna in 'ashen-grey clays' in the Esna-Aswan Reach of the Nile Valley by Dr. W. F. Hume, in the clays above the *Pecten*-Marls in the neighbourhood of Esna.

2. 'A Contribution to the Study of the Glacial (Dwyka) Conglomerate in the Transvaal.' By Edward T. Mellor, B.Sc., F.G.S.

The survey of a district lying east of Pretoria and extending from near the Diamond-Fields to Middelburg, has recently afforded much additional information with regard to the Glacial Conglomerate in this part of South Africa. The district lies on the northern edge of the principal area occupied by the Karroo System, and includes a number of outliers, the area between which affords information as to the source of the material of the Conglomerate and the character of the land-surface on which it was deposited. This surface is smoothed, grooved, and scratched by ice-action. The Karroo System is here only 400 or 500 feet thick, and the Conglomerate usually about 50 feet; but, where deposited in hollows, it may reach 200 feet or more in thickness. The fragments are usually from 1 to 3 feet in diameter, but may attain as much as 8 or 10 feet; they are often faceted and sometimes show striations. The majority of the boulders are of local origin. True bedding-planes are rare in the conglomerate, but there are included patches of sandstone, mudstone, or shale, some of which show ripple- or eddy-markings. The striæ are remarkably constant in direction, and they and the transport of boulders indicate an ice-movement from the north-north-west to the south-south-east. In the Prieska district Rogers and Schwarz found the movement to have been from north-north-east to south-south-west, and the same direction is given by Schenck from near the junction of the Orange and Vaal Rivers. During 1904 outliers of the Conglomerate were found farther north, near the junction of the Elands and Olifants Rivers.

3. 'On New Oolitic Strata in Oxfordshire.' By Edwin A. Walford, F.G.S.

The divisions of the Inferior Oolite of North-West Oxfordshire are described, and a quarry on the border of the county cited where the Cotteswold facies dies out in the '*Purkinsoni*'-stage. A higher division of the same stage (the *Trigonia-signata* Beds) of Northamptonshire type is shown to sweep over the North-Eastern Cotteswold region. The silico-calcareous beds (Chipping-Norton Limestone) cover the countryside which gives them their name, and are about 30 feet thick. Fossiliferous strata, separated from the



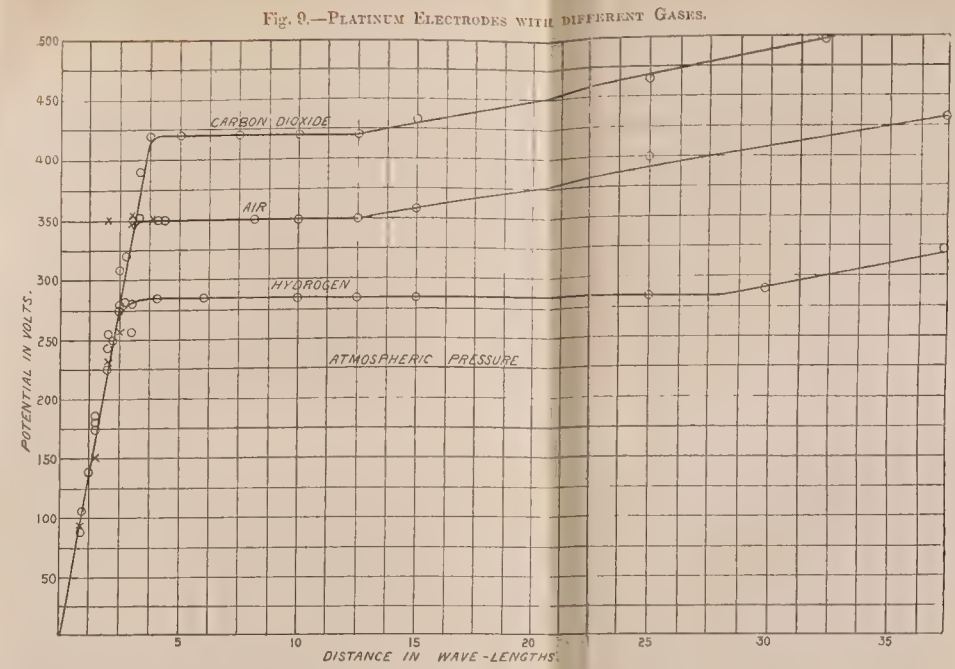
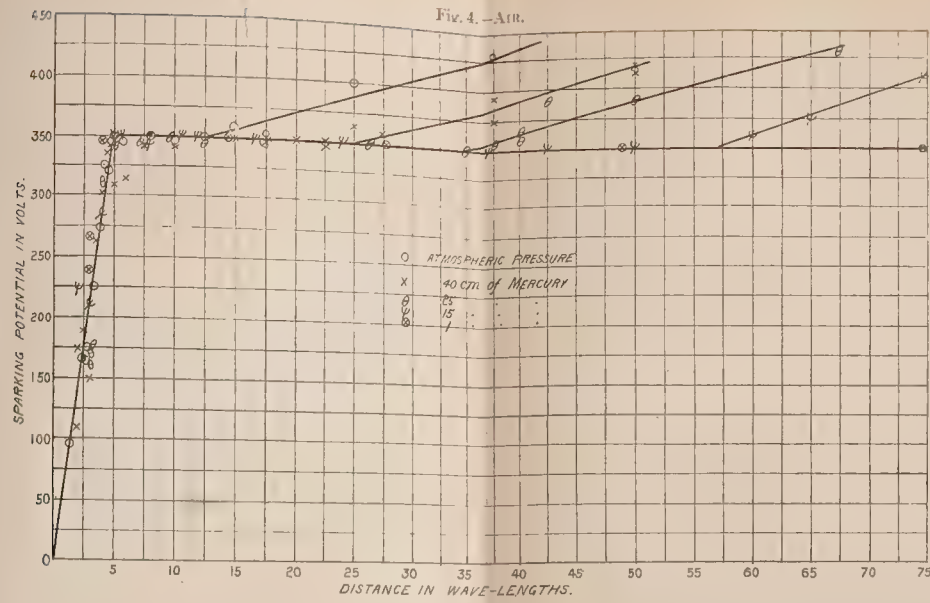


Fig. 5.—ALUMINIUM.

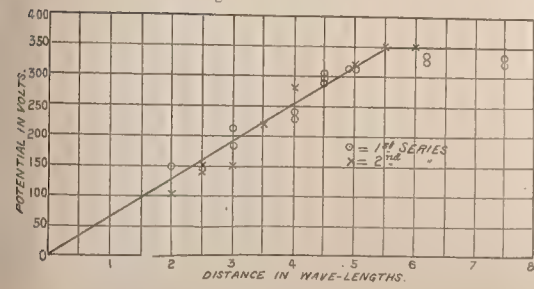


Fig. 6.—SILVER.

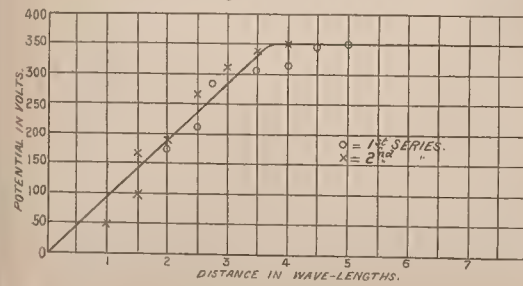


Fig. 12.—NICKEL.

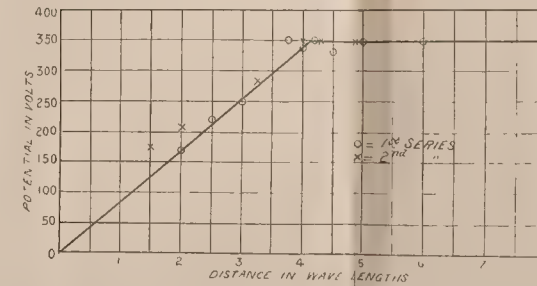


Fig. 7.—BISMUTH.

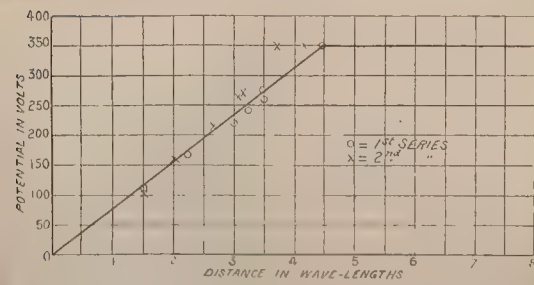


Fig. 8.—ZINC.

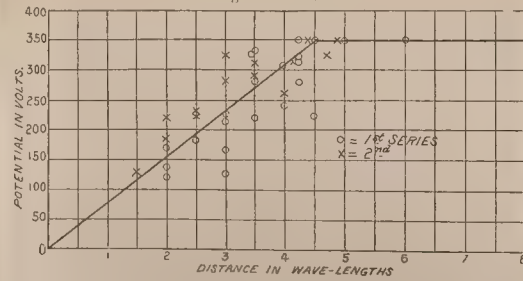


Fig. 10.—ANTIMONY.

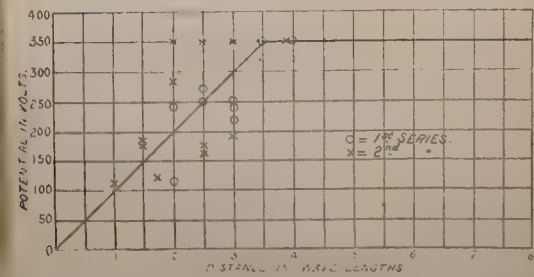


Fig. 11.—MAGNESIUM.

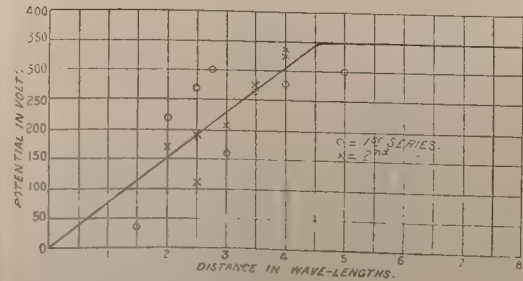
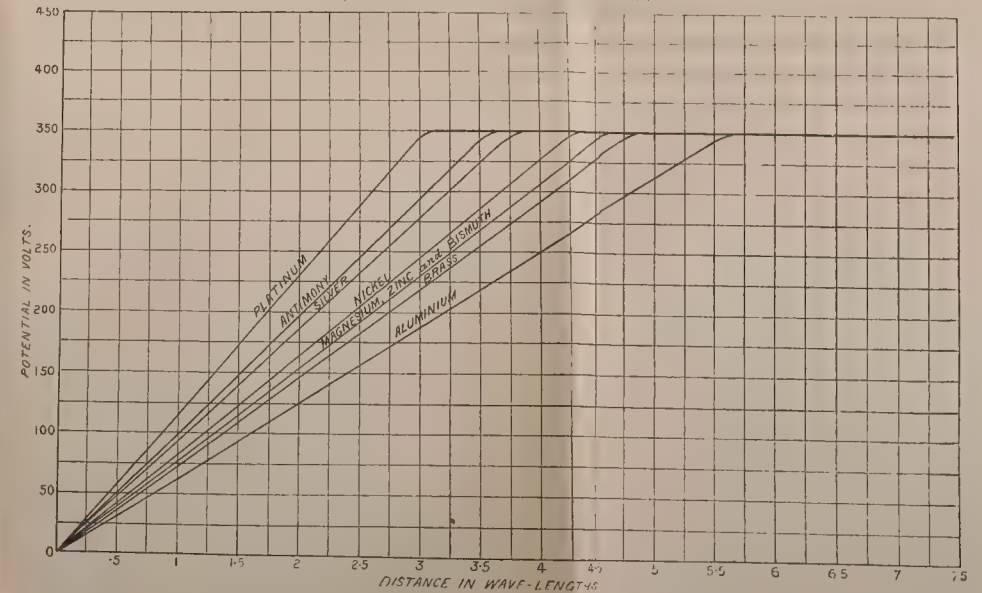


Fig. 13. DIFFERENT METAL ELECTRODES.



Chipping-Norton Limestone by a bed with vertical markings and a black clay-band, indicative of much 'inter-waste' of these and other beds, are described. They are shown to be similar to the Lincolnshire (Ponton) strata described by Morris, Judd, and Woodward. A new term is proposed for these beds, which are characterized by the presence of the shell *Necera*, from the *Perna*-Marls above the black clays to a higher series of black-and-green clays underlying the Stonesfield Slate. These beds and the Chipping-Norton Limestones are classed with the sub-Bathonian. The beds equivalent to those of Oxfordshire have, in Lincolnshire and Northamptonshire, been known in part as Upper Estuarine. In the 20 feet of Oxfordshire strata there appears to be represented the 150 feet of the Lincolnshire Limestone and the Upper Estuarine of the north-eastern counties. The author expresses the hope that his work may help towards the discrimination of the two kinds of deposit known as Lincolnshire Limestones, inasmuch as the fossils characterizing each local (Oxfordshire) stratum have been collected from the beds *in situ*. Lists of fossils are given.

4. 'The Causes of Variegation in Keuper Marl and in other Calcareous Rocks.' By Gerald Tattersall Moody, D.Sc., F.C.S.

Analyses of a large number of specimens of Keuper Marl bring to light the fact that the red portion of a variegated rock contains a considerably-higher percentage of iron and lower percentages of calcium-carbonate and magnesium-carbonate than the green portion. This is in agreement with earlier observations that red strata are usually less calcareous than adjacent green strata; but the author is unable to confirm the hypothesis advanced by Maw, that the lighter-coloured bands in variegated rocks are produced by bleaching or discoloration of red rocks through addition from extraneous sources of lime and magnesia. On the contrary, it is found that the removal of iron existing as ferric oxide from a homogeneous rock-mass is inhibited, if calcium-carbonate, or magnesium-carbonate, or both of these substances be present. When, however, a chalybeate water (ferrous bicarbonate-solution) permeates a rock containing magnesium-carbonate or calcium-carbonate, iron is precipitated as ferrous carbonate, and an equivalent weight of magnesium or calcium passes into solution. Subsequent access of air oxidizes the ferrous carbonate, and red or yellow ferric oxide is produced. The author finds that when a green marl is subjected to the action of chalybeate water in the manner indicated, it is readily converted into a material closely approximating, both in appearance and in composition, to the natural red marl of the same rock-mass.

The author concludes that the variegation of the Keuper Marls and of other calcareous rocks has been brought about by the percolation of chalybeate water through the light-coloured mass, the more porous parts of which have in consequence become stained with ferric oxide, while the harder and more crystalline parts, being non-porous, have remained unchanged. The uniformity in distribution of ferric oxide in some red rocks, such as the New Red Sandstone, suggests that the iron contained in them has probably been derived from chalybeate water in a similar manner.

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